UNITED STATES OF AMERICA

CENTERS FOR DISEASE CONTROL

+ + + + +

NATIONAL INSTITUTE FOR OCCUPATIONAL SAFETY AND HEALTH

+ + + + +

ADVISORY BOARD ON RADIATION AND WORKER HEALTH

+ + + + +

PANTEX WORK GROUP

+ + + + +

MEETING

+ + + + +

TUESDAY
JUNE 18, 2013

+ + + + +

The meeting convened in the Zurich Room of the Cincinnati Airport Marriott, 2395 Progress Drive, Hebron, Kentucky at 9:00 a.m., Bradley Clawson, Chairman, presiding.

PRESENT:
BRADLEY P. CLAWSON, Chairman
JOSIE M. BEACH, Member
JOHN W. POSTON, Sr., Member
PHILLIP SCHOFIELD, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
ZAIDA BURGOS, CDC*
JOE FITZGERALD, SC&A
STU HINNEFELD, NIOSH ORAU
JENNY LIN, HHS
JOYCE LIPSZTEIN, SC&A*
SARAH RAY, Petitioner*
MARK ROLFES, NIOSH ORAU

*Participating via telephone

TABLE OF CONTENTS

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

9:02 a.m.

MR. KATZ: So, good morning, everyone in the room and on the line. This is the Advisory Board on Radiation and Worker Health, Pantex Work Group. Just getting started.

The agenda for the meeting is posted on the NIOSH website under the Board meeting section and today's date. I'm not sure if there are other materials posted there, too. I don't think so.

Let's do roll call. Since we're speaking about a site, please speak to conflict of interest as well.

(Roll call.)

Brad, it's your agenda.

CHAIRMAN CLAWSON: Well, looking at this, it's almost been two years since we've met as a Work Group. Last time we met was May 3rd, 2011. So, I kind of wanted to go over a

little bit of where we have been at.

In the Work Group May 3rd meeting, we asked for a SEC from January 1st, 1958 through December 31st, 1983. This was accepted and was put into place October 20th, 2011.

One of the key issues on that was that we have later years that have been carved out. The 1990 bioassay resulted in 350 Pantex workers with the following: the '89 depleted uranium incident during the disassembly of the W28. NIOSH felt that they, these last few years that they had sufficient data to be able to gain this and they were -- they sought for some access records at Pantex for the 1990 bioassay data, and in this process, they determined that they could not gain access to these records and the result, we got production technicians on the W28, Rev 2 by Beal and LaBone in 2012 stating their stance on how they could do the dose reconstruction.

Since that time, SC&A and myself went to Pantex and we also had a technical call to clarify some of the scientific aspects of what their approach was and we went down to Pantex, and also Stu Hinnefeld accompanied us down there.

The first time, we didn't make it because Sarah had too much snow. But, we finally did make it down there and gained the access to the records.

So, where I'd like to be able to start today because actually this was a response to the LaBone and Beal letter and so, I'd just like to give NIOSH an opportunity to discuss their paper and we could go from there.

MR. HINNEFELD: Okay. Well, I'll kind of set this up a little bit.

Brad's described that we've got this bioassay dataset from 1990 and this was after the suspension of work on the W28

disassembly. The W28 disassembly had been going on for a while, probably throughout the '84 to '89 period, and it was suspended in '89 largely based on employee concerns about the dirty nature of the work and though we had this dataset of a -- the dataset was collected really from anyone who had ever been, I think, associated with W28 and then there were memos that identified people among that group who were production technicians and a smaller subset yet, people among that group who were actively engaged in W28 work, production technicians who were actively engaged in the work and the work stopped.

And we proceeded with that dataset feeling that there was sufficient evidence to conclude that the W28 disassembly was the most high exposure potential work and therefore, around that, we could bound all exposures.

Now, in the interim in some interviews that we've conducted down there,

I've been down there a couple of times in interviews, it probably not quite as definitive in my mind that W28 was, in fact, the dirtiest work. There were other weapon systems that were described as also being dirty and so, while there are some reasons to believe W28 might have been the worst, you know, and it was kind of the largest amount of unalloyed DU, depleted uranium, and one of the guys said, well, he had to beat it up pretty bad to get it apart, you know, so, it was -- it may have been. But, I don't know that there's the same level of confidence that other weapon systems weren't similarly done, but all the focus at Pantex was on W28 because that was where the protests or the complaints about the work arose. So, there's that. There's that argument.

And then once we tried to interpret this bioassay dataset, we were faced with the problem, well, we don't really know these

people's history. How long a duration was there exposure to W28?

And so, the Beal and LaBone paper went through essentially all the potential interpretations of intakes assuming no earlier than '84. You know, because before '84, the Board has already, and the Secretary have already concluded that dose reconstruction isn't feasible for uranium intakes before '84.

So, from '84 through '89 when the work was suspended, the Beal and LaBone paper looked at the potential intake scenarios. You know, one was: what if people were exposed continuously from '84 through the work that was suspended? What if they were exposed for only one year and that year was 1984? What if they were only exposed for one year and that year was like 1988 or '89? And then other increments of exposure.

So, they kind of blanketed the possibilities of what would the total intake

be to a worker given this bioassay data that was collected in 1989, and they concluded that the largest intake would have occurred for a one-year intake starting in 1984 and then the person's not exposed any more until they give that -- and then they give that bioassay sample of 1989. So, they had this long period of time away from exposure. So, the bioassay would be interpreted as a very big exposure back there in 1984.

And so, by running through all these scenarios, they demonstrated that that one-year intake in 1984 would bound, you know, the possible scenarios.

So, I think that probably is bounding for intakes that occurred from '84 through '89, but the other aspect of the argument is: how does that really relate to what really happened to these people? You know, we know that the workers weren't exposed for one year in 1984 and then not exposed any

more.

We know some of them were exposed, you know, either throughout or at various times between then. So, it's not a -- to my mind, it's not a terribly satisfying approach just to say, well, I've got this number that's bounding and so that's good. I don't know that it really relates very well to the actual experience of the workers during their time and so, I wonder about the technical connection there between what was proposed and what's not.

And, as SC&A has pointed out in some of their papers, if the exposures occurred before 1985, for instance, back in the '83 period or '82 or '81, that would also distort your interpretation of that bioassay. Those intakes back there would not be -- you know, we can't reconstruct those anyway. It does also distort the interpretation of the bioassay.

So, there's some issues with that approach that kind of make me not real -- I don't know that it's a really robust approach for that uranium exposure through '89 or perhaps '90.

Now, in 1990, they collected a lot of bioassay data, but a lot of it was the -- this kind of W28 collection, but there were other bioassay samples taken that year, too. That's kind of when they started doing bioassay and then by '91 or -- by '91, then they had their bioassay program, you know, pretty routinely in place for a couple of years. That way they just kind of routinely bioassayed a lot of people.

So, I guess from our standpoint, from NIOSH's standpoint, I think there is a number out there that's -- that would bound the intakes from '94 through or '84 through '89 or maybe into '90, but I don't know that just saying, well, this is a big number and,

therefore, it's good and bounding. As a general rule, we don't normally go that. We normally look for a more technical connection between what we're proposing and reality.

So, I guess that's where we stand on that. So, I'm kind of lukewarm on the approach we're proposing, even though I think it is a number that would bound the intake. So, that's from that standpoint. From the uranium intake.

That's mainly what we've been working on from our side, and I know that thorium's been in the news in the paper we got today. So, I won't get into that just yet. I think maybe we'll get into that discussion later on.

But, that's kind of where NIOSH is on the '84 to '89 or possibly '84 to '90 period, is that -- you know, that we got this number, but we don't know really if it's tied to the reality of work at the site.

You know, I'd just say that -- I mean, there's quite a lot in -- SC&A has talked about the various isotopes of uranium that were detected in that bioassay. To me, I mean, there's a way those isotopes sum up the total uranium activity.

I think that's what LaBone and Beal did, is they added all those uranium isotopes together and treated them probably as U-234 because it has the highest dose conversion factor among U-238, U-234 and U-235. U-238 gives you the highest dose per atom or per activity intake. So, I think what they did was just summed all the isotopes and treated them as U-234.

I mean, there is a question raised by SC&A about whether these were really depleted uranium exposures and since we have isotopic results, in our mind, that's really not a particularly important issue. We got the isotopic results. You can tell them as U-

234 and view the dose that way, which is what we've done in many places when there were a mixture of potential isotope exposure.

MR. FITZGERALD: Okay. I guess, to respond, you know, this issue sort of originated with our concern about using retrospectively the bioassay data. I mean, the only real good bioassay set was from the '89 event which was taken in '90 and the concern was applying it backwards almost 30 years and, you know, just the question about operations over time, the question of whether this effect was representative of the worse kinds of contamination you might see over that time.

The W28 had such a long operational history. You know, this probably would have worked for something that would have been more short term, but this was a long-term issue and I think that's how the Work Group ended up dispositioning the issue. Thought it was too

long and the operations would not have been static enough to assume that it would be representative going back that far.

And I think the Work Group accepted the counterpoint that NIOSH raised. Well, what about the dismantlement period for retirement? Which was the five-year period from '84 to '89. That certainly was a period of time where you did have relatively static type of procedures. You would expect the operation to be pretty normalized and, therefore, it's more possible that you could take that data and find a way to apply it for what would be a much shorter time period really. Five years.

And that's kind of how it was left and I think the approach that's in the paper is certainly a different approach than what was originally proposed for the 30 years and applies a model that I think is certainly a better model in terms of providing bounding

scenarios.

Our concerns really as we noted were twofold and one sort of ties to what Stu was saying in terms of a large number. A large number can, I'll use that as a code word for the kind of modeling where it is an umbrella type of model, can encompass pretty much the exposures that you would anticipate during that period. But, when you go broader, I think you also increase the uncertainties that might be associated.

So, in a slightly different way, what we're saying in our review is that yes. Yes, we can see the model as being valid, but, you know, because it's applied broad enough, this large-number approach, your uncertainty starts growing and that's one thing that we've pointed out that appears to be an order of two magnitude. Uncertainly at least associated with the M Class of uranium from the analysis.

The other issue we raised really

for Work Group discussion. I mean, it was kind of -- it wasn't this question of high-enriched uranium being present. That wasn't discussed in the Beal and LaBone report and our concern was how it would be addressed in this proposed approach and, you know, we were concerned whether the approach, in fact, could do that.

And part of what we wanted to do certainly today is have that discussion -- I think you've touched upon it already -- how, you know, NIOSH would propose addressing HEU.

We weren't even sure -- now, I'll defer to Joyce because she did a lot of the hard crunching, numbers-crunching on the analysis, but we weren't really sure about whether this was, in fact, real. In other words, well above what might be attributed to environmental levels.

And part of what we did down at Savannah River -- down at Pantex in our site

visit was to looking at more monitoring records and just see, you know, if it were a background level present in the workplace, you would probably keep seeing it. I mean, it's not something that you could clean up if it was a residual amount.

So, we looked at some more contemporary bioassay -- bioassays that were down there and I picked a random sample to out -- and sure enough, we were able to see elevated U-235 in the two random samples that we picked out of the bioassay file.

And we did talk to a couple of the health physicists who had been there for decades, 20 or 30 years and say, you know, this is what this seems to be and, you know, these are folks that are familiar with their monitoring and I thought it would be kind of a straightforward thing to, you know, explain why you happen to have enriched uranium contamination even at the smaller levels

present.

But, they could not explain it. They had some theories of why it was there, but certainly it appears that it is real. That, in fact, you do have it there and what we were kind of looking for is how the method would accommodate that and there may be some straightforward ways to do that.

MR. HINNEFELD: Well, yes, to me, I think that my recollection of the Pantex and the interpretation of the current bioassay where they're seeing U-235 was in the cases -- in the recent case, the ones that we looked at, there was also detectable and more U-234 and U-238, and the bioassay manager attributed the U-235 positive to natural uranium in the individual water source. Because, you know, the well, if they were on a well as opposed to commercial water, wells can have varying amounts of natural uranium in it and they attributed -- the bioassay manager attributed

those U-235 results to natural uranium.

And when you get down around detection limits for, you know, 238 and 234, you've got so much variability in the analysis. You've got to be -- you know, if you're close to the detection on these, you got to be a little careful about reading too much into those ratios.

But, regardless of all that, regardless of all that, in my way of thinking, the dosimetric way to handle this, since you have isotopic bioassay results, you're not doing mass analysis where you have to make some judgment about the specific activity, you've got the activity results from these two isotopes. If you sum those isotopes and you do the dose reconstruction as if they were all U-234 because it has the highest alpha -- it's the highest alpha energy of 235 and U-238 and U-234, and based on that, you then know that the exact mixture isn't the problem.

You have bounded the dose and you've bounded it pretty closely because the entire U-235 energy is higher or U-234 energy is higher, but it's not so inordinately higher that you're just throwing a big number out there. That, to me, is a good, valid technique and we do that at a number of places where we have uranium intake of mixed radionuclides and we say we're going use U-234 as the one in the dose reconstruction.

So, to me, the isotopic content of the bioassay and whether there was some -- and as I recall the conversation of the HP, they were quite puzzled by the U-235. They knew of one event where a U-235 component had been damaged and there had been an investigation with that, but they felt like in a routine situation they couldn't envision a U-235 intake and that's why they couldn't interpret. But, a bioassay manager interprets U-235 as natural.

MR. FITZGERALD: Well, two things and again, the notes are in Germantown. So --

MR. HINNEFELD: Yes, along with everything else.

MR. FITZGERALD: Yes. Right. right. One thing the U-235 levels were well above the MDL, minimum detectable. I mean it must have been --

MR. HINNEFELD: In a few, yes. In a few.

MR. FITZGERALD: Yes, and those two sample bioassays are in Germantown.

In terms of the interview, because I again showed them the bioassays and the levels that were in those bioassays and because they were appreciably above the minimum detectable, kind of asked [identifying information redacted] where do you think it came from and at least one of them opined -- and there was no way to, you know, verify this, that perhaps it came from Y-12 because

the deleted uranium, of course, comes from Y-12 and Y-12 handles enriched uranium. They thought now there was a good possibility it was just simply a residual amount that came over on the depleted uranium, but they really did not know. But --

MR. HINNEFELD: They did say that?

MR. FITZGERALD: Yes.

MR. HINNEFELD: She did opine.

MR. FITZGERALD: They did say that and --

MR. HINNEFELD: Okay. He had just come there and he was probably --

MR. FITZGERALD: Right. Right. And that was his guess that maybe it came from that source, but they really didn't know.

But, they did acknowledge. I guess I'll have to disagree with you. They did acknowledge that this wasn't a background level. This was certainly a real level, but they really didn't have an explanation for,

you know, where it might have come from.

You know, clearly, some of this did come from natural sources, but this was enough above the minimum detectable that they felt it probably must have come from Y-12 or some other source.

And there was other folks and we cited those who in the past have cited sources of -- you know, you got 30 or 40 years of handling enriched uranium. Even at component form, it's certainly could very well come up with a residual level in the background.

You wouldn't be looking for it because frankly, you wouldn't expect to see it, but you would have a residual level.

I want to stop there, because again Joyce had spent a great deal of time looking at that particular issue and had concerns about it.

Joyce, I think you've heard --

DR. LIPSZTEIN: Yes.

MR. FITZGERALD: -- the news. A rationale for how they might approach dose reconstructing even with the enriched uranium presence. Do you have any comments?

DR. LIPSZTEIN: Yes, I do. I think the problem is not -- it's knowing what happened between '84 and '89. Because until '83, an SEC was granted based on you don't know what really happened and from '84 to '89, it was assumed that the workers had a more or less uniform exposure because they all worked in the same places, and then there was this accident in '89 with measurements done -- bioassay measurements done that gave what was supposed to be the highest excretion rate that would be extrapolated to '84.

As already was pointed out before, there is a great uncertainty on what really happened and even with assuming that it only occurred in '84, it's about 100 times higher than if the exposure would occur uniformly

between '84 and '89 and that's on NIOSH papers. So, this is a big difference, 100 times difference between exposures in -- uncertainty on the exposures.

The other thing is that if this was the worse accident and the worse exposure, people could not have been exposed to -- would be exposed only to depleted uranium and not have enriched -- not have uranium-234 and uranium-235 and uranium-236 in their urine samples.

So, the uranium-235 result in some of -- results in some of the samples are too high to be attributed only to natural uranium.

The ratio of some workers rose up to 11 between uranium-234 and uranium-238 and all of the -- we only analyzed data that were above the limit of detection.

Also, some of those workers had also higher than detection level uranium-235 and uranium-236. So, it cannot be depleted

uranium. Even if it's mixed with natural uranium, you can't measure uranium-236 in natural uranium and depleted uranium.

So, I think what we wanted to show with this and there were some other exposures to this work that cannot be attributed only to the depleted uranium exposure during the accident. So, if we don't know what these workers' rates, there is a big uncertainly in using those '89 exposures bioassay to really bound the exposures of all the workers.

MR. HINNEFELD: Yes --

DR. LIPSZTEIN: Did I make myself clear or not?

MR. HINNEFELD: Yes, Joyce and what you're saying is sort of consistent with when I started out -- and I didn't really spend a lot of time on this. Part of my lukewarmness towards the approach we've proposed is that the approach essentially is predicated on this W28 being the worse exposure and the

dominating exposure.

And based on interviews we had and as Joyce has described, some interpretations on the bioassay, it's not so clear that among the sample population that the W28 was necessarily the dominating exposure. I think that's kind of what she's saying here.

DR. LIPSZTEIN: Yes. Thank you.

MR. HINNEFELD: There's uncertainties in the exposure and so -- and I don't think I'm going to argue with that.

I think based upon what we've learned in our more recent investigation, I guess there's not really a good -- I don't really have an argument against that.

MR. FITZGERALD: Yes, I might add to that that I, too, thought the W28 was the worst actor on the scene, but that was before I got into the W55 discussion with DU and that seems to be, you know, right up there. So, it's kind of -- you know, there is some

ambiguity about that.

In talking to site experts on W55 relative to thorium, they say yes, I think it was some thorium in there. But, boy, the depleted uranium was the -- by far the worst actor.

So, you know, it is kind of ambiguous about the -- you know, which particular system gave you more DU. It sounds like a lot of them gave you bad DU at that point in time late in the game. So, I don't know.

But, I do think Joyce's point relative to source of enriched uranium and representativeness is an important issue for using that 28 data back because there is some further question about the enriched uranium and where it came from and how to account for it I think is the way we put it in the paper.

And I might add, you know, we did have some discussion earlier about, you know,

possible sources in addition to the interviews down -- I think in our technical call, we kind of opined that maybe it might have come from off-site and we did -- as it says in the paper, we did cross-check the best we could and it wasn't easy.

MR. HINNEFELD: No. Right.

MR. FITZGERALD: In terms of whether some of these W28 workers did sufficient work in other DOE sites that you could account for maybe exposures at these sites, we found no inconsistency to suggest that that couldn't clearly be an explanation for why this was showing up and, of course, so, we were trying to make sure that was real and from our opinion, it is real although it's a residual amount, a small amount.

But, you know, as Joyce points out, it raises the question of, you know, how one can account for that using this dataset. Because again, we're relying on this single

dataset as the basis for the method.

CHAIRMAN CLAWSON: Anything else, Stu or does that pretty well sum it up?

MR. HINNEFELD: I think that's kind of where we're at on the uranium. I think the -- kind of covered the '84 to '89.

MR. FITZGERALD: Yes. Now, the only thing I might add, and I think Stu touched on it this, is that even though we felt bioassays of the '89 incident in '90 -- that were taken in '90 gives a benchmark, it certainly is not the same level of benchmarks you get in '91 and '92. I mean, it wasn't a step function.

Well, you had, you know, more bioassays for more people in '90. You had a lot more bioassays in '91 and '92, but it was a judgment call and, of course, it sort of, you know, falls to Work Group and NIOSH as to whether, you know, -- you had the 305 samples taken covering, I think, 40 or 50 workers.

Did I get them right? Forty-nine in 1990.

Certainly in my view, gets you a pretty good benchmark for 1990, but a lot of this is a judgment call and certainly, you're in the hundreds by '91 and '92 and thereafter.

So, again, there seems to be enough of a benchmark for 1990 for uranium. Where you could certainly have a basis for a coworker model or certainly an approach for 1990.

But, it is not a step function. I just want to make that clear. It's sort of a -- you know, it goes up to '91 and you have certainly more data for '90 and you have no data really for -- you know, before that.

CHAIRMAN CLAWSON: I understand. Well, I guess basically it falls onto the Work Group to be able to come up with this and what I'm proposing is that we have a SEC from January 1st, 1984 to December 31st, 1990 for the uncertainty of depleted uranium.

How does the other Work Group feel?

MEMBER SCHOFIELD: I totally agree that we want those numbers.

MEMBER BEACH: I agree with that as well.

CHAIRMAN CLAWSON: Okay. So, we're going to bring this before the Board at the January meeting.

MR. KATZ: Do you want to check? Dr. Poston, are you on the line? Dr. Poston?

MEMBER POSTON: Yes, that's -- I'm in favor.

MR. KATZ: Okay. Thank you.

CHAIRMAN CLAWSON: Thank you, Dr. Poston. We didn't hear you come on. Sorry about that.

MEMBER POSTON: Well, I didn't announce it. I had simply sent Ted a message because I didn't want to interrupt Joe in the middle of his dissertation.

CHAIRMAN CLAWSON: Okay. I

appreciate that.

All right. One thing that I did miss out on was in the earlier years, we were talking about from '51 to '58, there was a question that I believe, Mark, that you said that they had no radioactive materials showing up at Pantex if I'm --

MR. ROLFES: No, I never said that.

CHAIRMAN CLAWSON: Okay.

MR. ROLFES: There's been radioactive materials on-site since I believe 1954. Some of the uranium components that were coming in for assemblies where showing up in '54. So.

CHAIRMAN CLAWSON: Good. Well, and this is probably a TBD issue.

We're not -- depleted uranium actually started showing up in '51 time period, but this was clean DU and actually, they had the -- I believe it was Mark 6, 7 and 8 that -- or the 6 was the worst one. They

actually showed up as early as '53, but different than what I thought, none of them were disassembled until Gravel Gerties were built, then showed up back to the plant, but nothing was done with them.

Well, that's where I came into the misunderstanding of -- I thought they came in and they were disassembled at that time, and they weren't, because the uranium and the HE were bonded together, but they never disassembled. The basically clean DU was coming in. So, as far as we go for the earlier years, there -- there isn't really going to be any change from what the previous one was.

MR. KATZ: So, Brad, you said until the Gerties were built. When were the Gerties --

CHAIRMAN CLAWSON: Fifty-eight.

MR. KATZ: Fifty-eight. Thank you.

MR. FITZGERALD: Was the first year

for the classes that's already been --

CHAIRMAN CLAWSON: Right.

MR. KATZ: Thank you.

CHAIRMAN CLAWSON: The Class that's already been established.

MR. KATZ: Thanks.

CHAIRMAN CLAWSON: It had been built and that's when they were first used, was in '58. So --

MR. FITZGERALD: Do you want to -- want me to go ahead?

CHAIRMAN CLAWSON: Right.

MR. FITZGERALD: You know, part of what we want to do -- there was some bookend issues, as I call them. The early period and the latter period on uranium and '51 and '57 as Brad was pointing out, there was some ambiguity about, you know, when you might have had DU, what systems, what potential exposure may have existed.

A large part of the site visits was

to nail down some of the ambiguity and try to make sure we had a very clear idea and frankly, our approach was just to identify what systems may have had disassembly issues or contamination issues similar to the 28 and just make sure that we were clear on, you know, what the implications might be for that.

And we identified three systems, 6, 7 and 18, and upon further review, it looked like the 6 was the one where you actually had some accounts where -- and then there were some contradictions in some of these -- you know, you're talking about 40-year-old accounts. But, clearly, there was some contamination issues associated with disassembly.

So, we were looking at two things. You know, what was the earliest disassembly of a Mark 6 and, you know, was there any evidence, you know, whether it's former worker accounts or documentation that would suggest

that the mere handling of these forms might have had some contamination associated with them.

As Brad pointed out, before the Gravel Gerties were built in '58, you did have handling of components. You did have handling of radioactive material, particularly DU forms, and so, that was the question and, of course, the challenge was: this was a long time ago.

So, what we established as I think Brad pointed out was, even though the Mark 6 was on the scene and, you know, certainly doing things, there was no disassembly of the Mark 6 until the Gravel Gerties were in -- in fact, there was no assembly or disassembly of any systems until the Gravel Gertie was in place and that was in '58.

So, unless that was established, we felt -- unless there was some other documentation that suggested some residual

contamination associated with these forms from Y-12, then there really would not have been an exposure pathway and we did not find any documentation. I think the accounts are pretty uniform that the -- that the DU forms from Y-12 were -- were clean. There might have been some minor oxidation, but nothing that would have presented a major pathway in terms of handling.

And so, we concluded that certainly from '51 through '57 we didn't find any evidence of any exposure pathway for depleted uranium such as existed after '57.

And we also looked at -- there was some issues raised early on that we didn't want to lose, hydroshots and the firing pit. So, we went back just to make sure that was part of the analysis and tried to nail that down better and there and again, talking to the HPs down at Pantex and looking at further documentation, we felt that in neither case

the burn pit nor the hydroshots you had any situation where the air sampling data, in terms of the burn pits, wasn't sufficient to provide some information for the dose reconstruction going backwards.

The issue here is that you had some data, but it was just over the cusp into the early '60s. Could you use it for the late '50? And we certainly found documentation that suggested that the factors were the same and that there was enough data to provide a basis for doing that. So, that was the reasonable -- that all bases were covered '51 to '57 as it applies to depleted uranium.

And that was the source term, of course, that we were concerned about. Wasn't any other source terms that we felt figured in that early period. So, we're comfortable with that.

MEMBER BEACH: Joe, I didn't go to Pantex. Did you guys interview people from

that time period? I read through stuff, but I don't remember if you had someone that --

MR. FITZGERALD: We actually had interviews, quite extensive interviews, from that early period.

Unfortunately, with the four or five years intervening, those people were not available anymore. Either they -- you know, just weren't available.

So, but, the information that they provided --

MEMBER BEACH: Was there.

MR. FITZGERALD: -- which is cited and referenced was very relevant to this question both for the handling of the weapon systems as well as the -- as well as the burn pits and hydroshots.

We did talk to one individual who was the sort of site technical expert on issues and really had a good grasp of the history and we talked to him about the Mark 6

and went through and not only him, but also the individual who was tied into the classification system at Pantex at the senior level and had both individuals, these are senior people, actually do extensive searches on the Mark 6 in terms of, you know, the 1950 era.

How it was handled? What was the exact time frame of disassembly? How the handling practices might have figured into that?

And we did come up with one classified piece of information that bared on exposure associated with the Mark 6, but it was an incident. It was sort of a screwy incident and it wasn't really germane to any standard disassembly practice or anything. So, it was isolated.

So, we kind of beat the bushes as much as we could to see whether or not, you know, any of these systems in the '50s would

have figured in any exposure potential for that time period and we did not find any.

And understand, too, that, you know, we made a -- we had some interviews with individuals who've pointed out that depleted uranium oxidizes fairly quickly if you have a raw, you know, unalloyed form. So, certainly, the concern was whether you would not only have oxidation, but begin to have contamination and we're trying to look at that from a standpoint of any evidence, any accounts, any documentation that would show, you know, they were dealing with any contamination at all and we did not find any.

And it looks like the major handling took place in the Gravel Gerties when they started taking these apart. They had been in the stockpile for four or five or six years at least, so.

CHAIRMAN CLAWSON: Well, we did confirm that actually depleted uranium was

there as early as the '82 product. It was there.

MR. FITZGERALD: Yes, I think -- yes, before the Gravel Gerties, Pantex was limited to simply mating high explosives to the depleted uranium. They just didn't do any fissile material handling. So, I think we can make that distinction.

That was a little ambiguous. I think going in and there was still some dates being thrown around. So, I think that was a value of going on-site and really trying to nail this down and classified reviews, what have you, was to really get some more precise information on that and I think we did get -- do that.

CHAIRMAN CLAWSON: Okay. Well, we talked about '51 through '57.

MR. FITZGERALD: And we did talk about '90 and '91.

MR. KATZ: We need a recommendation

on that period.

CHAIRMAN CLAWSON: Oh, for '51 to '57, that we --

MR. KATZ: Because there's still an open question in the SEC.

CHAIRMAN CLAWSON: Oh, I understand. I'm proposing to the Work Group that we accept NIOSH's stand that on the -- that there was no contamination points from that earlier year, that we had confirmed what they suggested.

MR. KATZ: So, your proposal is that reconstruction is feasible for that period.

CHAIRMAN CLAWSON: We accept NIOSH's -- yes. Right.

MR. FITZGERALD: And on the other bookend issue which is 1990/'91, I heard your conclusion on --

CHAIRMAN CLAWSON: Right.

MR. FITZGERALD: -- previous

conclusion. With the 1990 period being a step-up period.

MR. KATZ: That'll be a separate -- separately --

CHAIRMAN CLAWSON: That'll be a separate one. Okay.

MR. KATZ: Yes.

CHAIRMAN CLAWSON: Yes, I recommend to the Work Group that from 1951 to 1957 we accept NIOSH's recommendation that they could do dose reconstruction. Any feedback?

MEMBER SCHOFIELD: I'm in agreement with that.

MEMBER BEACH: I'm in agreement with that, but I also -- how will you do dose reconstruction during that time period? There's some more sampling. Do we --

MR. HINNEFELD: Well, I think the Site Profile has the firing ground, right? The burning ground.

MR. ROLFES: Yes, the burning

grounds and the firing site weren't active until after 1958. So, there really isn't any source of intakes from uranium for that perspective.

MR. FITZGERALD: Yes, just to further answer, we looked at those dates and just wanted to make sure that that aligned with the current SEC. We agree with that.

MEMBER BEACH: Okay.

MR. KATZ: So, Dr. Poston, did you hear the motion from Brad?

MEMBER POSTON: I agree,

MR. KATZ: Okay.

CHAIRMAN CLAWSON: Okay. So, now, let's talk about uranium 1990 to 1991.

MR. HINNEFELD: Well, now, uranium 1990 is part of the Class.

CHAIRMAN CLAWSON: You covered this. Right. So, '91 to --

MR. HINNEFELD: Ninety-one is the end of the --

CHAIRMAN CLAWSON: Ninety-one.

MEMBER BEACH: Ninety to '91.

CHAIRMAN CLAWSON: Yes.

MR. HINNEFELD: Ninety is not even --

MR. KATZ: Just 1991.

MR. HINNEFELD: Ninety-one.

MR. KATZ: You're addressing that.

CHAIRMAN CLAWSON: Okay. Yes, that's right and as we said earlier and we saw it down there that the '91 time period for depleted uranium and stuff. We saw that their bioassay sample process went up and I think that they had enough variety that they can actually do it for that period.

So, I recommend to the Work Group that we accept NIOSH's stand that for 1991 that they could do a dose reconstruction for depleted uranium and uranium.

Any questions?

MEMBER SCHOFIELD: No.

CHAIRMAN CLAWSON: So support? Phil.

MEMBER SCHOFIELD: We'll mark that.

MR. KATZ: Josie?

MEMBER BEACH: Yes. Yes.

MR. KATZ: And Dr. Poston? John? John Poston? Dr. Poston, I think --

MEMBER POSTON: Yes, I agree.

MR. KATZ: Okay. Thank you.

MEMBER POSTON: I'm having trouble with my mute button.

MR. KATZ: Yes, I hear. Thanks.

CHAIRMAN CLAWSON: Okay. That bring us to issue number five. Which is thorium and then, Joe, I'll turn that over to you.

MR. FITZGERALD: Yes, thorium was an issue that I guess the best word is, it got subsumed a little bit because we were in the throes of looking at the W28 uranium issue, and I think the original NIOSH proposal was to

have, you know, thorium handled as a mass ratio of the uranium and obviously, as goes uranium goes thorium.

So, we did touch on some of these issues at the last Work Group meeting, but I think decided as a group that, you know, let's handle the uranium first and then deal with the uranium -- thorium later, and I guess later is now.

So, anyway, because of the SEC that was granted earlier, we're primarily focused on the '84 through '91, which is the end of the petition period.

And what we outlined in our review -- and these issues were raised at the last Work Group meeting. It feels like eons ago. Almost two years ago.

Our concern was just basically with the use of information from the 1990s and trying to have that information be the basis for characterizing your -- or bounding your

dose reconstruction in the '80s. Very familiar issue.

And even though there's a mass ratio -- and this is of air samples. I think I -- in my second revision, I actually had it right the first time. I changed it. It's air samples.

Either this approach or even the previous approach relies on mid-1990s and beyond data primarily. There's a couple of bioassays that happened to have been taken in '83, but the preponderance of the data comes from the '90s.

And our concern is that in '91, it's almost like a split point. You got a split point at the end of '89 for the W28 uranium exposures because that's when they sort of had an epiphany about the DU contamination and did a major overall of practices and came up with contamination control and a lot of bioassay monitoring and

that all happened after the `89 event.

For the W55 which was -- like the 28, served the poster child for the thorium question. That happened in '91 where recognition grew and this was driven --

(Background noise on telephone line.)

MR. KATZ: Sorry, Joe.

MR. FITZGERALD: In any case, go back. The W55 was a pretty bad actor. Comparable -- it's hard to judge whether it was worse or better than the W28. Both of them are pretty bad.

In terms of contamination on disassembly, they rate in 1991 --

(Background noise on telephone line.)

MR. KATZ: But, go ahead and try to talk over her a while. It's quiet right now, Joe.

MR. FITZGERALD: All right. I'll

go for it.

I was just saying though, you know, one, thorium is relatively more hazardous than depleted uranium. I don't think there's any disagreement with that.

Two, it's clear that there was a much smaller amount of thorium contamination than depleted uranium, but nonetheless, it existed, was an issue, something that arose.

On the W55, they did go to a downdrafted glovebox table in late '91. We were able to pin that down a little better, with the notion to control the DU as much as anything else, but certainly to control thorium as well.

The difference between thorium and DU contamination I think was clear. That, you know, with the DU, I think Stu pointed out earlier, you could lift the unit up and the DU would just fall out. I mean, it was sort of gross contamination using gross in the most

graphic way. So, it got everywhere.

Thorium contamination was different. There you actually had to physically manipulate the material, the actual component to have any contamination. It wasn't something that -- I think we used the word spallate. It wasn't something that just sort of fell out. It was something that if you, you know, used a screwdriver as we heard from one of the workers to separate units or to move things around mechanically, you would get some loose contamination.

I think one interview in particular pointed out that in a glovebox environment that in the downdraft table, they were able to qualitatively measure it as something like you'd get a teaspoonful of thorium contamination as compared with a cupful of DU contamination when you took the W55 apart.

Our major concern really is just the fact that I think the mass-ratio approach

is predicated on some representativeness between what was going on in the '90s versus what was going on in the late '80s and we don't think that is true that it, in fact, would be representative.

Because you get this downdraft table actually installed in late '91, and it was such a secure, you know, engineered safeguard that literally, they didn't do bioassays after '91 or '92. Primarily because they didn't see any contamination at all. It's very, very small in terms of the air sampling, what have you, that took place after that table was put in place and I think in our note they ran ten test disassemblies through and just didn't see much. So, they just said well, you know, we don't need to do a routine bioassay.

But, before that, before '91, you essentially had a different situation. We discussed this with one of the individuals

that was prominent in the W55 program, first-hand information and, you know, he confirmed that yes, you know, they had concerns on the W55 that, in fact, thorium contamination existed. It wasn't as bad as the DU, but it was there and that it was part of the motivation to put the downdraft glovebox in place and once that was put in place at the end of this tenure, that pretty much took care of the issue.

But, there was a little bit of a lagged between the W28 practices and procedures that came into place, about a year lag, before the W55 was brought into a similar control state.

So, our position is that, you know, the mass ratio assumptions that are being used in the method that's being proposed for thorium for '84 through '91 I think is undercut by the fact that the conditions before '91 -- whether it's handling practices,

because we did find a lot of practices that were standard in the '80s and before were quickly discontinued once they became more sensitive to the fact that if you manipulate thorium components they will give you loose contamination. So, the idea was to avoid that for one thing.

And the fact that you had an engineer's safeguard to the extreme and I think this is reflective of the concern about the W55 to put a glovebox -- and there's a couple of pictures in there -- to put a glovebox in place like that, you're -- you know, with your manipulation through the gloves and everything, you're dealing with certainly concern over the contamination associated with that system.

So, again, that's our concern.

MR. HINNEFELD: Okay. Well, you know, since we've not talked about thorium for more than two years, we've not really gotten

very far into analyzing this issue.

It occurs to me, though, that the mass ratio that we've proposed is based on smear -- a smear result. Right.

MR. ROLFES: The mass ratio is based upon some BZ samples that were collected I think in the 1990s. Later -- mid to later 1990s.

MR. FITZGERALD: Nineteen ninety-six.

MR. ROLFES: Nineteen ninety-six. Thank you.

MR. FITZGERALD: Yes.

MR. ROLFES: And they analyzed basically the BZ or lapel sampler data. They basically did an isotopic count of the filters and then they used scanning electron microscopy and energy dispersive x-ray analyses to characterize the individual particulate matter on the filters, and I guess they determined that in excess of -- I think

it was in excess of 98 percent of the measured alpha activity on the filter was attributed to depleted uranium and less than 2 percent was attributed to the thorium. Let me think here.

MR. FITZGERALD: And this is outside. Just to clarify, this is outside of the glovebox. The downdrafting.

MR. ROLFES: These are BZ samples.

MR. HINNEFELD: Yes, they'd be outside the glovebox.

MR. ROLFES: Yes.

MR. HINNEFELD: It would be outside the downdrafting.

MR. FITZGERALD: Just to clarify that.

MR. HINNEFELD: Okay. So, then the question, then, would -- to me, this is -- you know, this to me is a kind of interesting ratio case. Because you have uranium present in some amount. You have thorium present in some smaller amount. Would the downdraft

table preferentially capture one over the other? I mean the -- it would seem to me that the capture of the downdraft table is, you know, sort of independent. It's going to capture the loose stuff that's there to some degree.

And it also occurs to me that if we're talking about thorium content on the order of 2 percent of the uranium content, and you look at the uranium exposures that are going -- you know, based on the -- on the uranium bioassay from 1991, I mean, they're probably not going to be that big of bioassays given the bioassay data we have.

You're talking about almost a vanishingly -- aren't we talking about almost a vanishingly small dose here from the thorium? I mean, if it's -- if the thorium is 2 percent -- well, of course, if you do have the BZS --

MR. FITZGERALD: The thorium's -

MR. HINNEFELD: If it's bone surface or something.

MR. FITZGERALD: Right.

MR. HINNEFELD: Certain organs like bone surface will be --

MR. FITZGERALD: Well, just to continue your -- I understand what you're saying. That's kind of what we're after, too. Is that, yes, you have to account for the negative pressure in the downdraft table because that's going to have an effect on the particulates that are going to be available to get out. Then you have filtration and the seals themselves.

Because what you're really seeing outside the downdraft tables is fugitive emissions, which are very small to begin with, because this is a pretty tight thing. That's why they discontinued bioassay in the first place.

So, the question becomes: is the

ratio outside that, you know, contraption going to be different than the ratio inside?

MR. HINNEFELD: Inside the contraption would have been before they installed the downdraft --

MR. FITZGERALD: And our point is that it's doubtful, but there's not -- from our standpoint, it's not clear how you would do that, how you would validate or verify that. You'd almost have to compare it with date from inside the downdraft table or from the 1980s. You'd have to have some way to compare or validate that. Because otherwise, you're kind of theorizing, you know, relative capture of particles through filtration or seals and there's -- you know, you have to almost hypothetically figure out what the negative pressure is that then influenced.

There's enough variables there. I don't know how you would do that.

MR. HINNEFELD: Well, the other

item you mentioned is this was not a -- this was -- as I understand this weapon system, it was not thorium alloyed with depleted uranium. But, there was a depleted uranium piece and a thorium piece.

MR. FITZGERALD: Right.

MR. HINNEFELD: Okay. And so, the relative generation of the contamination of the two would be dependent upon the treatment of those two pieces on that particular sampling. Okay?

MR. FITZGERALD: And, you know, again, the practices differed because they manipulated them a lot more in the '80s before they figured out, you know, you probably should do that with the thorium. Uranium was a --

MR. HINNEFELD: Yes, I do remember that interview.

MR. FITZGERALD: -- uranium was a lost cause because you opened up and --

MR. HINNEFELD: Yes.

MR. FITZGERALD: But, the thorium if you didn't really handle it as much, it would give you as much loose contamination. So, that -- those practices were discontinued as we progressed through the '80s and you recall the interview with the screwdriver. He was told: don't do that again.

MR. HINNEFELD: Yes, I do remember that interview.

CHAIRMAN CLAWSON: Don't do that anymore. Yes.

MR. FITZGERALD: But, you know, they did do those things because they weren't aware of the potential.

Now, presumably in the downdraft table, I guess that's -- I guess one could argue they might have done it just because they knew it was secure, but probably did not. They were probably careful about that. But, you know, my concern is that you end up with a

lot of speculation about how -- you know, if they didn't manipulate the unit as much in the downdraft table, they may not have generated very much thorium, but then we have that contemporary -- not contemporary, but interview where, you know, the teaspoon versus the cup.

MEMBER BEACH: How did they manipulate those before the downdraft? Did they use mechanical devices to pick them up and move them or did they do it by hand?

MR. FITZGERALD: No, they did it by hand. Directly with, you know, different devices. Just a -- they tended to pop things off and it wasn't --

MEMBER BEACH: Yes.

MR. FITZGERALD: -- done very securely and they didn't recognize -- they certainly knew -- to go back, DU wasn't considered that much of a hazard. And they did have to lob it around, but until the W28

incident, they didn't react to it --

MEMBER BEACH: Right.

MR. FITZGERALD: -- in a control way.

Thorium, didn't really see that as an issue at all, but toward the '80s, they recognized that they were getting thorium contamination as we learned from this one individual and it became clear to them that unlike uranium, depleted uranium, it was more due to the manipulation.

MEMBER BEACH: Right. Right.

MR. FITZGERALD: If you mechanically manipulated it, you got contamination. If you kind of left it alone or were careful about it, you got a lot less. So, they got better at handling it, but until the downdraft table came along, you still had that exposure potential and this, you know, again was a lot less material. But, thorium from a relative hazard standpoint was a higher

hazard. So, it's kind of, you know, you had to juggle that.

CHAIRMAN CLAWSON: Well, and something else I'd like to bring up. This is Brad.

If you remember right, 55 was the only one that conducted a downdraft table. It wasn't -- the 53 would still have to be done because of its massive size. They could not build a big enough downdraft table to accomplish that. Plus, they were coming to the end on that one.

And this is what kind of brought some of the workers issues out was you got us now doing it this way, but on this one, we're still doing it the old way and what were the issues on that. That's where some of the mistrust of the workforce was kind of being seen and this was brought to us in the interviews there. You got two different processes.

MR. FITZGERALD: Well, it's just the disparity of, you know, at one point you're handling the stuff directly and the next day, you have it in a sealed glovebox. You know, it was, you know, obvious that some major tightening of practices and what does that mean for the past practices? So, there was that implication.

But, you're right. There were certain systems that just wouldn't fit in a downdraft table. They just couldn't do it that way.

But, the 55, you know, was suitable for a downdraft table.

MR. HINNEFELD: So, I'm trying to decide if there -- would there be some type of evidence that would be sufficient to indicate -- you know, what type of evidence would have to be found that would be sufficient to say that in 1991 the thorium to uranium ratio technique can be used? I mean what kind of

evidence would there have to be. I'm trying to decide what -- if there's anything that could be pursued. Because I don't know.

Presumably, the search has been done for this information. I don't know, though. You know, there was a method arrived at prior to the time when there was a lot of discussion about, you know, downdraft table installations and perhaps differences there and so, if the data search had been done today knowing about the concerns, about the installation of the downdraft table, changing issues, would there have been other things found that would have been relevant to the work in 1991 and I don't know that we've actually search for that yet.

MR. FITZGERALD: Well, I think there's been a pretty -- and this is part of the search that we did at Pantex this last go-round.

MR. HINNEFELD: Yes.

MR. FITZGERALD: Was to hone in on whatever, you know, missing information might be had for thorium exposures and that was an expressed search pattern that we did and, you know, there was certainly a lot of thorium information that Mark and his folks had collected and this last go around see if there was anything that could augment that.

And we did -- I think again the difference now is the acknowledgment of this downdraft table and the fact that the BZ samples were taken outside of it and that I think undercuts or raises some serious questions about the mass ratio approach.

Mark can jump in. I don't think and, you know, given the fact that this issue's been around a while, I don't think there's any thorium additional air samples or specific thorium smears or thorium bio -- you know, there's never been any thorium bioassays that would give you that information other

than that which was taken outside of the downdraft table in the '90s.

There is some -- I think a single identified bioassay in '83 and a handwritten, unidentified bioassay in '83. So, you get two data points in 1983 that were event-driven, but beyond those two items, you do have gross alpha, but that wouldn't give you much of a handle.

MR. HINNEFELD: Right. No.

MR. FITZGERALD: So, I don't think there's anything other than those two data points before 1991.

MR. HINNEFELD: Mark, do you recall some --

MR. ROLFES: Yes, there's the component wipe database that we have there. They're not bioassay data, but they were characterizing the components in the workplace.

In 1976, Los Alamos National

Laboratory or LASL at the time, issued a paper on thorium-232. Basically talking about the health physics and industrial hygiene concerns of thorium-232 exposures.

Their conclusion in the paper was that there was no airborne contamination problem associated with the material due to the large size of the thorium involved.

They had recommended using personal protective equipment to include gloves and lab coats and they did encounter some removable alpha contamination less than 20 dpm per 100 square centimeters, and if they discovered a level of alpha contamination of 40 dpm per 100 square centimeters, they had recommended prompt cleaning.

There's hundreds of wipes that support a judgment that was made that a contamination area wasn't necessary at Pantex and then there were approximately 73 BZ results from the later 1990 period. That also

showed that there was no airborne activity.

This LASL report was LASL 1976, the occupational and radiological health aspects of exposures to uranium -- or, excuse me, to thorium.

Let's see. At Pantex, I believe in the 1990s they had established an administrative control level of roughly 40 dpm for 100 square centimeters, but the individual who was involved in sort of analyzing whether or not there was a potential exposure concern indicated that they didn't think there was a potential to exceed the 10 CFR 835 criteria of 200 dpm per 100 square centimeters.

So, they had a debate on the site, apparently, over whether they should post an airborne radioactivity area for a tenth of the DAC under a requirement of PRCM-235.

MR. HINNEFELD: That's Radiation Control Manual?

MR. ROLFES: Yes. Yes.

They didn't have any area monitoring, but they did try the lapel and BZ samples, which we do have available, and that's what the thorium intakes were based upon or the activity ratios of the thorium uranium.

I'm just looking at some of my notes from the review of these documents regarding thorium. I've got some of the production technicians listed during that time period 1996 here.

Let's see. My notes also indicate that the scanning electron microscopy and energy dispersive x-ray analyses did find some smaller particulates in 2 to 5 micron size particulate. But, it looks like there was an analysis.

Also, you had mentioned the bioassay result from 1983. I remember seeing a fecal and a urine sample which wouldn't be very effective at determining uranium or

thorium intake. Excuse me. Just because of the insoluble nature of the thorium.

But I also do recall seeing some particle size characterization studies for thorium oxide that was conducted in roughly the same time period in 1983. I don't know if we might be able to use, you know, a comparison of the thorium oxide particulate particle size distribution, compare that to the uranium particle size distribution for an analysis. You know, to investigate whether or not that -- the thorium ratio to uranium ratio would be any different. You know, whether uranium would be more likely to escape from a -- like a HEPA filtration system associated with the downdraft table.

MR. HINNEFELD: Well, I think the question can be simplified a little bit and we want to look at the time we're interested in, 1991. Okay. That's the year we're interested in.

And I think of what we discussed the relevant information would be do we have component smear data from 1991.

CHAIRMAN CLAWSON: Stu, the '91 is the time period there because the implementation of the downdraft table came in late 1991. That's where everything changed.

Most of the samples that I saw for thorium and anything else were done after the downdraft table was put in.

MR. HINNEFELD: Well, yes, I mean that's what's been done in the argument so far, is that we got this air sample data from 1996 and these are the ratios there. So, that's what we propose to use.

And the year we're talking about is 1991. So, the question Mark would discuss if we have component smear data from 1991 and from components from the W55, even then it's kind of where does that take us?

We'd almost -- you need the -- you

need an --

MR. FITZGERALD: Well, I don't think there's any data from '91. I think the data starts arising after '91. And mostly, they were trying to verify that the downdraft table was working, and that's when they started taking the samples and --

MR. HINNEFELD: Well, that's why they took the air samples and things.

MR. FITZGERALD: Right, and then they took more later in '96, but the '91 -- after '91, they were validating that this thing was working. They wouldn't have to have a routine bioassay program and, of course, they established that there was very little, if any, in the workplace. So, they stopped the routine bioassay program.

So, really, if you want to just look at '91, '91 represents a point in time where lots of things changed. One, you had the engineered safeguard, but you also had

lots of sampling just to make sure that it was working. Before '91, you didn't have anything that was routine in the way of smearing or bioassay or contamination control. So, there was a certain juncture point there.

And truthfully, a lot was driven by DU because 55 was a bad actor for DU. So, they were trying to figure out how to handle that, as well as this thorium question.

But, I think just to go back to some of the issues Mark was saying and, you know, certainly, they had -- and this doesn't get to the mass ratio. So, but just to kind of cross that T, they had done some hourly particle sizing, just to decide whether they were going to worry about thorium and do some monitoring for thorium and similar to what happened with depleted uranium, because depleted uranium they also sort of came to the conclusion, well, the particle size is pretty large and, you know, don't worry about it.

They came to the same conclusion for thorium.

But, I looked at that and we don't really have good DC sampling which would be the, you know, apples and apples for what was done in '96 in the '80s time frame, late '80s time frame that you could compare and likewise, I think there's some hazards with particle sizing analyses and I went ahead and cited at least one study that was done in the '90s by Coleman. Just to point out that, you know, even when they were doing some extensive particle sizing in the '90s, they were finding out that you had to be really careful about a representative sample and looking at thorium versus uranium, because you have so little thorium that, you know, if you didn't do it right, very easily you could misrepresent what you were looking at.

And in this particular case, they found that the thorium particulate size, the sizing was actually a lot smaller than the

uranium, which wasn't expected.

So, again, I think that's an interesting question, but again, I don't think it answers yours, which is for '91, do you have any data that would give you a starting point for looking at mass ratios that would give you some confidence that the mass ratio was valid for that time frame?

And I think that's what we pointed out in our paper that no, you don't have any data. If you did, you could corroborate even the value that you have from the '96 data, but there's no data that you can compare it against to give you that confidence and that's kind of where we're at.

MR. HINNEFELD: Well, I think we'll want to make a real careful statement about what are the -- what is it that leads us to the conclusion that in 1991 it's not feasible to reconstruct thorium doses for these reasons. I mean, and that's maybe the task at

hand is to come up with that careful statement. You know, being the -- see, because I have to essentially convince my boss and so --

MEMBER BEACH: So, let me make sure I'm clear. We're not talking about just '91. We're talking about pre. So, '84 to --

MR. HINNEFELD: Well, everything up to '91 is already off the board because it relies on a uranium intake. Our thorium approach relies on a uranium intake.

MEMBER BEACH: Okay.

MR. HINNEFELD: And so, the Work Group has recommended that --

MEMBER BEACH: Because we are -- okay.

MR. HINNEFELD: -- through '90 we can't do uranium. So, since we can't do uranium --

MEMBER BEACH: So, we're just looking for '91. Okay.

MR. HINNEFELD: We're looking at the single year 1991.

MEMBER BEACH: Okay. I wanted to make sure I understood what was happening.

MR. HINNEFELD: Yes, and so --

MEMBER BEACH: Thank you.

MR. HINNEFELD: -- if that's the -- so, if that's the Work Group's consensus that in 1991 there is sufficient reason to doubt that the technique we proposed, because I don't see us proposing another one, that the thorium to uranium mass ratio is -- there is not enough evidence to support that and here are the reasons why.

Then that should be a careful statement here and it should be on the record at the Advisory Board meeting, so that that's explained well on the record and then my boss and the Secretary can take that information.

Because our Evaluation Report says we're going to -- we can do dose

reconstructions and so they have to have a rationed argument that says, a well-reasoned argument that says these are the reasons why we are concluding as the Advisory Board, assuming the Advisory Board works real close with the Work Group, that I, the Secretary, am concluding with the Advisory Board that this is not feasible. So, we have to have a well-reasoned statement for 1991 for the thorium ratio not being valid.

MEMBER BEACH: I was just looking at SC&A's conclusion on page 21 of 25 and it's pretty well stated in that paragraph, I would think.

MR. KATZ: Do you want to read that?

MS. LIN: Josie, I think there's a difference between the SC&A's position in the White Paper, which is a draft working paper --

MEMBER BEACH: Right.

MS. LIN: -- versus the position

adopted by the Work Group and the Advisory Board.

MEMBER BEACH: Sure.

MS. LIN: So, if that specific justification is something that the Work Group also concurs, that needs to be filed to the Advisory Board.

MEMBER BEACH: Yes, and I thought that Stu might want to read that since that's what he was looking for as a well-worded statement.

MR. KATZ: Yes, you can read it. I mean, if you have it, you can read it, if you think that's a rationale.

CHAIRMAN CLAWSON: Joe, why don't --

MR. FITZGERALD: Do you want me to read it?

Well, let me just read. There's two places that would be a basis for some wording by the Work Group.

On page 21 of -- I have 26 on the PA version, this is right -- this paragraph right before the conclusion. It says "For the mass ratios being proposed by NIOSH, the differences between the pre-imposed '91 W55 disassembly practices and the advent of glovebox containment negative pressurization and filtration and improved workplace contamination, smearing and air sampling would have likely led to different ratios of thorium oxide to DU found in the 1980s than those found in '96 and beyond. Confirmation of this difference would require analysis of contamination surveys of both uranium and thorium from this earlier period which are not available."

And then in the conclusion itself, the statement reads: "The use of a mass-ratio-based intake value for thorium, i.e., 2 percent, based on air sampling data from 1996 is not valid for the SEC period in question -

- 1984 to, in this case '91 -- because such a parameter would not be necessarily representative of or bounding for that operational time period due to significant differences in worker handling practices, contamination controls, and workplace and personnel monitoring."

MR. KATZ: Can I just, as a layperson listening to this, see if I understand?

I mean, what I heard Joe to say is that since, in the earlier period they would probably be generating more thorium than they were once they got wise to the need to be careful in how they did that disassembly, then when you get these measurements that you have outside the glovebox at that point in the glovebox, they would be more careful, more likely to be more careful with the thorium, they'd be generating less thorium.

So, that ratio you're getting

outside of the glovebox, even if it was disproportional to what's inside the glovebox, wouldn't necessarily reflect a ratio you would have had when they had worse practices earlier on.

Is that correct, Joe?

MR. FITZGERALD: Well, I think it's --

MR. KATZ: More or less.

MR. FITZGERALD: -- another way of saying it. It's just --

MR. KATZ: It's layperson's.

MR. FITZGERALD: If the mass ratio is meant to bound previous exposures, the values in that mass ratio have to be representative of those exposures.

MR. KATZ: Right.

MR. FITZGERALD: And there's no way to demonstrate that, given the data that's available and there's not any real good data available for '91.

That's the issue as I see it. That the data that's being relied upon comes from '96 and that is, after these practices were changed, after the glovebox was put in place and there's no demonstration that those values for uranium and thorium would be -- would represent the same values in the '80s when you had none of that. That it's more than likely that the quantities that were available for exposure were different.

How different? It's not --

MR. HINNEFELD: Okay. So, that I think where I'm coming from on this is that -- I think is that with the installation of the downdraft tables, of necessity the work practices would have to change because they've got different access to different parts because you're essentially out of the glovebox trying to -- and having changed because of that, if there's -- you would have little confidence that the new work practices would

generate the same relative ratios as was done with the old work practice.

CHAIRMAN CLAWSON: Or there was no downdraft.

MR. HINNEFELD: Well, yes. Okay. So, that -- okay. So, that sounds like the argument and then you could even, you know, what we talked about earlier. Since we're not talking about uranium and thorium coexisting in the same piece, but they are different pieces, the behavior on any given unit could alter the ratio of that unit. So, you may not have a consistent ratio anyway. Even without the change of practices necessitated by the downdraft.

MEMBER BEACH: When was 53 finished? Was it before '96?

MR. FITZGERALD: You're talking about 55.

MEMBER BEACH: No, 55 was put into the downdraft. Fifty-three was still on the

outside because it was too big for the downdraft table, but that was because it was coming to the end of its project. So, was that even going in '96?

MR. ROLFES: The 53 didn't contain thorium.

MEMBER BEACH: Oh, I thought it did. Oh, it did not. Okay. Just depleted uranium. Okay.

MS. LIN: So, maybe I missed it. So, how are we only talking about thorium in 1991?

MR. HINNEFELD: Because of the --

MR. KATZ: The SEC. It's covered already.

MR. HINNEFELD: That's -- everything up to '91 is in the SEC. The SEC petition goes through '91. At the end of '91, the downdraft table is installed and so presumably, the conditions observed in '96 would have pertained after the installation of

the downdraft.

MR. FITZGERALD: I think your issue goes to the fact that the thorium value relies upon the uranium value and the Work Group recommended an SEC be considered for '84 to '90 for the uranium which subsumes the thorium. Yes, automatically.

MEMBER BEACH: Wouldn't you still have -- because we're going to take it to the Board through the end of '89. So, you'd still have '90 and '91.

MR. KATZ: The recommendation's through '90.

CHAIRMAN CLAWSON: We've got one year that we're worried about, and all the thorium data is past -- but, their sample is '96 time era where everything is changed and that was -- you know, SC&A's standpoint on it.

MR. HINNEFELD: I think that's something that we could write up.

CHAIRMAN CLAWSON: Well, with that

then, I'd like to bring before the Board -- well, before the Work Group right now that we establish an SEC from -- let's see, it would be January 1st, 1991 to December --

MR. KATZ: December 31st.

CHAIRMAN CLAWSON: -- 31st, 1991. One-year period.

MR. ROLFES: Brad, is this for all employees?

CHAIRMAN CLAWSON: Yes.

MR. ROLFES: This is for all employees?

CHAIRMAN CLAWSON: Yes, until the downdraft -- the downdraft table was put in, everybody was under the same process and then it came in in part of '91. So, I don't think you'd be able to distinguish between it. So, let's say for all employees from January 1st, 1991 to December 31st, 1991.

MR. ROLFES: I did want to -- sorry.

CHAIRMAN CLAWSON: Go ahead.

MR. ROLFES: I did want to point out that there is an Access database that would give you names of the employees that were specifically working on certain aspects of disassembly. That I didn't know if --

CHAIRMAN CLAWSON: You know what? We looked into that in great detail too, and in that time frame, everybody had access to those hallways and as a matter of fact, the assembly/disassembly areas where they were doing it still ran down through all -- the only thing that was protecting them was the screens. Bottom line, there was no buffers in between them.

Remember right off that tour, we had that big fan that was pulling everything out into the hallway and then out.

I don't think you'd be able to distinguish between which people there were, especially for thorium with that process.

So, I'm proposing that it be for all employees at that time.

Any questions on it from other Board Members?

MEMBER SCHOFIELD: No, that seems a reasonable approach to me.

CHAIRMAN CLAWSON: So, you accept that?

MEMBER SCHOFIELD: I accept that.

MEMBER BEACH: I accept that as well.

MR. ROLFES: I had another question --

CHAIRMAN CLAWSON: Okay.

MR. ROLFES: -- that might tip it.

MR. KATZ: Dr. Poston?

MEMBER POSTON: Yes, sir, I accept that, too.

MR. KATZ: Okay.

CHAIRMAN CLAWSON: So, with that, we're basically done.

MR. KATZ: So, do you want to talk about preparing a presentation?

CHAIRMAN CLAWSON: Yes.

MR. KATZ: Do you want SC&A to draft something up?

CHAIRMAN CLAWSON: Actually, yes, they do a fine job on that. I do it the same as we did with Fernald. Joe will bring up a short synopsis of where we are at and then I'll bring my recommendation to the Board after Joe gets done.

Well, go ahead, Joe.

MR. KATZ: Do you need to present or --

MR. HINNEFELD: Like yesterday, I'm --

MR. KATZ: Yes. Okay.

MR. HINNEFELD: -- happy not.

MR. KATZ: I'm just asking the question. That's all.

MR. HINNEFELD: No, I will be

available for comment for the Board Members who want to come. So.

MR. KATZ: Okay.

MR. FITZGERALD: I would just say --

MR. HINNEFELD: That have questions.

MR. FITZGERALD: -- given the careful wording on a couple of these, I guess there will be enough cross-talk to make sure the wording is representative.

MR. KATZ: Please do that. Yes, please do that up front. Okay.

CHAIRMAN CLAWSON: Are there any other things that need to come before the Work Group?

MR. KATZ: Well, so, are there any outstanding TBD issues at this point?

MR. FITZGERALD: There is, but really, you have the presentation. I have the presentation before the Board from last -- and

we're going back historically here. So, some of this is getting old.

But, what I would propose is that I update what was a survey outline of open and closed items and there's some nuances here because there's been some issues just by virtue of this process that we have closed out effectively, and I think for the Board's sake it would be helpful to provide the basis for how that was closed so that you can, you know, report to the full Board that this is how it was dispositioned.

There's some other loose ends, but quite frankly, there aren't that many. There are a few. I think you mentioned data adequacy and completeness. A few T's that need to be crossed, but as far as substantive technical issues, there aren't any major substantive technical issues. Certainly no SEC issues that are pending.

MR. KATZ: Well, there's no SEC,

clearly, because we've --

(Simultaneous speaking.)

MR. KATZ: -- the SEC, but I guess my question is just, it will be good at the Board meeting to present, if there are any TBD issues whatsoever that affect how they plan to do dose reconstructions for those who they can do the incomplete ones in effect, partial dose reconstructions.

If there are any issues remaining that need to be discussed, it would be good to just present that to the Board and they realize what -- if there's something left for this Work Group to do.

MR. FITZGERALD: Yes, since we have a little time, let me just kind of --

MR. HINNEFELD: Can we take a comfort break before we get into that?

MR. KATZ: Yes. Okay. So, just a ten-minute comfort break. It's 10:35. About a quarter to 11:00, we'll just set back up.

(Whereupon, the above-entitled matter went off the record at 10:35 a.m., and resumed at 10:55 a.m.)

MR. KATZ: Okay. We are the Pantex Work Group. We're back from a short break.

Let me just check and see. John Poston, are you on the line?

MEMBER POSTON: I am.

MR. KATZ: Great. We can carry on.

MR. FITZGERALD: Okay. Yes, this is Joe Fitzgerald again.

We're moving to the -- what we call the site matrix listing of issues for Pantex. I'm looking at a May 2011 edition or version. Which is the last version that was developed and we haven't returned to the matrix because we've been focusing on the SEC issues, but just to go through this listing and identify where the status of Site Profile issues and closed issues might be.

I know a lot of you don't have

this. I'm going to try to give you some background as I go.

The first issue on this matrix is adequacy of internal dose records and much of this focuses on thorium, uranium, plutonium bioassay. Looks at the -- and tritium -- and looks at the methods in terms of the completeness of the data feeding those methods.

And the last status which is going back to May of 2011 pertains to the data completeness and adequacy paper that SC&A generated in April of 2011 where we cite some concerns over how the TBDs read. Particularly the internal TBD reads as far as the handling of bioassay information, the completeness and adequacy of that bioassay information.

And based on the discussion and conclusions reached on the SECs that dealt with thorium and uranium, I think a lot of that is certainly the basis for changing the

wording as NIOSH tends to do once an SEC passes. Goes back and changes the language in the TBD as well as supplements the existing evaluation report to illustrate the conclusions reached about the adequacy and completeness of data that supports dose reconstruction. In this case, internal.

So, much of what we have in this first item, the adequacy of internal dose records, I think has been dispositioned by the discussions, extensive discussions that this Work Group has had on uranium and thorium.

We did, I think, reach a satisfactory conclusion that there's no issue with tritium and plutonium. Certainly -- a lot of data revolving around -- you know, Pantex did a lot of bioassays around tritium and plutonium over time. So, to us, that's not an issue and in terms of uranium and thorium, we have dispositioned that.

So, subject to changing the

language in the -- or supplementing the ER and changing the language in the TBDs, I think we've dispatched the question of the adequacy of the internal dose records.

It was listed as open in the presentation before the full Board in August of 2011. So, I think that's something that the Work Group can consider as far as closure.

MEMBER BEACH: Would we put that in abeyance or would that be a closure based on --

MR. KATZ: I mean the abeyance is sort of procedure stuff, but where you've put together -- where you've really put it all to bed because you have an SEC, I think you could just say closed. Because you don't really have to wait and see what technical approach is being preferred. There isn't one.

MEMBER BEACH: True.

MR. FITZGERALD: So, is that closed?

CHAIRMAN CLAWSON: Yes, that's good with me. I guess, you know, I just still want to be able to see what NIOSH -- if they change that at all, you know, I just -- I guess we're going to review -- are you going to change any of the --

MR. HINNEFELD: Well, I mean if the Class is added for what has been discussed, assuming the Board agrees with the Work Group and goes forward, we revise that profile to reflect that decision. So, what we would say is that the approaches we have proposed for uranium from '58 to 1990 and for thorium in '91 are not feasible to be done for all workers.

As far as I know there's not been any particular criticism of individual bioassay numbers. So, in the event that someone has some bioassay in that person's exposure record and they have a non-presumptive cancer, we would use -- we would

interpret it as -- if we can interpret with our existing procedures, we would use that in their -- that's normally what we do.

CHAIRMAN CLAWSON: Okay.

MR. HINNEFELD: So, we would rewrite the Site Profile to say things like that. To explain why we're not doing uranium intakes for everybody, but -- if like a person has got a bioassay in the record we will.

CHAIRMAN CLAWSON: I understand. So that one could be closed. Phil.

MEMBER SCHOFIELD: Yes.

MEMBER BEACH: Yes.

MR. KATZ: So, Dr. Poston, is that good? I think maybe you took yourself -- put yourself on mute. John.

MEMBER POSTON: Okay. Sorry.

MR. KATZ: Yes, there you go.

MEMBER POSTON: I lost count as to whether I was up or down.

MR. KATZ: You need a special light

on your phone, John. Okay.

MR. FITZGERALD: Okay. On the second item, internal dose models for uranium, I mean that's what we closed out today. So, I think --

CHAIRMAN CLAWSON: That's closed. Phil?

MEMBER BEACH: I agree.

MEMBER SCHOFIELD: I agree.

CHAIRMAN CLAWSON: Everybody agree with that?

MR. FITZGERALD: Now, moving briskly along. Number three deals with plutonium which the conclusion we had here -- well, SC&A questions the methodology and we had some questions on that. The issues appear to be moot given the lack of any historic evidence that routine Pu internal exposures occurred due to lack of integrity of components handled.

So, there and again, we had some

issues, but there was no evidence that we could identify where you had, you know, an exposure question with plutonium and the instance that we had identified were covered and we had recommended closure on that back two years ago and so, that's just pending before the Work Group.

CHAIRMAN CLAWSON: I would say that we've closed it.

MEMBER BEACH: I agree.

MEMBER SCHOFIELD: I agree.

CHAIRMAN CLAWSON: Mark, remind me. Because one of the logs that came up was the cracked pit and you guys had already --

MR. ROLFES: Back in 1961, there was an incident where there was a plutonium release into the cell and they basically -- there were three people involved that evacuated and subsequently provided a bioassay for plutonium intakes or to assess the plutonium intakes.

They set up like a radiological

decontamination plan for the time and we have a list of workers who participated in the decontamination of basically the tools, the floor and the unit itself. They all participated in the bioassay and those data are available to us.

CHAIRMAN CLAWSON: Okay. And this would take into the cracked pit one?

MR. ROLFES: That would be 1993. Okay.

CHAIRMAN CLAWSON: You know, I just wanted to make sure.

MR. ROLFES: There were two different incidents where there were plutonium releases that occurred. There was the one in 1961 in the cell and then there was one subsequent to that in 1993 where those people also provided bioassay samples for --

CHAIRMAN CLAWSON: Okay.

MR. ROLFES: -- the incident.

CHAIRMAN CLAWSON: So, I guess my

recommendation to the Work Group is that's closed.

MEMBER SCHOFIELD: Second that.

MEMBER BEACH: I agree with --

MEMBER POSTON: I agree.

CHAIRMAN CLAWSON: Thank you, John.

MR. FITZGERALD: Okay. Item four is dose estimate approach for thorium and clearly, we have dispositioned that today.

CHAIRMAN CLAWSON: So, this outstanding issue of thorium would be closed. Phil?

MEMBER SCHOFIELD: Agree.

CHAIRMAN CLAWSON: Agree, Josie?

MEMBER BEACH: Yes.

CHAIRMAN CLAWSON: Dr. Poston?

MEMBER POSTON: I agree.

CHAIRMAN CLAWSON: Thank you.

MR. FITZGERALD: Okay. Item five is metal tritides and we recommended closure two years ago on that particular issue. I can

give you an update on that.

That was really involved with sealed reservoirs and whether or not you might have some tritides present on the outside due to permeation through the reservoirs as well as the use -- possible exposure from the boom box if you remember some of the explosions and whatnot.

And following a final data capture, that was in June of 2011 and that was the design to look at what actual compounds may have figured and we didn't find any evidence that there was any exposure potential from tritides, but we wanted to cross that T back then.

So, we recommend the issue be closed back in May of 2011.

CHAIRMAN CLAWSON: This is also, too, when they quit reusing the reservoirs due to permeation through at Savannah River.

MR. FITZGERALD: Right. Well, you

know again that was -- we were going back four or five years and there was a question at that time on that issue and I think that's been put to bed. So, there's not a question on tritides with Pantex.

You're right. This was a generic issue that was raised at several sites.

CHAIRMAN CLAWSON: Yes. So, for Pantex, this one would be closed, too. I recommend that the Work Group close the tritides.

MEMBER SCHOFIELD: I agree.

MEMBER BEACH: I agree.

MEMBER POSTON: I agree.

CHAIRMAN CLAWSON: Thank you.

MR. FITZGERALD: Okay. Item six is the interpretation of external dosimetry data and if the Work Group recalls two or three years ago we had spent a fair amount of time before we got into the internal dose issues trying to reconcile some questions that we had

on neutron exposures from weapons systems and some of the questions about how you -- neutron gamma-photon ratios and there was other questions about how those dose estimates were arrived at in the NIOSH scheme and some of these were actually cross issues. We had neutron-photon ratio issues from Mound at the same time we were trying to deal with them at Pantex.

If you recall, this is where we got into some questions about how the data would be analyzed and how the information would be used and what we ended -- I'm just reading from the 2011 -- and this gets into the adjustment factors and this why we felt this was a Site Profile issue. During the May 2nd, 2011 Work Group meeting SC&A noted that we had a number of Site Profile issues and most of these involved what we felt were incorrect adjustment factors of values that would affect the accuracy of dose estimates that would be

used.

And at that time, NIOSH agreed to review the external dose findings from the SC&A Site Profile review in that context. In other words, not SEC, but whether those adjustment factors were appropriate or not and these were also cited in the adequacy and completeness paper that was presented in April 2011.

I think we did not get a --

MR. HINNEFELD: Yes. My recollection or what my understanding is is that I thought we had reached some sort of agreement on the NTA adjustment factors. That that was essentially disposition and the neutron dosimetry at Pantex, they are essentially divided in three blocks to think about.

There was the NTA period. There was the initial, the early TLD period. We'll call it the TLD period and then it was the

late TLD period.

The early TLD period, normally, you think when people start doing dosimetry with TLDs, they're pretty good for neutrons. But, after they had been using their neutron TLD or neutrons for a while, the DOELAP testing came up and they performed badly on the DOELAP testing and so, that called into question that early TLD period which runs up through about I want to say '92. Something like that.

And so, what we've done is that the site -- you know, what it was was an algorithm problem. An algorithm is the set of calculations. It describes how they calculate the output of each of these films -- each TLD chip and what calculations you do in order to arrive at a neutron dose and a gamma dose and whatever doses you're measuring.

And so, they -- after they had the poor performance on their TLDs, Pantex rewrote the algorithm and did a corrected or a revised

or corrected algorithm for that early period and recalculated people's doses with the revised algorithm and the revised algorithm, using that, then they're data didn't pass DOELAP.

Shortly after that, they got a new TLD badge and they've been -- and it passed right away and so, the late TLD period, we're not concerned about.

The early TLD period, we have the -- here's what we have. We have the TLD adjusted data and the unadjusted, the old algorithm data for the early TLD period. We've got both sets of data and we have -- but, we have it unidentified. We don't have the person's name with it.

So, based -- so, what we've done is we have taken the adjusted data, you know, the corrected algorithm data and generated neutron to photon ratios using that and then we'll apply that to the photon, because they always

passed photons.

Apply that to the photon dose for the early TLD period to arrive at the neutron dose.

So, that's what we're proposing to do. We just -- that work was just recently completed, but that is what we expect to do.

CHAIRMAN CLAWSON: So, SC&A has not been able to see that yet?

MR. HINNEFELD: No. No. I mean this would be one we'd want to go through and sort out.

CHAIRMAN CLAWSON: Sure.

MR. HINNEFELD: There is -- we have -- it might be possible if we can get DOE to convince Pantex that we have a right to get it. Because this is data for all places. This isn't just claims data.

We might be able to get it identified since they've recalculated all these with the new algorithm. We might be

able to get it identified. Because when I first saw this, I said why are we doing this? Why don't we just use the adjusted numbers and I found out that we got it de-identified.

So, if we get it, you know, identified meaning we know who, you know, Joe Smith and this is his revised, we would just use that because that revised algorithm did pass the DOELAP testing that the earlier algorithm failed.

CHAIRMAN CLAWSON: So, basically, that one's still open?

MR. HINNEFELD: I think it's open, but that seems to me as really doable and it falls almost entirely within the Class.

CHAIRMAN CLAWSON: Yes. Well, I'm going to be honest because this brings up one of the petitioner's issues and so forth that came in. Was when all of their -- you know, they got all these different notices and then a few years later, all of a sudden everything

got changed.

MR. HINNEFELD: Yes.

CHAIRMAN CLAWSON: This is --

MR. HINNEFELD: That's why.

CHAIRMAN CLAWSON: Well, and this is going to bring that to bed, too. So, I'd say that's still an open issue that --

MEMBER BEACH: Yes, I just looked up -- the last response we had was from March 2011 which was NIOSH's answers to the Site Profile stuff. So, it's 54 pages and it goes right through each one of these and you're saying there's a later one so there's a change.

MR. HINNEFELD: We just got the most recent one. I think -- my understanding and I wasn't real engaged two years ago with Pantex. But, my understanding was that we felt like we had a good correction factor for the NTA film period and that the early TLD period still was an open question because that

TLD had failed DOELAP testing for neutron exposure.

And so, that's what we've worked on recently. Is to determine what can we do and like I said, we got this de-identified dataset. If it's -- I hate to ask Pantex for stuff because it takes so long to get it. You know what I'm saying?

CHAIRMAN CLAWSON: Yes. Yes.

MR. HINNEFELD: And if this is acceptable, I would rather go with what we have.

CHAIRMAN CLAWSON: Yes.

MR. HINNEFELD: So, that's what we're going go with.

CHAIRMAN CLAWSON: Well, I guess then what my suggestion would be is for SC&A to review this and --

MR. HINNEFELD: Yes, they don't have it yet, but we --

MR. FITZGERALD: We don't have it

yet.

MR. HINNEFELD: -- we'll get it to them.

MR. FITZGERALD: And what we did receive, Josie was taking about in March, we were favorably disposed and at that point felt that it was responsive to some of the adjustment factor issues and if you look through the matrix there's a number of these sort of cats-and-dogs adjustment factor issues that weren't SEC issues, but questions of whether they were sufficiently accurate or adequate or that kind of thing.

And I think because they weren't SEC, we didn't spend a lot of time trying to resolve them all.

CHAIRMAN CLAWSON: Right.

MR. FITZGERALD: But, I think there's a lot on the table and with the addition of this newer piece, I think we can close this out, but it's probably not closed

now.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: Okay. So, that's an action item for SC&A just to review the new material when you get it.

MR. HINNEFELD: Yes. The first action for us is to get it to them.

MR. KATZ: Right. No, I --

MR. FITZGERALD: And with the March 2011 response which we did look at, but did not formally respond to, that will give us enough to recommend to the Work Group where that stands.

MR. KATZ: And this is item seven. Right?

MR. FITZGERALD: That was item six. Item six.

Item seven actually we did manage to reach closure on and it took a while. This is the neutron-photon ratio issue as a basis and we actually benefited from the Mound

discussion efficiency.

This is where the MCNP approach was ultimately proposed as a better way to go and that was applied at Mound and subsequently applied at Pantex and if you look at the matrix, we are waiting for a disposition on the MCNP, but we did reach closure on MCNP. So, as a reasonable approach to apply to Pantex as well.

As opposed to -- we had problems with the neutron-photon ratio.

CHAIRMAN CLAWSON: Right. Which is basically still open there, but --

MR. HINNEFELD: Well, I think the MCNP approach was for the NTA film. I think that's how we arrived at the --

MR. FITZGERALD: Right. But, the -- we had a generic issue with the --

MR. HINNEFELD: Yes.

MR. FITZGERALD: -- ratios and the NTA film and how that was applied and I think

the overall strategy of going to MCNP is a better way to go. Was the resolution at Mound. I'm just saying that that carried over to Pantex and I think we agreed that all-in-all that was satisfactory.

So, the neutron-photon ratio has been closed, but we haven't officially -- the Work Group hasn't officially closed it.

CHAIRMAN CLAWSON: And this isn't -- I guess I'm kind of getting confused and so, this isn't tied to the earlier neutron?

MR. HINNEFELD: No, this -- remember I said there were three periods.

CHAIRMAN CLAWSON: Right.

MR. HINNEFELD: There's the NTA film period, the early TLD and the late TLD. The first thing I talked about was the early TLD period. The MCNP pertains to the NTA.

CHAIRMAN CLAWSON: Oh. Okay. I understand that.

MR. FITZGERALD: Let me just go

ahead and read the -- we haven't talked about this in a while and just sort of reach a bottom line.

I'm going to start from the beginning. "The current Site Profile for Pantex recommends you use the following method to assign neutron doses prior to '94. For unmonitored workers who may have had the potential to be exposed to neutrons, multiply the claimant's photon dose by 0.8 to the 50th percentile neutron-photon ratio value to assign neutron dose. For monitored workers, if monitored for neutrons or had the potential for neutrons, multiply the claimant's photon dose by 1.7 which is the 95th percentile neutron-photon ratio value to assigned neutron dose. For '94 forward, third period, use the recorded neutron dose with the appropriate ICRP-60 adjustments.

"In response to concerns that while the recommended neutron to photon ratio method

may bound some of the Pantex workers neutron doses, it cannot be assumed that it will bound all worker neutron doses for '51 to '93.

"NIOSH proposed a new approach that mirrors a similar one proposed by NIOSH in the course of the Mound Work Group SEC review proceedings. This approach applies measured doses in place of neutron-photon ratios with corresponding correction factors for NTA film and MCNP modeling for missed doses of certain energies for the coworker model."

Okay. This is the update. "In response to Work Group request to SC&A, review the new approach to neutron dose estimation. SC&A provided a review that was forwarded to the Work Group and NIOSH on December 27th, 2010.

"At the May 3rd, 2011 Work Group meeting, SC&A also raised the need for NIOSH to demonstrate in its upcoming response to this SC&A review how the proposed parameter

for MCNP are bounding for the ranged of systems assembled/disassembled for the period '51 to '91 at Pantex."

And that was the endpoint. I believe NIOSH provided that information as far as the different systems.

And we -- and I'll have to go back. We would clarify the implication in a memo report now that the notes have been cleared. So, we presented something and I don't have it with me on that.

MEMBER BEACH: Right. And the last thing we got on this issue was the March paper and it's quite extensive of a write-up and I'm -- does that incorporate NIOSH's work?

MR. FITZGERALD: That's the MCNP.

MEMBER BEACH: That is the --

MR. FITZGERALD: And like I said, our concern was using the neutron-photon ratio for 30-some years covering all particular operations and, you know, we felt that wasn't

going to work for all those time periods and operations involved and that's where the new approach was introduced and I -- you know, I can go back and verify. I don't have it with me, but we had a memo, and I recall it, that basically closed this out. Recommended closure and laid it all out. We had it cleared by DOE and sent it to the Work Group. But, this was two years ago.

So, I can -- if you want to condition closure based on my resurrecting that memo and making sure that everybody sees it again, but we felt satisfied with the new approach on neutrons and the issue of reliance on the ratio across all time periods effectively went away.

CHAIRMAN CLAWSON: Okay. Like you said, I'd conditionally close that, but I'd still like to review it. It's been a while.

MR. FITZGERALD: I'm going to go ahead and get --

MR. KATZ: No reason to conditionally. Just we'll put that in the same teleconference when we address issue five and six.

CHAIRMAN CLAWSON: Okay. I just wanted to read something to you. I just got a text from Sarah Ray that she wasn't going to be able to join us back, but especially for Mark, myself and Joe, "Please let the Work Group know that two wonderful members that they interviewed passed away. Bob Tolley and Tomes. Just passed away."

You guys interviewed them and they were -- "Appreciate for all your help. Sorry I won't be able to return. Tell everybody thank you for all the sincere work and honest effort that they've put into this. Thank you, Sarah Ray."

So, those people were inspirational. We interviewed them quite in depth. They were the last ones that we

weren't able to get to. They were in a rest home and stuff. So, I just wanted to make sure that everybody knew that. Okay.

MR. FITZGERALD: That was item seven. So, that's held pending. Producing an update and further information.

Eight is completeness of interpretation of historical radiological exposure sources. This is kind of one of these -- it's not a technical issue. It's just the review that we had done of the TBD. We felt there were operations -- historic operational things. Like I think we cite Tweezer which is the off-site activity and other things that were not covered in the TBD and what we basically concluded, the types and sources raised exposure at Pantex from an historic had not been fully characterized in the TBDs.

And I think the document of note for that issue is the data completeness

adequacy piece again that was submitted in April 2011 to the Work Group and NIOSH which talks to some of these activities.

That is open. I mean it's certainly a TBD question of completeness and it's not very different from some of the other issues we raised at other sites saying that we identify certain activities or exposure sources that don't seem to be addressed in TBD and it's almost one of these informational things that certainly in the next revision maybe consideration ought to be given to including that for the sake of the dose reconstructor and that's kind of how we left it.

And I think almost every review -- Site Profile review we kind of highlight if we find anything that doesn't seem to be addressed in the TBD. We highlight it for information's sake.

So, that certainly is where that

comes from.

MR. KATZ: Just a question, sir. Do these things that have been omitted, these operations that have been omitted, they have a functional impact on how the dose reconstruction would be done?

MR. FITZGERALD: No. Again, that's what I'm saying. The way it's worded, it's completeness of the historical rad exposure sources. It's just a question of source terms. Whether there's any source terms that aren't highlighted in the TBD as a source of the exposure. It wouldn't change the methodology necessarily.

MR. ROLFES: This is Mark and we did address this in the past I know, but the Tweezer facility operations weren't conducted on-site at Pantex. They were off-site at the Nevada Test Site.

CHAIRMAN CLAWSON: Did we address the broken arrows that came in though? How

they dealt with that. Because that was a --

MR. HINNEFELD: Well, did any come in before '58 or what -- I mean the broken arrows that came back to be examined would have been either --

MR. ROLFES: Thule and Palomares wastes were shipped from the Medina facility at the time of closure over to Pantex and there was an incident that occurred in 1979 at Pantex in one of the igloos and we do have -- there were some contamination measurements made and some bioassays requests -- for bioassay samples requested from the employees.

We do have a few memos and some group participants on who entered that igloo and was involved in the clean-up of the plutonium and tritium waste that were shipped to the sites.

CHAIRMAN CLAWSON: Well, because that -- I'm trying to -- how can I word this? Because this was one of them that came in

that actually burned up in the fuel and --

MR. FITZGERALD: Let me suggest this. You know, we recognized that there were a number of questions about completeness and that was the genesis of writing that White Paper in April of 2011 saying okay, now here is the, you know, collection of sort of completeness questions in terms of exposures, source terms, what have you that we felt were germane to Pantex and we put that in that particular paper.

I don't think between NIOSH and the Work Group, we actually have dispositioned that paper and I would suggest that on this item as well as the previous item we, you know, go back to taking that White Paper and just dispositioning it. Because I think that's going to be the way to resolve, you know, where everything stands as far as the TBDs and everything without getting into a new round of discussion. That paper pretty much

presents this issue as well as questions on the databases.

Now, the databases are pretty much resolved because we resolved them as part of the SEC discussions, but in terms of operations and whatnot, that's also reflected in the -- in that document and in the matrix we point to that document as the hand-off point for this issue anyway.

So, I think that's -- you know, given the way it's laid out in the matrix, that would be the way to address it.

CHAIRMAN CLAWSON: Okay. We could regenerate that one or --

MR. FITZGERALD: Well, it's out there and I think it's even posted. So --

MEMBER BEACH: Well, I was able to find NIOSH's responses, but the dates are wrong and that was in May. But, I never did in just my quick review this morning find --

MR. FITZGERALD: Okay.

MEMBER BEACH: -- find the data adequacy paper of April.

MR. FITZGERALD: It's April 2011.

MEMBER BEACH: Yes. And I'm sure I'll find it, but maybe we could resend it.

MR. HINNEFELD: Okay. So, we'll look then at the April 2011 completeness and adequacy paper and then also our May paper and see what we responded to there.

MEMBER BEACH: Yes. Yes.

MR. HINNEFELD: Some of these things may have been overcome by events.

MEMBER BEACH: This says March. Which seems odd.

MR. HINNEFELD: You know, when you have an SEC Class '57 through -- or '58 through '90 and what we can do in that period for internal intakes is going to be limited by what's in the person's exposure record. So, you know, as a practical matter, it sounds like it's probably going to be dispositional.

MR. FITZGERALD: Okay. So, again, I think that would be item eight. That we can focus on the paper and where NIOSH stands on that and I think some of these issues can be speedily addressed.

Number nine deals with incidents and where we felt there was some incompleteness with the incidents that were acknowledged in the TBD and that too is addressed in the data completeness piece.

So, again, I would suggest rather than sort of having all these separate issues since that was how it was all consolidated that that can be addressed similarly.

CHAIRMAN CLAWSON: That's fine with me. Yes.

MR. FITZGERALD: Yes, I think that was actually the purpose of that paper -- was trying to assimilate all these different SEC matrix issues and make sure they were addressed in some form so that it's in that

document.

Number ten, inadequate consideration given to the firing sites. Obviously, in this last rendition, we spent some time focusing on that at the site and felt that that should be closed as part of the SEC process.

CHAIRMAN CLAWSON: All right.

MR. FITZGERALD: There were some questions there, but now, there is a residual question on the hydroshots that is actually in this recent paper. Where I think a 95th percentile is used based on the data from the '60s applying it to the hydroshots with DU and we raise a TBD question there as to whether that's conservative enough given the variables that are cited in that analysis and there's quite a few variables, wind direction, assumed locations and they're all cited in the footnote to that item in this most recent paper we just gave you.

And that is something I think that would bear some further discussion as to --

CHAIRMAN CLAWSON: Right.

MR. FITZGERALD: -- whether the 95th or maybe even consideration at the 98th or 99th might be appropriate given the number of variables involved in coming to the conclusion, but again, that's a judgment call.

CHAIRMAN CLAWSON: All right. So --

MR. KATZ: Is there more that's needed from NIOSH on that?

MR. HINNEFELD: So this was described in the paper you just delivered last week?

MR. FITZGERALD: Yes, it's touched upon. I wouldn't say it's really --

MR. HINNEFELD: Touched upon, but it refers back to earlier work?

MR. FITZGERALD: Yes, it -- in closing out the hydroshots in the '51 to '57,

if you look at that one, it's on page 16 to 17. Actually, it's on page 17.

We go on to say "The raw data SC&A reviewed do not support use of the 95th percentile. The 1960's outside air concentration of 24 picocuries per cubic meter's appropriate or necessarily claimant favorable given the likelihood of the large variance due to highly variable conditions during firings" and in the footnote, I say "For example, the TBD cites differing masses of DU and HEU that exist at location samplers in relation to cloud, varying particle sizes" and sort of a list of variables that were involved.

And given the number and extent of the variables, we're questioning whether the 95th might actually be conservative enough and whether some consideration of something that would be more conservative would be warranted. That discussion we really haven't had. That

was actually a Site Profile finding that got carried over into the matrix, but, you know, was acknowledged in the Site Profile issue from the get-go.

So, not a question of whether you can do it. Whether the variables involved would argue for something more conservative at 95th. That's just hydroshots.

MR. KATZ: Right.

MR. FITZGERALD: So, on number ten, that would be the only question that's sort of out there.

CHAIRMAN CLAWSON: Right.

MEMBER BEACH: So, we would put that under a Site Profile to be --

MR. KATZ: Yes, for the teleconference, too.

MEMBER BEACH: Okay.

MR. FITZGERALD: And there may be a valid, you know, basis for saying the 95th is conservative enough, but we were concerned

given the extent of the variables cited.

Number 11, we're recommending closure which is the question of whether the most highly exposed worker was badged or not. I think there was a fair amount of discussion in the Work Group on that. Let's just go back to this.

I'm not going to read all of this, but we say "The information presented on this question by NIOSH addresses practices in the later disassembly years 1980 to 2000, but not in the earlier era. While no documentation is valid regarding the implementation of monitoring against these requirements, the issue of back extrapolating exposure experience and monitoring effectiveness has been challenged by SC&A for internal dose estimation. For external dose estimation, SC&A has indicated at the May 2nd, 2011 Work Group meeting that it believes the use of latter-day dose distributions for coworker

dose assignment per the Strom 2004 study is sufficiently accurate for the weapon systems involved."

So, again for external which is what we're talking about here, we felt the basis in Strom 2004 was sufficient and this is again after some discussion. This went back and forth for a while.

CHAIRMAN CLAWSON: Okay. So, that one is going to be closed then?

MR. FITZGERALD: Number 11's recommended for closure.

CHAIRMAN CLAWSON: Any Work Group --

MEMBER BEACH: I agree with that.

MEMBER SCHOFIELD: I agree with that.

MEMBER POSTON: I agree.

CHAIRMAN CLAWSON: Thank you, John.

MR. FITZGERALD: Number 12 is accuracy of plant exposure data. This is a

petitioner issue and let me just read what the issue is.

"The ER implies that early film dosimeter data for Pantex are reliable. The ER and external TBD do not recognize the inaccuracies in calibration methods and uncertainties introduced into the dosimetry program by poor or improper practices. In an assessment of the external dosimetry program, the," and this goes back to a DOE investigation, "the DOE investigation cited key findings that concluded the following."

This is from the petitioner. "Gamma calibration response curves for TLDs did not have sufficient range. The scientists and laboratory technicians assigned to the Pantex dosimetry program were inadequately trained. There were no formal operating procedures for the Pantex dosimetry program. The quality of the Pantex dosimetry program was less than adequate.

"SC&A considers the deficiencies identified by the DOE investigative Board to be highly relevant to the credibility of the dosimetry data for Pantex. The ER needs to consider these deficiencies for their implications on the accuracy of external dose reconstruction."

Number two, "Further complicating matters are issues with individuals not wearing their dosimeters all the time. During a survey of film badge utilization in June '69, Pointer, a name, found several instances where personnel were not wearing their badges. The extent of issues that involved inappropriate wearing of the dosimetry is unknown. However, radiological control staff subsequently established a program to spot-check badge racks to determine whether individuals were wearing their badges."

And this is the bottom lines. The deficiencies noted in the 19 DOE -- 1980 DOE

investigative board only apply to the '72 to 1980 period that the TLD program was operated in-house. Prior to '72, film badge service was supplied by a reliable commercial service. From '80 to '93, the TLD dosimetry program was based on a reliable Panasonic TLD and readers with an acceptable uncertainty range. After '93, the DOELAP-accredited Panasonic TLD program had an uncertainty range that was even less than that. Plus or minus 10 percent.

And then finally, SC&A noticed that this additional information response to petitioner issues -- notes this additional information that was provided by NIOSH in response to petitioner issues and recommends that the Work Group consider this issue closed. This was back in May of 2011.

So, that additional perspective was provided.

CHAIRMAN CLAWSON: So, I move that

the Work Group would close.

MEMBER BEACH: I agree.

MEMBER SCHOFIELD: I agree.

MEMBER POSTON: I agree.

MR. FITZGERALD: Okay. Number 13 was too few workers monitored for valid dose reconstruction. This again was a petitioner issue, and our initial review showed that statistics provided for external monitoring by year are based on limited data prior to 1958. There was limited data prior to '58.

The ER, Evaluation Report, does not provide the population of radiological and non-radiological workers by year for comparison to the number monitored. So, it's difficult to answer that petitioner question from that standpoint.

Early monitoring was concentrated on radiographers. This is back in the '50s. Whereas, later years included multiple job categories.

The ER has not demonstrated that variations in badge radiation workers are the result of changes in weapons production rates on the rad material present.

Now, in turn, NIOSH cites ORAU 13-6 and a SRDB reference 14338 by statistical responses to SC&A questions regarding concerns raised by the petitioners.

And, again, we haven't provided the Work Group an assessment of that particular issue, but the statistics provided by the TBD and this particular reference basically provides the distribution that supports the NIOSH ER recommendation.

But we haven't closed that out yet. That's something that we owe the Work Group.

CHAIRMAN CLAWSON: Can I say, because that also brings in a question that I had in the earlier years and so --

MR. FITZGERALD: Yes, the earlier years.

MR. KATZ: SC&A action.

MR. FITZGERALD: Right. Number 14, records incomplete for subcontractors, temp workers, short-term employees. Again, another petitioner issue.

And the question was the response was not specifically provided in any Evaluation Report. And we have as initial review SC&A responses pending additional record review, and our latest reading is all short-term or temporary workers were treated as visitors and monitored as such. These records were preserved by name and other identifying information.

So, we recommended, based on our review of that and -- oh, I'm sorry. Let me go a little further on that.

We've reviewed additional data capture information in our visit to Pantex. Unless additional information is found that would be inconsistent with NIOSH's

characterization of past practice and how temporary or short-term employees were handled, we would recommend Work Group closure of this issue.

This was back in May of 2011 and we have not found anything that would question how that -- how short-term workers or temporary workers were handled. They were handled as visitors and we pretty much have found that to be the case in terms of the records.

So, we would recommend closure of that issue.

CHAIRMAN CLAWSON: We can close that one?

MEMBER BEACH: I agree.

MEMBER SCHOFIELD: I agree.

MEMBER POSTON: I agree.

MR. FITZGERALD: Number 15 is exposure from tritium leaks, and that was another petitioner issue.

You know, reservoirs began arriving at Pantex in late '56 or early '57. However, there's no mention of how tritium doses prior to '60 would assessed. So, there's a bit of a gap there in terms of tritium reservoirs.

The ER indicates that Pantex tritium monitoring focused on workers of the highest likely exposure. Furthermore, they indicate this data can be used to bound tritium dose.

Prior to 1972, the ER suggests that ten individuals were randomly selected per month for tritium bioassay from about 1960 to '71. However, the ER does not explain how the "highest likely exposed" individuals were selected and how they have verified this assumption.

Table 5-3 of the TBD indicates that the number of workers monitored for tritium uptakes was not constant and only up to four workers were monitored per year from 1972 to

'75.

But it goes on to say that -- so, there's some questions about the TBD in terms of how tritium exposures were characterized, and we go on to say that the TBD does not clearly define either the data used or the values that were derived from the data. So, there's some questions on how tritium in the early years was done.

And we go on to say this issue's addressed in more detail in the data completeness and adequacy paper of April 2011, and we'll defer further conclusion until responses forthcoming from NIOSH.

So, again, this is a question of data completeness. In this case, where tritium is included in that paper. So, again, this is one that would --

CHAIRMAN CLAWSON: Still be open?

MR. FITZGERALD: That would be open, but would again be subsumed within that

review of that particular paper that was generated.

To be frank, it was generated in April of 2011, right before the last Work Group meeting and we were in the throes of trying to disposition the W28 question. So, I think, you know, since this was a Site Profile question, it just wasn't picked up on at that point in time.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: So, this is a NIOSH response.

MR. FITZGERALD: To -- yes, for the paper.

MR. KATZ: Right.

MR. FITZGERALD: Just to go back. Item -- and there's only two more --Item 16 is badge placement, and this was another petitioner issue, and there wasn't a response that was addressed specifically in the ER. This gets to worker geometry and proximity to

radioactive materials, and in particular with the systems that were being handled. That, you know, obviously was a pretty important question and how they were handled in the early years were such that there was quite a bit of contact. So, the question of geometry was relevant.

And the petitioners have stated some workers held bare pits on their laps during some work practices such as cleaning the pit surface. It was determined that the surface of a new pit could be cleaned in only a few minutes prior to assembly. It was also determined that throughout the history of Pantex operations, pits and other components have been handled in various fixtures.

NIOSH acknowledges that while some workers could have held pits in their laps, it would be possible to estimate conservative doses requiring some adjustment to calculated organ doses for work in the early years. The

early years in this case being '59 to 1970.

When use of fixtures for handling pits was not rigorously required, i.e., after '70, you had these frames that could be used. Before that, it's certainly, from worker accounts, likely that they held pits in their laps and that kind of thing.

For pit operations that took place at waist level, the guidance of OTIB-10, and this is OCAS 2005, should apply and would be an adequate basis for any corrections to organ doses.

And our response in May of 2011, SC&A still questions how NIOSH will apply guidance from OTIB-10 for a glovebox geometry to a -- that was for a glovebox geometry. For a more variable geometry such as a worker handling a pit, which is obviously different, so if the basis for an OTIB-10 is a glovebox geometry, how would that fact be applied to a much more variable situation where you're

having direct handling of a pit?

That was kind of an outstanding question given that initial NIOSH response.

CHAIRMAN CLAWSON: This is still an open issue?

MR. FITZGERALD: Yes, I would think so.

MEMBER BEACH: With NIOSH having the action. Right?

CHAIRMAN CLAWSON: Yes.

MR. FITZGERALD: And maybe there's something that has superseded OTIB-10, but that's where we had left it.

The last one, item 17 is the efficacy of the health physics and IH programs. This was a petitioner issue and this goes back to the 1990 Tiger Team report on Pantex which raised a number of HP and IH programmatic issues, and I think we're pretty familiar with most of those. And I'm not going to go through all of them. But, you

know, there's a litany of issues that were raised about how the IH and HP program was managed.

SC&A addressed the adequacy of employee exposure records under Item Two and Seven of this matrix. The characterization of the workplace exposure conditions was addressed also under Item One. So, we were trying to parse out, of these many issues, which ones we had already addressed.

Health physics support staffing levels and training, general health and safety, program inadequacies and the controlled rad sources provided valuable background information on the effect of control of the short-term, but are not directly pertinent to dose reconstruction. There was a lot of stuff that was in there on program management that didn't bear on dose reconstruction. So, we wanted to make that clear.

And maintenance of survey records, contamination records and field air sampling records were mentioned by the petitioner as being important to the dose reconstruction effort in the absence of personnel monitoring data. And we felt that was -- you know, that was something that NIOSH considers, you know, whether or not you have secondary survey records, monitoring records and field air sampling to backup dose reconstruction. That's the hierarchy of what data is relied upon.

In any case, we in this particular item recommend that this be closed, and the issues that we felt important were, in fact, addressed elsewhere in the matrix. So, you know, the ones that dealt with the adequacy of -- let me see. You know, characterization of worker exposure conditions and the completeness and adequacy of the employee exposure records themselves for both internal

and external, which were three of the primary items that were cited in that particular petitioner comment we obviously addressed already elsewhere in this matrix. So, we felt this was one that could be closed as far as having been addressed pretty completely during the course of the Work Group proceedings.

CHAIRMAN CLAWSON: Okay. I move that it be closed.

MEMBER SCHOFIELD: Second that.

MEMBER BEACH: I agree.

MEMBER POSTON: I agree.

MR. FITZGERALD: And that is the list.

MEMBER BEACH: Joe, the last thing was the addendum note regarding burn area exposures, and everything has been covered on the note that you have in the matrix. I just wanted to just double check that that is all completely covered with the other items.

MR. FITZGERALD: Yeah, this is

addressed in the most recent analysis. Just to, again, one reason we went ahead and put the burn area exposures and the hydroshots in the most recent paper is that, one, they're germane to the early period, but, two, I didn't think we really dispositioned them completely. We had touched on, we had raised some questions, we had some dialogue. But I thought we ought to close them out. So, that's closed out relative to the most recent paper.

We felt the information, the air sampling data that was available for the burn pits was, one, you know, certainly extensive enough for that time period and, two, the practices behind how they did that in the late '50s was comparable and representative of the '60s when the data was actually captured.

So, the issue for us is could you take that data from the '60s and use it for the late '50s on the burn pits? And we

concluded, yes, you can because you have enough of it and the practices themselves had not changed in any degree.

We have interview information that sort of supports that fact. We have the individual who was in charge of the burn pits from the '50s into the '60s. So, that was pretty strong substantiation of that.

So, this was really put in here as a footnote to make sure that there was some detailed information on both the burn pits and hydroshots because, again, there was some nuances there that we thought wouldn't be captured in the Site -- I'm sorry, the issue matrix itself. So, that was tacked on the back.

But, that's the reason it was addressed more fully in this latest paper.

CHAIRMAN CLAWSON: And the only one in question is the hydroshots. The burn pits, we've determined that we've got sufficient

data to be able to cover that. That was the placement of the air sampling data, the boundaries around --

MR. FITZGERALD: Yes, and if you read this here, this is almost the same assessment. In fact, I had looked at this when I wrote the other paper. We raised that question relative to the hydroshots. For the burn pits, we raised the question that all the sampling data came from the '60s, and so the question was whether you could retroactively apply it. And as we say in this latest analysis, we feel you can.

So, we kind of -- these are just laying out the issues, but they don't really provide any conclusions. The latest paper provides the conclusions, but we do have that one hydroshot issue left.

MEMBER BEACH: And I just found the last -- NIOSH's response to SC&A for data completeness and adequacy was August 5th,

2011. That's when we got that.

MR. FITZGERALD: Okay. So, we actually have something we can use then.

CHAIRMAN CLAWSON: Can you forward that to me, Josie?

MEMBER BEACH: Sure. It's on my flashdrive. I'll give it to you off my flashdrive.

MR. KATZ: So, are you saying, Josie, that some of these things that we think are not action items may already be addressed in that response?

MEMBER BEACH: Potentially.

MR. FITZGERALD: It may be SC&A's action to close that out then.

MEMBER BEACH: To look at their responses.

MR. FITZGERALD: All right.

MR. HINNEFELD: I think we can take a read, too. I mean this going to be -- you know, it's kind of getting long in the tooth,

and so we'll take a look at the April report from SC&A and our response and how we feel about it today.

And some of these things might be subsumed by events that have occurred in the meantime.

MR. FITZGERALD: So, we'll deal with it on a joint basis and the timing of August 2011 was, of course, the W28, the date. So, I have a feeling that the response came in in the mail, but may not have been focused on at that time given the Site Profile issue.

MR. KATZ: So, then it seems to me, for the Board meeting, Joe, you can give just sort of a very summary status of the TBD and let the Board know that down the road there will be a teleconference and then there will be a full presentation to the Board to close up the TBD matters.

MR. FITZGERALD: Right. And it'll be much -- obviously, much more succinct.

MR. KATZ: Yes. Right.

MR. FITZGERALD: But, yes, I --

MR. KATZ: But you don't really need to go into details at this Board meeting on TBD matters.

MR. FITZGERALD: No. No. Just to acknowledge that we do have a few loose ends that --

MR. KATZ: Right.

MR. FITZGERALD: -- you know, we had shifted focus to the uranium and thorium and are returning now to some loose ends that exist.

CHAIRMAN CLAWSON: And let me bring up a question on that. Because I'd really like to keep the Site Profile issues separate from the SEC issues. I'd like to take care of them first and then maybe just have a follow-up on the remaining issues there.

MR. KATZ: Joe, I think we've got an hour and half for Pantex, too. So, we

should have plenty of time for, you know, him to give a footnote at the end about where the TBD business stands.

CHAIRMAN CLAWSON: Right.

MR. FITZGERALD: So, I was just pointing out to Josie in the presentation in August before the full Board, that we acknowledge --

MR. KATZ: Yes. Yeah, our timing that you were saying.

MR. FITZGERALD: Yes, so -- yeah, again, we have to close that out.

MR. KATZ: Okay. So good.

CHAIRMAN CLAWSON: Okay. Are there any more questions that need to come before the Work Group or, if not, I suggest that we adjourn the Pantex Work Group at this time.

MR. KATZ: Thank you, everybody, for all the hard work and have a good day, everyone on the line.

MEMBER POSTON: So long.

MR. KATZ: Bye, John. Very good.

(Whereupon, at 11:53 a.m., the meeting in the above-entitled matter was adjourned.)