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

PRESENT:

BRAD CLAWSON, Chairman
JOSIE BEACH, Member
BOB PRESLEY, Member*
PHIL SCHOFIELD, Member*

1
ALSO PRESENT:

TED KATZ, Designated Federal Official
ISAF AL-NABULSI, Department of Energy*
TIM ADLER, ORAU*
KATHY BEHLING, SC&A*
BOB BISTLINE, SC&A*
MEL CHEW, ORAU
KATHY ROBERTSON-DEMERS, SC&A
JOE FITZGERALD, SC&A
STU HINNEFELD, ORAU
JENNY LIN, Health and Human Services
JOHN MAURO, SC&A*
ROBERT MORRIS, ORAU*
JIM NETON, ORAU
SARA RAY*
MARK ROLFES, ORAU
PAUL RUOER, ORAU
BILLY SMITH, ORAU*
MATTHEW SMITH, ORAU*
JOHN STIVER, SC&A

* Participating via telephone
Welcome and roll-call/introductions

WORK GROUP DISCUSSION:

Summary of Activities since last meeting

NIOSH White Paper: Pantex Bounding Uranium and Thorium

NIOSH Responses to "SC&A Draft Data Completeness and Adequacy Review for the Pantex Plant"

SC&A Summary of Pantex Onsite Review (memorandum)

Resolution of remaining SEC Issues

Work Group conclusions and recommendations to full Board

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

9:00 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. We are just getting started, and we will begin as always with roll call, beginning with Board Members with the Chair.

(Roll call.)

MR. KATZ: All right, so that takes care of roll call. Let me note for everyone on the line, please mute your phone except when you are addressing the group. If you don't have a mute button on your phone, if you press *6, that will mute your phone, and then press *6 again, and it will take it off mute; and, please, nobody put the call on hold. Hang up and dial back in, if you need to leave for a piece. Thank you.

We have an agenda, which is posted
on the NIOSH website in the Board section.

Brad, it is your agenda.

CHAIRMAN CLAWSON: Appreciate it.

The first thing on the agenda that we are going to discuss is activities since our last Work Group meeting.

We did have a meeting, and it wasn't really a Work Group meeting, but we got together to review some documentation in Germantown to be able to review some of the classified documents that have been posted there.

Also, SC&A and NIOSH have made site visits since our last Work Group meeting, and captured some more documentation. Does SC&A want to mention anything on our data capture that we have done, or NIOSH? Any new information?

MR. ROLFES: Well, if Joe -- I don't know, Joe, do you want to answer anything?
MR. FITZGERALD: Well, I think after our session in Germantown, we noted that we wanted to go back to the site, and we did. We spent the better part of four days at the site, did a couple of additional interviews. This was all kind of focused from our discussion in Germantown. So this was definitely a very focused type visit. I outlined some of the touchpoints in this two-pager that we managed to get reviewed by DOE and is cleared.

The detailed notes, I understand from Mark, have made their way to Germantown. So they are available to both sides when the Work Group wants to take a look at them. So I think that is probably the key thing, and all that information that we did get and all the notes from the interviews are at DOE, Germantown.

MS. ROBERTSON-DEMERS: This is Kathy Demers. I actually wanted to direct a
question at Isaf. Now I know that, from
talking to Mike Lohr before he moved on, that
the classified documents from our trip to
Pantex were sent to DOE Germantown.

There were some unclassified
documents which we don't know the status of at
the moment, but the direction we provided to
Pantex and the subsequent direction we
provided to DOE were to provide us with a
redacted copy of our notes from Pantex.

I was wondering if you knew the
status of that and whether -- or if you could
find the status of that, because we need a
turn-around on those notes.

MS. AL-NABULSI: I will check that
for you, Kathy.

MS. ROBERTSON-DEMERS: Okay.

CHAIRMAN CLAWSON: Isaf, when you
send that out, could you also make sure that
the Work Group has a copy of that and so
forth?
MS. AL-NABULSI: I will.

CHAIRMAN CLAWSON: Thank you very much.

MS. AL-NABULSI: You are welcome.

MR. ROLFES: Yes, I think for our updates, we went down and looked at the same records that were pulled for both SC&A and NIOSH. We made some selections, and as I understand, those selections were forwarded up to Germantown as well. We haven't had the opportunity to review them in detail. We just made some initial selections to look for examples of the types of records that were available to us.

We looked for some contamination swipes, which we took a sampling of, made some notes regarding how many samples there were and which systems were involved. Let's see, what else did we find? We had looked at some earlier access registers, information on which employees worked where, some of the early
training records. I am trying to think what else there was. There was both some dose rate surveys and some contamination surveys that I recall. Anything else that I haven't -- No? Okay.

MR. CHEW: No.

MR. ROLFES: There might have been some early --

MS. ROBERTSON-DEMERS: This is Kathy Demers again. In one of the documents we looked at, it was a log of hydroshots, I believe, and there was mention of thorium in one of those logs, and I think we need to look into that a little bit more and see if that shot actually occurred.

MR. ROLFES: Yes. We had spoken with a couple of subject matter experts about this, and there was one individual who had said that it was possible that one had occurred, but they couldn't pinpoint a date.

So what we have done in our Site
Profile to account for that is to apply an exposure from one shot which could have resulted in exposure to thorium. The date wasn't known by the subject matter expert or whether or not it actually had occurred.

So we assumed that it did occur, and had assigned an intake based upon some air monitoring data from the area where it would have occurred.

MS. ROBERTSON-DEMERS: Okay. Is this a proposed assignment or is this actually in the Site Profile as it exists now?

MR. ROLFES: This is in the Site Profile that is actually being used.

CHAIRMAN CLAWSON: Is that it, Mark?

MR. ROLFES: Yes, that is all.

CHAIRMAN CLAWSON: Okay. I appreciate that.

MR. HINNEFELD: I had one question, just to go back to your document
that got cleared and got to us. Was that the summary of the interviews? Is that the document you are talking about?

MR. FITZGERALD: No. Actually, what I wanted to do, knowing the lag time in redactions we have experienced in the past, was to at least get something out. So I talked to the Pantex folks, said if I wrote a two-pager, with the likelihood of that being available for the Work Group, would that be fine, and they said yes.

So what I did was just do the highlights of what we found from the records as well as what we gleaned from the interviews, but these are just simply the highlights. So there is a whole interview summary -- actually, this is what Kathy is referring to -- and other documents that we requested that are in Germantown, apparently, right now.

MR. HINNEFELD: But I mean, what--
DR. NETON: It was an email with twenty-eight --

MR. FITZGERALD: The memo isn't a summary of the interviews. What it is, it is just the touchpoints, the highlights of what we learned while we were there. This is kind of what we took from our visit. It was four days. This literally is just the highlights, and the details and the references of the interview notes are in Germantown. I suspect that will take time to get those redacted, in our experience. This took four or five weeks.

MS. ROBERTSON-DEMERS: This is Kathy, just to clarify something. The site expert interview summary that was sent out to the Working Group covers our interviews back from the Site Profile review through, I think, an August visit in 2010, the two interviews that we conducted on this last visit -- actually, one was related to Pantex; another was related to the Medina site. Those are in
raw note form, and that is what was sent to Germantown.

The process for getting interview summaries reviewed by the interviewee is very cumbersome. So it would probably be easier to review our interview notes, if you want to view something in a timely manner.

MR. HINNEFELD: Okay.

MR. FITZGERALD: I think the interview that we did do at the site, which is alluded to in our little memo, was a pretty good interview. So I would certainly suggest that, if it is there unredacted in Germantown, it would be worth looking at. We found it pretty useful.

MR. HINNEFELD: And that is the one you did in -- since Germantown.

MR. FITZGERALD: Yes, since Germantown, this last visit we spoke with a -- I think he was an operating engineer who was involved with the systems that we are talking
about, and went back far enough that he has
some pretty good perspective, and we touch on
some of what he said here, but I don't have
everything here.

CHAIRMAN CLAWSON: Stu, that was
our June 20th through the 24th down at Pantex.

MR. KATZ: Then just to clarify,
Joe, this two-page memo, is it summarizing a
longer written piece that sort of pulls it all
together that they are still clearing or are
there just the notes and the documents?

MR. FITZGERALD: No.

MR. KATZ: The primary documents
and then this?

MR. FITZGERALD: Yes. Literally,
we don't even have the primary documents
cleared such that we could even write a
summary yet. So I wanted to get something out
of it, given the short time frame, and this is
what we were able to get out of it.

CHAIRMAN CLAWSON: While we were
down there, Joe talked to me, and he said he was worried about us being able to get this before the Work Group. So this is why he built this two-pager for us to be able to go through.

MR. FITZGERALD: And this has been redacted. So, you know, the unredacted version of this is at Germantown as well. So there we go.

MR. ROLFES: I guess I just wanted to point something out about travel right now. We are, obviously, not going to be able to get to look at those until about October sometime because of travel restrictions right now due to the end of the fiscal year. So it is going to be a little delay, at least for us to be able to go up and get our eyes on those documents.

MR. HINNEFELD: We have a particularly early travel cut-off. So our travel plans have to be essentially made by
now pretty much.

MEMBER BEACH: Isn't it the 15th, this cut-off, for us I think it is?

MR. HINNEFELD: Yes, the hard and fast on it is the 15th which, of course, is Monday.

CHAIRMAN CLAWSON: Okay. The first thing on the agenda here is the NIOSH White Paper, the Pantex Bounding Uranium and Thorium. I would like to note again, Mark, that you received this over the weekend. It seems to me to be kind of a trend, just right before the Work Group here. It makes it very hard for us to be able to respond or to be able to even review these when we get these this late.

If nothing else, I would at least appreciate -- you know, the reason we put this later on into August was to be able to give us three weeks time after your paper was done to be able to review it. So in the future, at
least make a notification that we are going to be late.

MR. ROLFES: Sure.

CHAIRMAN CLAWSON: I would appreciate that, because it makes it pretty difficult to be able to respond, but we will do the best that we can.

Do you want to go over your paper and explain it?

MR. ROLFES: Yes. I will just give a quick summary. It is a seven-page paper, sort of responsive to the memo that was put together, and it is basically sort of laying out some of the reasons that we feel that our uranium intakes are bounding intakes.

Basically, the way we developed our Site Profile, it basically relied upon the 300 bioassay samples that were collected in the 1989-1990 time period, basically following an incident that occurred where there was some depleted uranium contamination which workers
identified on their clothes and gloves. They had basically been doing this same operation for about the past five years, and this operation, the contamination incident sort of stopped operations and prompted the site to take bioassay samples from about 300 people who could have potentially been involved in this operation.

Basically, that five-year chronic exposure period and those 300 bioassay formed our basis in the Site Profile for dose reconstructions. We went back and interviewed -- as a result of the last Work Group meeting, we went back and interviewed several individuals from the site who had some involvement in that particular incident that occurred in 1989.

We asked them questions: If this contamination incident involving uranium would have been a bounding-type potential for exposure. I think everyone that was involved
agreed that it would, and they gave several reasons, primarily due to the length of the
time that this particular weapon had been in the stockpile, due to the size of the uranium that was involved.

Basically, this particular series had been stored in various temperatures, humidities, areas, different places out in the field for many, many years, up to 30 years in the stockpile; and since the corrosion was dependent upon the time that the components were out in the field or the weapons were out in the field, the internal potential for exposure was really an issue with the oldest weapon programs.

This particular program was one of the ones that Pantex was aware of as having a potential for uranium oxidation or corrosion to develop, and that knowledge was likely gathered as a result of the stockpile surveillance program that was conducted,
basically since 1958 forward.

Basically, we looked back at the survey data as recorded in documentation we have received from the site, and looked at the contamination levels in the area where this incident occurred, and the contamination levels, I think I mentioned earlier, on the workers' coveralls and gloves were in between 200 and 400 dpm per 100 square centimeters.

We also looked at the survey data that was collected as a result of the contamination that had built up in that cell, then came up with various ranges of both fixed and removable alpha contamination in there.

We looked at the air monitoring data also, basically for all operations. We had previously looked at some of the air monitoring data. We had initially done the analysis from the seventies through the eighties, I think. That was when we had the most of the data, but since that time we have
identified additional air monitoring data from the sixties.

So one of the things that we are currently working on is updating our analysis of the average air concentrations in the cells and bays over time. It looks like we have found, at least tentatively, an increasing trend. It appears that the air concentrations in the work areas in the earlier years appear to be lower, and then as the disassembly work ramps up in the seventies and eighties, it appears that the average air concentrations increase.

We have actually compared -- Now these aren't breathing zone samples. They are general area air samples which are taken from a pretty small work area. We have actually compared intakes from those air monitoring results to the intakes that we have defaulted to based upon uranium bioassay data, and we are using the intakes developed from the
uranium bioassay data to perform dose
reconstructions for Pantex employees, because
that results in a much higher intake and
resulting internal dose.

We have also asked the involved
subject matter experts if there were any other
weapon systems that had similar oxidation
concerns. There were a couple mentioned, but
none were of the magnitude of the B28 incident
that occurred in 1989.

We have also included a
description here which is slightly different
from our original approach in the Site Profile
to assign thorium intakes. I think we had
defaulted to a 40 DAC-hour thorium intake for
every year of employment at Pantex. We
actually have gone back and evaluated some air
sampling data from disassembly work, and came
up with a thorium intake based upon that.

I don't know if there are any
questions about my quick summary here.
MS. ROBERTSON-DEMERS: I have a couple of questions. You interviewed four people.

MR. ROLFES: Correct.

MS. ROBERTSON-DEMERS: I think. Can you tell us what their job responsibility was?

MR. ROLFES: Yes. Let's see. I believe two of them were safety engineers. One of them was a health physicist. Well, I guess you could call all three -- the two safety engineers could also be called health physicists as well, because they had experience in radiation safety. They had received radiation safety training, both prior to their employment at Pantex as well as on site at Pantex.

Then the fourth individual -- what was his experience?

MR. CHEW: He is a health physicist from Livermore, going down to
support a specific program.

MR. ROLFES: Okay.

MS. ROBERTSON-DEMERS: Did you talk to any of the field technicians?

MR. ROLFES: We have in the past, not in this particular set of interviews. We have definitely spoken with just about anyone and everyone that we could think of that might have had some kind of involvement in this incident.

MR. FITZGERALD: Just as a follow-up, you know, I knew (identifying information redacted), but was he in charge of health physics during the `89 incident, if you recall?

MR. ROLFES: I believe so.

MR. FITZGERALD: He actually managed the HP program. Where did (identifying information redacted) fit in, I'm a little sketchier on him.

MS. AL-NABULSI: Can we refrain
MR. ROLFES: Yes, I was going to say, we probably shouldn't --

MR. FITZGERALD: Oh, okay. I am sorry. It is in the documentation.

MS. LIN: But those are not PA-clear ed.

MR. FITZGERALD: The two individuals' names were presumably health physicists, but were they actually managing the Pantex health physics program?

MR. ROLFES: From the very beginning back in 1957, there was a group of about nine people that had been given radiation safety training.

MR. FITZGERALD: I know.

MR. HINNEFELD: What he just tasked about the two specific people -- and were they managing the health physics program.

MR. ROLFES: I didn't want to answer specifically about somebody's job. I
didn't think I was supposed to discuss --
Jenny?

MR. HINNEFELD: I don't know. Are we allowed to talk about --

MR. KATZ: They were working in their professional capacity.

MS. LIN: It really depends, because if it is someone that has a managerial position, then their privacy expectations are less than someone who is not.

MR. KATZ: A line worker.

MS. LIN: So I think we should --

MR. FITZGERALD: I consider this very pertinent to this particular -- because the interviews are the basis -- they are part of the basis for one of the key conclusions. So we certainly can go back to Energy, but I think who these people are is very relevant to the context of that information provided.

That is kind of the -- that would be the reasoning for wanting to know better on
DR. NETON: What type of information are you talking about?

MR. FITZGERALD: Well, let me just give you my general perspective on this. We discussed this issue at Mound as well, as you recall, on tritide.

I am concerned on statements from individuals who were in a management role for the operations and/or the health physics programs that these issues fall under, and I am not saying that from the standpoint of questioning their technical expertise, credibility.

I have known a lot of these folks, and they are the best HPs in the business, but from my experience, it is difficult to critique, in a sense, your own program that you were, in fact, responsible for. During the eighties and nineties -- and I think some of us share that time frame -- the department
and the health physics program in the department were going through just dramatic change, and there was a lot of realization that practices that were perfectly fine from the health physics standpoint, meaning that from dose control, and we all made judgments, what was important from a standpoint of dose, didn't necessarily pass muster with the rad-con manual or radiological controls in 54.11 and 835.

So rigor was imposed in the system, and that is what led to a lot of the changes in that very time frame that we are talking about here. I think it may be difficult to query the managers who were in place at that particular time, and sort of ask them about how the health physics program addressed issues as they arose or ask for judgments about the significance of this or that, simply because -- and again, simply because it was under their respective watch.
So I think there is an implication there. I am not saying that they would purposely say anything that way, but I think it is just that there is a tendency to not want to publicly acknowledge necessarily that it is something that maybe, because of the culture, because of the mindset, because of accepted practice for years and years that is the way business was done, and all of a sudden it wasn't the way it was done, and I think a lot of folks at that particular time were uneasy about how that reflected on their position as well as their professional judgment.

So I think that is the concern I have. I had it at some other sites, because I think it is a -- I have seen that many times. So that is the reason I want to raise this, that if we are asking less for objective information, more of a subjective "what do you think," or from your standpoint, you were in
charge, was it as bad as it seemed or -- I think we got to remember that the response is going to be from the vantage point of somebody who was responsible for either the operation or the actual health physics program that was in place.

DR. NETON: I completely understand what you are saying, and I think it has a lot of merit, but I think we can just look at what the objective evidence is that Mark just provided. It doesn't rely on people making statements about the quality of the program.

You have a situation where you have 300 bioassay samples that were taken at the termination, at the end of a five-year campaign to disassemble these weapons that are, I think, agreed by all that these were the potentially dirtiest, most contaminated weapons components out there.

This five-year campaign was, I
think, orders of magnitude higher in workload than any of the other previous years. Now you've got a situation where you have 300 bioassay samples on workers who had worked potentially five years with this material, and urine samples are a long term integrator of their exposure.

Now you take that integration, you take the 95th percentile of that, and you come up with 135 dpm per day intake. I think that is a pretty good scenario to bound what these workers were exposed to over that campaign.

MR. FITZGERALD: I am not speaking to that information.

DR. NETON: -- but that is what Mark was talking about, the bioassays.

MR. HINNEFELD: What the report says, Jim, is that the conclusion for W-28 units had the highest potential for exposure is based on information that you have got.

DR. NETON: But I think SC&A has
agreed to that. I mean, I read in their little statement that they agreed that W-28 is the highest contaminated component out there. I just read that in your write-up.

MR. FITZGERALD: The 28 -- let me step back. What I am speaking to is a broader background, including these interviews, that speak to the significance of what we are calling the incident -- I will get to that in a minute, but the '89 "incident," and I am going to use quotation marks on that -- as being the most significant one. Okay? Meaning the one that would be most notable from the standpoint of the contamination levels and the exposure potential and, therefore, being a good, if not the best, candidate for a bounding analysis.

DR. NETON: We are not doing that. We are taking 300 urine samples, projecting a chronic intake over a five-year campaign. That is all we are doing. We are not using
that incident to bound workers. We are
talking about a cumulative urinary excretion
of these workers at the end of a five-year
period, and how much could have been coming
out of their urine. How much could they have
breathed in, and be excreting that in their
urine at the end of a five-year campaign?

MR. FITZGERALD: Okay. So let me
just clarify, because this is a major change,
in a sense. I am just trying to make sure I
understand this then.

So whereas the Evaluation Report
certainly advances the '89 or maybe 1990
incident as the bounding bioassay results, you
are saying that that is no longer the case,
that you are not looking at '90 as necessarily
bounding. You are taking all the bioassay
samples, regardless of vintage, taking out the
firing pit, and that is going to be the basis
for assignment.

MR. ROLFES: Yes. All along in
our Site Profile, we have been relying upon this 1990 bioassay dataset, which was collected as a result of this 1989 incident. What Pantex did, they went back and identified any workers who was involved in the W-28 program that was on site, and they took bioassay samples from them. They were analyzed at the Y-12 facility.

In addition to that, they had brought in the Helgesen in vivo lung counter, and performed chest counts on these individuals.

We have always been using that bioassay dataset for dose reconstructions. Now in addition to that, we have gone back and analyzed some of the earlier bioassay data which was collected beginning in 1959, and we have incorporated that into our intakes and calculated the 95th percentile intake rate based upon those urinary uranium excretion results.
DR. NETON: The use of it is important itself. That is what drove them to collect the urine samples, but we are using the urine as indicator, which is not unusual. That is what we do very often.

MR. FITZGERALD: Now, going from that, you know, the ER, as I recall, when it advanced using the 1990 urine data as the bounding in this -- I guess that that is now not the case for the approach, the new approaches that take them all. Right?

It does make the case -- and of course, this could have been done from the get-go, but it does make the case that the reason -- I don't know if I have this actually cited here; maybe I do. The reason for the 1990 set of data being used is that it is the oldest set of data that provides isotopic determination of uranium alpha activity in urine samples -- this is a quote -- "and has significant data to perform statistical
analysis."

So, certainly, the background is that it has the data that is of quality and number, number of samples, to provide statistical validity. Then it goes on: "because it is the most comprehensive set of depleted uranium intake data found in the Pantex records" -- I don't think we would argue with that -- "and that it is of large, known high quality, and that the intakes from exposures are expected to be above normal operating exposures."

I guess my question is: that certainly points to -- and this is something that we have looked at as well, that there are earlier bioassay samples, but by virtue of the fact they are earlier ones, the number of those samples and the quality of those samples would not necessarily be the same as or approach the later ones.

I guess I would like to hear your
views about how to handle -- quality was raised in the ER as the reason for going with the 1990 set. Now that you are going back and taking those earlier datasets, why would that not be a problem or why would that not undercut the statistical validity of doing that?

DR. NETON: Well, what ended up happening, when you had the earlier bioassay samples, you ended up increasing the 95th percentile, but largely because of the detection issue. So it is claimant favorable to do that from bioassay samples, and there would be a larger value because of the detection limits which were with those sets of measurements.

MR. FITZGERALD: But I guess the part I am stumbling over a little bit, Jim, is that -- and I go back to what the ER originally said, is that what was attractive about the 1990 incident was the 305 data
points, that you get a lot of data points, whereas in complete operating history before that you maybe barely had 100 or something like that, and they were of questionable quality and some questions about even how they were taken, those kinds of issues. But now we are saying -- or you are saying that you can go ahead and blend those in, and it will be favorable, but I guess my question would be how can we know that the distribution is going to be a valid distribution when I think all of us would agree that you are not going to have captured perhaps the data points that existed.

You know, you have 305 from one incident, and you have 100 for 25 years. Clearly, there is a disparity from that standpoint.

DR. NETON: I would actually argue -- I think the 1990 cases are an issue. They were added in, I think, primarily because
people felt they were added in. The number goes up. It is claimant favorable. I didn't look at the analysis. The data were weighted based on the number available at that time period. So the '90 data essentially probably weighted -- it didn't weight it more heavily than earlier days.

Again, I go back to the 1990 data where you capture bioassays for 300 workers on a campaign. It was by far the largest campaign that disassembled this type of weapon in the history of the plant, for a five-year period starting in '84, ending in '89 -- orders of magnitude more, rather than the surveillance activities which occurred all preceding years. I don't know the numbers, but orders of magnitude less.

So now you've got a bioassay sample on a group of workers that worked on those weapons for a five-year period, and you've got their excretionary values, which is
an integrator of all of the exposures that occurred over that five-year period, and it looked pretty good as a method to bound what these workers were breathing in over that operation.

MR. FITZGERALD: But how do you know? How do you know? This is the question that we have kicked around for over a year. How do you know that this 19 -- or even three or four or five-year campaign, or even within the five-year campaign, that these results are, in fact, bounding? I mean, it seems like we come down to why are these 300 samples --

DR. NETON: Because if the workers were working with the weapons and at the end of five years you take a urine sample, and you say what could that person have breathed in over that five-year period and still have his urine below that value, that is what we are doing.

We are saying what possibly could
have this person breathed in over five years
and still be excreting x, in the 95th
percentile, at that time point? You can't pee
out anymore than you breathed in.

MR. FITZGERALD: Yes, I understand
that, but I am just saying how do you know --

DR. NETON: You have 300 workers.

MR. FITZGERALD: How do you know --
yes, you have 300 workers, because it is
1990 or '89, and the management woke up. The
workers complained, and you had literally the
kind of scrub that happens when you have that
kind of change. But I am just saying how do
you know -- and this again comes back to what
we have been talking about.

How do you know that the exposures
before that were, in fact, less than or the
same as the ones for which you have resolved
in the '90 incident?

DR. NETON: It has evolved in a
sort of a weight of the evidence argument. I
think that the bioassay data are very small. Now you take what we are assigning as an intake, which ends up being 135 picocuries per day, if it is a Type S material.

That equates to roughly 14 dpm per cubic meter of uranium in the air for every hour this person worked over that period. Then you go back and look at other operations involving uranium. You can go back and look at Kingsley and Harrison, all these other operations. What kind of airborne do you get from handling derbies of uranium?

In any operation that doesn't involve abrasive activities such as grinding, welding, cutting, sort of shaping operations, you get less than 20 dpm per cubic meter in the air. Matter of fact, most often the daily rate average is less than 10 for those types of operations.

That is consistent with what we are applying here. It is hard for me to
fathom that, for a person doing an inspection, pulling out whatever they are pulling out, examining it, writing down a number, whatever, putting it back, that you can generate more than 14 dpm of uranium per cubic meter. It is sort of a what are you doing kind of thing.

Well, I don't know how you can sort of get this idea where you are going to get higher than that.

The other thing is, if you look at the lung counts with the Helgesen full body counter, they are determined to be biased high because of certain background correction issues.

So the fact is, if you assign this 12 dpm -- or 14 dpm per cubic meter breathing rate over a five-year period, at the end of that period a person would have accumulated somewhere around 12 milligrams of uranium in his lungs. That bounds all the Helgesen whole body count data, not ridiculously implausibly
bounds, but it is higher than about a factor of two than the highest person that was measured with the Helgesen full body count.

That gives me some comfort, too.

MR. FITZGERALD: Yes, but we are still talking -- I understand the comparisons within this late eighties into the '90, whether it is the in vivo counting or in vitro counting. You have a contemporary picture, which I don't have any problems with the contemporary picture.

I am just trying to go back to the 25 years -- well, it is almost 30 years, but before that and say, does that picture basically take care of the handling before that, and can we rely on this being the worst case?

DR. NETON: The same weapons, as I understand it, are being inspected. There is a potential for oxidation to develop over time, to indicate that have more loose
contamination in the earlier period. Again, I get to the campaign. The workload -- if your workload is 100 times less over that entire 25-year period than we did in five years, I am having trouble figuring out why it is not bounding.

If I do 1,000 of something on the same issue all the time, every day, and then for the previous 25 years I do hundreds of that, where is the increase in release rate, the source term? I'm missing that.

CHAIRMAN CLAWSON: Because the processes have changed.

DR. NETON: Processes changed.

CHAIRMAN CLAWSON: Processes changed in how you are going to do it. In 1980, you saw a drastic change in procedures and how you were doing things. In the earlier years, you did not have any of that.

So what you are saying totally goes out the window. That is that, if 1989
back to 1957, it was all done the same way, with the same procedures, the same processes and everything else, then that could hold. But in that time frame from 1958 onward, processes changed, areas changed, how they did it changed. Everything changed.

DR. NETON: What changed? Or is that is not allowed.

CHAIRMAN CLAWSON: Oh, no, we can talk about changes, but --

DR. NETON: I'm open to that.

CHAIRMAN CLAWSON: Just take, for instance, how many weapons would be able to be allowed in there, which that changed over the years. How the processes go, and even after '89 era and going into the '90 era, that is when they started bringing in fume hoods and everything else like that. They started moving air different. They changed the cell diagrams. They changed the air flows on it. They changed the sampling programs on it.
They changed everything, 1985 up to --

DR. NETON: Well, what you are talking about are sort of -- the air controls, I can understand, if they did something special with air capture maybe, but sampling and stuff, I just don't see that.

The process change, to me, is something different. Now you are taking -- rather than just pulling it out, looking at it, and doing it, putting it back, whether you are abrading it, you are grinding it. You have to have some way to generate airborne activity. Without that, I have trouble understanding why you are going to get large quantities of airborne uranium, especially since this particular sample in 1989 they had, and it was less than one percent uranium. It was mostly organic material, primarily two percent lead.

MS. ROBERTSON-DEMERS: This is Kathy. Can I ask a couple of other questions?
First of all, where -- and, Isaf, please stop me if there is a problem with these questions.

MR. KATZ: Don't ask a question that you have any doubts about, Kathy.

DR. NETON: Why are you asking me? I don't have a clearance.

MR. KATZ: No, but I am just saying, Kathy can't ask -- when in doubt, don't ask.

MS. ROBERTSON-DEMERS: Well, let me see. I think I can ask a yes or no question. Was there only one mod of the 28?

MR. ROLFES: We will delay our response, but --

MS. ROBERTSON-DEMERS: Okay.

MR. ROLFES: We can answer that pretty openly. There were different mods of the W-28, and you know, the warhead was actually used in several different platforms, I guess, delivery platforms.

MS. ROBERTSON-DEMERS: Okay. Also
CHAIRMAN CLAWSON: So there were differences in how they were handled.

DR. NETON: Handling them how? I mean, it is pulling them out -- I understand that W-28 is the potential worst potential for contamination, because it is not an alloy.

MR. HINNEFELD: There were other non-alloyed. There were other non-alloyed weapons.

MR. FITZGERALD: But the W-28 was uncased, which makes it a particular problem.

DR. NETON: It seems to me there is agreement that W-28 is -- and there was a five-year campaign that disassembled it.

MR. FITZGERALD: Well, let me touch on that a little bit, though. I don't think I would disagree that the W-28 sort of stands out. It was an uncased DU design, and because it was uncased, the raw uranium would oxidize almost immediately once it was in the
air, and that would accelerate depending on environmental conditions, as Mark pointed out. But the one thing -- and I would invite you to look at the interview we did do, and you can certainly interview this individual again, but we wanted to talk to somebody that was intimately familiar with sort of the operational aspects, not just health physics but the operational aspects of the W-28.

It is pretty clear, you know, this wasn't sort of an episodic -- you had certain campaigns, and you had perhaps releases. You had unusual occurrences, maybe incidences, but this exposure got out.

In fact, it was a continuous process of not only assembly but retrofits, redesigns, surveillance -- trying to think of some of the other words -- dismantlement, modifications, and anytime you actually went into the system, because this was uncased DU there was a potential for exposure.
Now unclear how much, but it was pretty clear from the interview that, as time went on, this was pretty prevalent in terms of just having to deal with the fact that you were essentially going to have this get out, and you would be covered with this material.

Unfortunately, it is a very subjective thing, because --

DR. NETON: But the material, though -- you can have a lot of material, but not all of it is uranium.

MR. FITZGERALD: Well, that is another issue that we probed when we were at the site, and --

DR. NETON: Well, I looked at the analysis of the material.

MR. FITZGERALD: Yes, I did, too, the spectral analysis. I included it in my memo. I guess we have a point of disagreement on that, because I think the results that we looked at said it was predominantly uranium,
and there was lead and other -- cadmium and other materials that were also present. So we can go back and look at that, but --

DR. NETON: Less than one percent.

MR. FITZGERALD: Apart from that, because we are not talking about the -- talking about the amount of --

DR. NETON: See, that is worker perceptions, how much material was being spread around, and black material is not all uranium.

MR. FITZGERALD: I know, but I don't want to go down that path, Jim, because this is not about the size of the source term.

I think we all agree there was an exposure potential from DU, and whether it was one percent, five percent, 20 percent or 30 percent, the question is can one come up with a means to dose reconstruct?

So I am just saying, though, that certainly by virtue of visible signs, there
was, in fact, observable exposure potential from the DU, from the uncased uranium that was in the W-28 throughout this history of dismantlement.

Now dismantlement wasn't just for retirement. That is the point I am trying to make. Dismantlement was for retirement, surely, mostly toward the end, obviously, but also for retrofits, modifications, and in some cases the surveillance that was necessary. You had to, in fact, go in there, and you were, in fact, potentially exposed.

So that occurred throughout the life history. Now I will grant you that it was accelerated when they retired, dismantled and retired. But I want to make sure it is clear that there was dismantlement throughout many of these years, and that was confirmed at the site, that you had to do that.

MR. ROLFES: We all know that there was an earlier disassembly and
inspection. However, the numbers of disassembly and inspections were very, very small in comparison to the dismantlement effort that we are using as our basis to assign intakes.

We are using the 1984-1989 data, which is basically the worst case scenario, because those weapons have been in the stockpile for the longest and have been potentially corroding for the longest amount of time, over an approximately 30-year period.

MR. FITZGERALD: I am just coming back to the point, though, that we have this wealth of data for this one point in time, and we have various pieces of data for the previous 25 years covering this operating history of dismantlement that went on during that time frame.

I don't want there to be a perspective that, you know, these things sort of stayed sealed in the system and were
untouched. In fact, there was a lot of retrofits going on, a lot of modifications going on, and the system was very versatile.

So it went in and out of Pantex for a long time. In fact, one thing that we found and we are told that there were a number of workers who spent most of their careers working on the W-28 line, so to speak, in terms of doing these various procedures. They were most familiar with that particular system. They stayed with that particular system their entire career.

The other thing I want to raise -- and I raised this the last time, because it is the source of the confusion, I think, and we had a conversation that Stu was involved with. I am trying to reconcile the point that was made in the TBD for internal dosimetry where there was a comparison of mean uranium intakes. I think you know what I mean.

I think that was something that
you were going to come back with and sort of
give us some perspective. Maybe I missed it,
but I didn't see it in the analysis, but I
guess I am still wondering about that comment.

It is the comparison of the mean
uranium intakes for '66 to '79 versus '80 to
'90, and it is the .375 d per m per day versus
the .188 d per m per day. Just trying to
understand if one took the earlier excretion,
mean excretion rates, compared them with the
later ones, it appears -- now the TBD notes
that these were pretty close.

On the other hand, one is a factor
of two higher than the other. So I just
wondered if you were able to find out more
about that.

MR. ROLFES: Right. That was a
valid point. So what we did to address that,
we went back and looked at that earlier
bioassay data, in addition to those 300 urine
samples that were collected, and that is what
we are proposing to use now. We have calculated the 95th percentile intake rate from roughly 400 uranium urinalyses from 1959 up to 1990.

MR. FITZGERALD: So, basically -- I am just trying to understand. Basically, this is in terms of a mean uranium intake for that period, those are valid numbers, because I haven't actually crunched all the data. I am assuming that is a valid number.

That is kind of where I am going back to what we talked about earlier. We can talk about people's recollections and the weight of evidence, but one difficulty and challenge for this topic, when you get before 1990, is a lot of it is we get into weight of evidence.

This one, actually, I thought, was -- in terms of the issue, was pretty relevant, meaning that it sort of compares the 1990s data that was in the ER with the previous
data, and again it just appears as a contrast.

DR. NETON: You take and assign all workers an excretion rate that is equal to -- greater than 95 percent of all the samples that were taken of the 400. So you take the 95th percentile of 400 samples and say everybody is excreting that -- is assigned that excretion rate.

MR. FITZGERALD: And that is --

Maybe I am backing into what I thought we have already discussed. Excuse me, if I am, but that is where, I guess, I am thinking about statistics. I am just thinking that, again, we know we don't have a lot of data from the early years, and that what data we do have you are blending in, but it clearly suggests that the data from the early years is higher, much higher than the nineties data, and it seems to take a 95th percentile of what you got, which is these samples over the 20-25 years here and there, because they just didn't do that much
sampling, and the 305 high quality ones from 1990. You put them together in a pot, and you basically say I am taking the 95th percentile. It just seems to me that the data is going to be skewed, if your distribution, even with the 95th percentile, may not be representative of the earlier time.

DR. NETON: Take 400 samples at the site during this long time period, and largely many of those were incident-based, we are saying. You take the 95th percentile of those. It is hard for us to imagine that anyone was chronically exposed at a level that would be greater than the 95th percentile excretion rate. It is hard to fathom that that would be the case.

MR. FITZGERALD: Well, that is what I am trying to get to. It seems to me, this is a -- it is a judgment call. It is hard to fathom versus actually having a representative set of data from the pre-1990.
We are doing the best we can. We got what we got. You got the data that you have to live with. I am just saying that it seems to me that we are proposing that we think the data points that we do have from the early years, understanding that they seem to be much higher than the ones in 1990 --

DR. NETON: I would have to go back and look at --

MR. FITZGERALD: -- it's a factor of two.

MR. ROLFES: Just for one specific laboratory, it was a factor of two higher intakes based upon the bioassay data -- would be a factor of two higher, and I think it was largely based upon the laboratory who conducted the bioassay and that analyses.

For example, in 1959, Los Alamos National Laboratory actually had a pretty low -- they were reporting less than -- I think it was around one microgram per liter. Some of
the independent laboratories might have been reporting 5 micrograms per liter as their minimum detectable amount of uranium in urine. So it was contingent upon which laboratory or commercial lab did the analyses.

MR. FITZGERALD: Mark, let me just finish this. I understand that. I think you do the best you can, but in terms of objective data, not subjective data or weight of evidence data, but objective evidence or data, I think this is the hardest we have as far as contrasting the information from 1990 with the information from the earlier years and trying to figure out whether it is feasible to use the data that we do have and how we use it.

I think the first judgment that was made in the ER was, does earlier data have really enough of it necessarily, and it is questionable quality, but we have these 305 gold-plated bioassay samples from 1990, and we have a lot of confidence in that information
and, oh, by the way, because this was a final 
dismantlement campaign, say, so to speak, from 
28, that we think that would be the number to 
bound.

Now we are backing off a little 
bit from that and saying, okay, why don't we 
take in all this data. I am trying to 
reconcile the original position with the new 
position, saying, okay, I see where you are 
going. And, certainly, we raised this 
question about the earlier data, but I am 
still having difficulty with wrapping that 
earlier data with all those qualifiers and 
throwing it into a distribution, taking a 95th 
percentile, and feeling confident that it is 
going to not miss the data that wasn't 
collected from the earlier years and it is 
making the best stab at coming up with a 
conservative value, but it is not getting past 
the fact that you don't have good data from 
the earlier years. You do have data, but it
is not good data, which essentially was the context of what the ER said.

That is where I have a problem with the approach. I think it is a better approach, quite frankly, than just hanging your hat on '90 and saying, you know, this is the bounding, end of story. But I still think the statistics and the quality of the data that is part of this analysis is still questionable, particularly given this observation in the TBD that you have -- you know, granted, laboratories sometimes do slightly different analyses, but these are National Labs.

So I don't think they are going to be tremendously off, but you have a factor of two, a factor of two difference between the sixties and 1990. It is not 10 percent, 20 percent. It is a factor of two.

MR. ROLFES: The same lab can get a factor of two difference in a sample on a
day to day basis.

DR. NETON: I, frankly, am not familiar with how some of that data looked in the earlier time frame, whether it is based on different detection limits that are driving that factor. So in fact, I guess I can't.

DR. MAURO: This is John. Can I jump in a little bit, just to collect my thoughts and the way in which this is being described. It might be helpful for me, but it may be helpful for others.

Can everyone hear me okay?

MR. KATZ: We hear you perfectly.

DR. MAURO: What I am hearing, Jim and Joe, is that you really have two strategies that are being entertained. One strategy is you have got a collection of very good data in the later years, the '90s, and the big question is, okay, can we use that data somehow to bound exposures that may have occurred decades before.
One line of argument, which in theory -- and, of course, these are where the judgments come in -- could be used is that, well, you have got the following weight of evidence that said, yes, you can do that. One is the process knowledge. That is your knowledge of what went on in the past may not be substantively different than what was going on in the '90s.

Certainly, Brad has pointed out, well, there were differences, and the degree to which those differences are important, of course, need to be aired, but that would be like level one. Okay, process knowledge arguments.

The second one that I heard is that there are also what I call the Adley arguments. This is that special study that was done where lots and lots of data were collected regarding uranium airborne dust loadings for a whole variety of different
operations, and one could argue that, okay, if you look at the Adley data and all the different kinds of things that were done, just about everything you could think of, and if somehow you could say, well, the kinds of things that took place in the early years somehow fit into the kinds of things that Adley describes as operation type X as being reasonably representative.

So in that regard, you sort of have a surrogate data line of argument that is separate from the process to actually look at as a little bit different than the process knowledge.

Then the third one is you have got some early measurements that you are really not that comfortable with, but they are there, and those measurements somehow could be, okay, if I have got those measurements, are they compatible, consistent with -- not incorporating or blending them into your 1990s
data, but say, well, let me ask a question a
different way.

Here is my 1990s data. Is there
anything about the earlier data that tells me
that the 1990 data does not bound it or those
datasets are incompatible, just doesn't make
sense?

So what I am hearing is, if you
use what I call the non-blended approach that
I guess you originally used, and you are
making your case saying that the non-blended
approach -- go with the 95th percentile for
the 1990 data, and the way you validate that
for extrapolation back in time is through
compelling arguments along the lines of the
process knowledge, the Adley data, and the use
of the earlier measurements together.

If together those argue favorably
that, yes, the 1990 data are bounding, I think
you will have made a strong case. It sounds
like there are some questions regarding that.
Now finally, and I will make one more statement, the alternative approach is the blended approach, and it sounds like that is something new. I would have to say that the blended approach is -- my first reaction to it is that, if you have questionable data about its representativeness, quality, completeness and that sort of thing, and to blend that into your 1990 data, I think that is actually trying to mix two sets of data that may not be compatible, and actually hurt your distribution and the validity of the approach.

So my sense right now from listening to this interesting story is that the unblended approach coupled up with an argument that you can make your case on process knowledge, Adley, and early measurements, that would be the line of approach that could be the most compelling, whether it will hold up or not.
What I am hearing is there is some question regarding whether or not you have those three levels of weight of evidence to support your position.

The way I just characterized it, is that a fair representation of where the issues lie?

DR. NETON: I think so, John, and I would say that I am in pretty much complete agreement with you.

MR. FITZGERALD: Let me clarify, though, that he just said that the blended approach would --

DR. NETON: No, I agree. I have problems with the blended approach as well.

MR. FITZGERALD: Okay.

DR. NETON: I think it is a more compelling argument to take the 300 samples that you have in a distribution and ascertain what the chronic exposure could have been over that five-year campaign with a much higher
workload and a higher potential for oxidation, and now you can go back and look at the earlier years -- and John exactly said the right thing.

First of all, does that make sense, given what I know about -- the intakes that we are prescribing based on that approach, does that make sense, given what we know about the vast knowledge of people handling uranium itself?

We have a lot of knowledge of people handling uranium pieces, derbies, dingots, grinding, shaping, welding. If you go back at the Kingsley and Harrison or the Adley documents and it is consistent with that in that right same range, then you have a good feeling.

The only remaining piece then is, as Brad pointed out, are there significant process differences that would make that not valid for extrapolation.
MR. FITZGERALD: For the record, I am kind of confused now, because what you are saying basically is that the current proposal of blending, you would agree, may not be the way to go, but maybe go back to the original - - use the 1990s data, but along the lines of what John has outlined, make a stronger case for the continuity of operations and those kinds of arguments.

Okay, we will go back to --

DR. NETON: We are open for discussion.

MR. FITZGERALD: I know, I know. I am just trying to figure out where I am standing, because --

DR. NETON: You have heard what I think.

MR. FITZGERALD: Right. I am just saying, though, that that takes us back to the discussion we had in Germantown, which is on the 1990s data being bounding as well -- well,
bounding, because it would then envelope previous exposure potentials and -- okay.

Well, all right.

DR. NETON: Now that I have heard about the campaign and the magnitude of the workload, I feel very comfortable with that bounding unless there is something else I can hear that convinces me that they were doing something substantially different with these inspections or disassemblies in the earlier years that would generate more airborne than--

MR. FITZGERALD: That where I guess I still have to reconcile -- we don't have very much hard data to go with on the early years versus '90, but we just talked about the fact that what data we do have -- and it is highlighted in the internal dosimetry TBD. The early data is a factor of two -- the mean is a factor of two higher than the 1990s data.

I know you are shaking your head,
but I guess I just have trouble, and there
isn't that much that you can use to contrast.

DR. NETON: We would have to go
back and look at the distributions. I haven't
looked at those for a while. I apologize.

MEMBER SCHOFIELD: I have a
problem with that, too, because here is my
catch. We keep talking about airborne
contamination. We all know workers scratch
their heads, wipe the sweat away, things like
this. So you have this transfer of material
that is not necessarily airborne.

DR. NETON: Well, Phil, that is
true, but our model incorporates an ingestion
component as well. The ingestion is in there.
The fact is that a dose from ingestion is
much, much smaller than any dose that would
accrue via inhalation.

MEMBER SCHOFIELD: I would like to
know what basis you can say that, because you
could have both inhalation at that point, and
you could have ingestion, too. It could enter both pathways into the body.

DR. NETON: Well, we do, Phil, and that is accounted for in the current approach, but when you ingest something, a small percentage of it is absorbed into the body. Most of it goes out the other end. When you inhale it, it is directly deposited in the lung, and that is what ends up giving you the larger component of dose. It is not ignored. It is included, but it is in there.

MS. RAY: This is Sara Ray. Can I ask a question?

MR. KATZ: Go ahead, Sara.

MS. RAY: Are you taking into consideration the ongoing practice that is well documented of burning the depleted uranium? The fire department did it for training purposes, and many, many items were contained in the materials that were burned in open pits, and this was dispersed into the
air.

MR. ROLFES: Sara, this is Mark Rolfes. What we are trying to focus on right now is specific to the assembly and disassembly operations conducted in the plant. In addition to the bioassay data that were collected at the site historically, we also have bioassay data from, for example, the firing sites and the burn grounds, which were not included in this specific analysis or discussion, but those data are considered separately.

MS. RAY: Bioassay was not done in the a timely manner on the Cell 1 incident. That is fairly common knowledge.

MR. ROLFES: Sure. Yes, there was a delay. We are aware there was a delay in between the collection of bioassay data following that 1989 incident. Well, you are referring to Cell 1. So you are referring to the tritium release, which is different than

75
the depleted uranium chronic exposures that we are referring to.

MS. RAY: That was also a common practice, was the delay, because there were so few people in the safety department, and the operations were basically 24/7 during the early years, especially during the war years.

MR. ROLFES: Since we know the date that this incident occurred, and we have the date that the bioassays were collected, we consider that in the dose reconstruction process; and if we don't know the date, for example, we can take a mid-point between a previous bioassay and the most recent bioassay result and usually use the mid-point between those two to estimate a reasonable and claimant-favorable intake to assign internal dose.

MS. RAY: Oh, you are using the earlier, questionable data, from what you are saying.
MR. ROLFES: I'm sorry, Sara Ray. Could you please repeat what you said? I didn't catch that.

MS. RAY: Then you were saying that you take an earlier point, a mid-point, and a later point, and you are trying to come up with a good figure using these. I was just saying you are using the earlier data that is questionable.

MR. ROLFES: Well, this is a common practice in internal dosimetry. If there is an incident that you don't know the date of, and you have bioassay data that was collected prior to the incident and bioassay data that was collected after the incident, we actually would use -- if we didn't know the date that the incident occurred, we would use the most claimant-favorable incident date to interpret that bioassay data to assign the highest possible and claimant-favorable internal dose in our dose reconstruction.
process.

MS. ROBERTSON-DEMERS: This is Kathy. I have a similar question along that line. Do you have a solubility determination for this uranium?

MR. ROLFES: Most uranium, if you take a look at uranium metal, it is usually Type M with a little portion of Type S. We usually assume the most claimant-favorable solubility factor based upon the target organ in our dose reconstruction.

So if we have a lung cancer case that we are reconstructing a dose for, we would assume that the uranium is insoluble and that it resides in the lungs a longer period of time and results in a higher internal dose.

If it is a systemic organ, we would choose something that is more soluble, because it would deliver more dose to systemic organs.

MS. ROBERTSON-DEMERS: So we are
talking about bioassay collected one year after the incident, and we are talking about solubility Class M.

MR. ROLFES: What we are talking about is the same as what we have had in our Site Profile for the past several years. We have bioassay data that was collected after a 1989 incident which would essentially bound any potential exposure incurred by employees for the five-year operation from 1984 through 1989.

DR. NETON: Actually, both solubility classes or types were modeled, and that is what can see in our report, Type M and Type S.

MS. ROBERTSON-DEMERS: I guess my concern is what is left in the urine after you have waited a year to take the bioassay sample?

MR. HINNEFELD: This is Stu -- what is in the urine a year after the
exposure, regardless of solubility class -- well, particularly for more soluble types, intermediate and soluble -- is the uranium that is being decorporated from where it deposited initially.

DR. NETON: But if you have inhaled this material for five years and it deposited in your kidney and your skeleton and other depositional loci --

MS. ROBERTSON-DEMERS: We have had this discussion before where there was a delay in the bioassay sampling, and in order to see the detection level at that time that it was taken, you would have had to have taken in a lot more for, say, Type M than Type S.

DR. NETON: Correct. That is factored into the calculations. These are modeled based on the standard ICRP metabolic models, and that is accounted for.

MEMBER BEACH: Well, models aside, I am still having trouble with the lack of
bioassay data in the earlier years.

DR. NETON: But again, I wish someone could give me an idea why the process campaign in the five-year period was orders of magnitude more workload of the same weapons than what happened in the preceding years, why that would not be a bounding scenario. What happened differently to generate more airborne on a daily basis than what would have been observed in the five-year campaign?

MR. FITZGERALD: At the risk of being repetitive, my answer would be it is all subjective. You know, we can talk to people and ask them their opinion as to the 30 years of -- you are talking 30 years, three decades of handling of this system and trying to get them involved in more contemporary -- at the very beginning. I was trying to find people that could at least remember talking to somebody who did do the early systems, but we are trying to say that over 30 years, from
1960 to 1990, the operations were normalized such that you could backfit these values. We have been looking for objective information. I mean all of us have, actually. I am looking at this as sort of a search for something that would give this a hard edge, other than the fact that we have to rely on people's recollections and judgments and what have you.

Again, I tend to find the only thing that I can hang a hat on is the values that were cited in the NIOSH TBD where, quite apart from whether we think they were the same or quite apart from whether you can't imagine that they would be different, the mean excretion rates for workers handling the W-28 in the early years was double the mean excretion rates of the workers that handled it in 1990. I can't get past that. Everything else is conjecture on our part to say could they have handled it
that much differently or not. Now I guess I am with Brad from the standpoint that over 30 years, the radiological controls improved to the extent that -- you know, they were vacuuming up the cells a little better, and weren't letting things get too crapped up, and there was that degree of diligence, but there wasn't really a formal program until the overhaul in 1990. But there was an evolution where things got slightly better over time, as there is in any plant. We are talking 30 years.

So if it was five or 10 years, I would have less of a problem saying that there was a reasonable chance the operations were much the same, rad controls were similar, and you could confidently apply that. But 30 years? That is almost the entire operating history of the plant up to that point, and we are trying to make that assumption in the absence of any objective evidence.
The only objective evidence we have points to a factor of two difference in what seems to be an indicator of exposure. That is the inescapable point.

When I look at what is objective and what is subjective, the objective evidence is that mean average, and I don't think trying to apply a 95th percentile is going to do you any good, because you are looking at data that is incomplete at best.

Yes, back in the early days they certainly had no systemic bioassay program. So we are assuming they kind of did bioassays on the worst case. If they had a release of some sort or somebody thought it looked pretty messy in the cell, they did a bioassay sample, but there was no regime there. It was a judgment call, not necessarily even by an HP.

So how can we even know that a 95th percentile of that hodge-podge back then would give you any reasonable assessment of
conditions that would be comparable? To me, it is apples and oranges. You are trying to apply a statistical test of data that is small, to begin with, and incomplete to a set of data that we know is pretty darn good up front.

So I am just saying, I don't know where you go with that.

MEMBER BEACH: I was going to say, Joe, isn't it true interview notes say -- you interviewed an engineer. He said '89 wasn't even the worst case. There was worse prior to that.

MR. FITZGERALD: You know, we wanted to raise that question. Irregardless of the answer, I think, if this decision comes down to trying to get people to make a judgment call of was this worse or was this worse or did you think the one before that was worse, to me, it doesn't matter if you are asking people to say, you know, 20 years ago
was that '89 worse than all the ones that came before?

DR. NETON: How are we going to know whether '89 was the worst? We have already talked about that.

MR. FITZGERALD: No, but I am just saying that, you know, Josie raises the question. I am just saying that I don't think that part of it -- maybe we are in agreement here -- that part of it matters, because we are asking people to recollect and make a judgment or a value call which is completely nontechnical. Was it the worst? Who knows?

DR. NETON: The real question is, is the '84-'89 work conditions and the subsequent bioassay samples bounding of the previous years? That is the question. That is the question, not whether one is worse than the others.

MR. FITZGERALD: But, you know, we are using the values in '90 for that five-year
DR. NETON: That is all we are saying.

MR. FITZGERALD: That is right, and I am just saying that, in doing so -- and you are couching it a little differently -- that we are still saying that the values from that five-year period would be bounding, because we can't imagine that the exposure potential would have been higher than that reflected in those bioassays.

DR. NETON: For various reasons.

MR. FITZGERALD: Yes, and I am just saying that that is a weight of evidence value judgment. I think we said that earlier, and I am trying to reconcile that with the only objective information that I have, which is these values we keep going back to. I am just making the case that I don't think doing a statistical analysis of the early data to compare it with the more contemporary data is
going to demonstrate anything. We already
know that data is incomplete and --

DR. NETON: You are back to the
other aspect, which is what makes sense on an
empirical basis of what we know about handling
uranium. Can you generate more than 14 dpm
per cubic meter on a continuous basis from
this operation, knowing we have a lot of data
about experience with people dealing with
uranium in nonventilated situations, whether
they are inspecting something, they are just
moving it or shaping it.

You have sort of a lot of
empirical data out there. It gives you a
sense that you can only get so much in the
air, doing what we know was done on this
project.

MR. FITZGERALD: I would --

DR. NETON: And that binds up
nicely -- matches up nicely with the
urinalysis data.
MR. FITZGERALD: I have two problems with that. One, I think from NIOSH's own hierarchy, before we turn to modeling or modeling assumptions, I think we have to look at what hard data exists. I think we have mean excretion rates for both periods of time in question.

So, actually, you know, it is the only actual -- you call it empirical -- empirical information we have. I think that has got to take precedence over modeling. That is one issue.

DR. NETON: We are not modeling. We are validating.

MR. FITZGERALD: Well, we are using a set of assumptions based on, whether it is Adley -- we are using those assumptions. I am just saying that we have actual measurements that were taken at the time.

The second thing I want to raise: we talked about contamination surveys and air
sampling, but I want to make sure that the Work Group is reminded of a review -- and this is on the SRDB, and I know you guys are familiar with this -- by the Albuquerque Operations Office.

They were rather chastened by the Tiger Team when they went through earlier and found a number of serious issues with the internal program, but as you know, Tiger Teams were compliance based. So they didn't go into the actual practices as deep. So Albuquerque ordered up a follow-on evaluation focused specifically on the internal dosimetry program and how it stood up with accepted practice.

I want to read just these findings. I want to make sure we are reminded of how things were from the air monitoring standpoint as well as contamination survey standpoint. This is a review that took place October 30th to November 3, 1989, so very much in the same time frame, reflecting backward on
existing practice.

"First, no fixed air sampling stations. Second, air monitoring system used as a remote alarming monitoring" -- it is a RAM system -- "and not for quantifying air concentration."

So RAMs were designed to detect accidental releases and not breathing zone air, never intended to be representative of air sample, never intended to be used to assess dose. It was simply an alarming system.

"There were, and are, no areas controlled for potential airborne or surface contamination except in the 12-44 Cell 1 where the tritium incident occurred. No self-monitoring and no chronic low level airborne contamination monitoring. Self-monitoring was not performed except at the waste compaction operation in the B28 disassembly, and the operating and inspection standard for B28 was
not specific as to who performed personal monitoring" -- this is what Albuquerque says -- "or even if it is performed." So there was some question as to whether or not there was even monitoring performed.

"Use of nose swipes or special surveys were at the discretion of the radiation protection technician with little written guidance from written procedures. Evidence that this system was not adequate is most obvious in a recent DU contamination incident" -- which is the one that we have been talking about. "This disassembly program resulting in this contamination was in progress for several years" -- as we know -- "before a full assessment of internal exposure potential was initiated. The current contamination monitoring program is not adequate in type and frequency of swipes and surveys to assess workplace contamination."

"Disassembly workers observed
visible airborne black dust during the years of routine operations before the incident."

The reason I am raising this is that there was not a reliable contamination survey program at the site. The air monitoring results were based on data from the RAMs themselves, and I think that was taken into question by the site.

The cautionary note on using a lot of this data pre-1990, before they actually overhauled all these programs, is to keep in mind that they were collected in a way which, even at the time, the Albuquerque HPs and the consultants from the labs that helped the review found wanting.

That is the only context I would provide on that one, is that I think we got to be careful if we tag the air monitoring results, survey results, and the contamination survey results too religiously in this case, because at the time they were found to be
inadequate in terms of doing the job.

Just as a backdrop -- I read this in your analysis. The thing I keep going back to is I am not sure if you have qualified that so-called corroboration -- you are corroborating, I think, some of the data -- with the fact that the programs under which that data was collected were wholly deficient as determined by Albuquerque in their review at the time; because this was surfaced by the Tiger Team. They felt they had to go in and look at it, and they found it to be completely inadequate. These are the findings. We have the review.

I just want to make sure. It is not every piece of data, but I am just saying that in terms of the programs that were in place to collect the data, I want to make sure that that context is clear.

MR. ROLFES: I think you made a couple of points there, and I will try to
address them. The use of the data that was collected in 1990 represents chronic exposures that would have occurred over those past five years. We have that data.

So it becomes a matter of interpretation of the data how we completed dose reconstruction, not if we completed dose reconstruction.

Regarding the collection of data, yes, there are certainly fewer bioassay results in the earlier years. The first year that they started sampling people for uranium exposure was in 1959. I think there were roughly about 12 or 15 individuals that were sampled, maybe 10. I would have to look back.

Then we have some additional data again in 1961. It is about every two years we have a group of bioassay data that is collected.

To get back to what you had said about not having a reliable contamination
survey program really bothered me, just because of some of the things. Now I am saying there may be exceptions to this, but we recently came across this memo from 1959. It is from Mason & Hanger-Silas Mason, basically the people operating the Pantex plant.

They sent some correspondence to the Y-12 plant, basically describing that, basically, upon receipt everything that they received is placed on brown paper and swiped. Swipes are taken over the entire container and the components themselves.

Basically, Pantex has stated, when positive swipes were obtained and verified, the container is cleaned; usually a deep Kleenex is sufficient -- a damp Kleenex, excuse me. You can barely read the writing here.

There is an important point here. It says, "We are committed to the Army Ordnance Department to allow no detectable,
removable contamination into the assembly area." So, basically, in 1959 there is data here showing the results of the surveys for shipments to the Pantex Plant, and also a pretty bold statement that says they have committed to the Army that they will allow no detectable, removable contamination into the assembly cell.

So, basically, when components are received on site, they are surveyed. If anyone needs to clean them, it would be people that are trained in radiation safety. Those components would be placed in storage or released to assembly as a clean component.

So work would be done to assemble that weapon, and then that weapon would be sent out to the military or put into the stockpile.

Really, you know, the components may have some removable contamination. There are survey results showing 200 dpm removable
of 1197 dpm. They range from 200 to 1500 dpm here. But that --

MR. FITZGERALD: Mark, can I stop you right there? Yes, I know we have had this discussion before.

MR. ROLFES: This is new.

MR. FITZGERALD: Well, my point is, absolutely, Pantex had a rigorous contamination control program on the assembly side. In fact, they had one mishap, which I won't get into, where some contamination crept in, and it was a big deal, and they actually really had to clamp down even further.

So I don't have any argument that the rigor of the contamination control program on the assembly side was there. You could not deliver to the customer something that was even slightly contaminated in Mel's lab, because that would be not good.

On the disassembly side, they
shared a blind spot -- I actually talked to Mel about this at breakfast -- a blind spot for depleted uranium that most of the complex had. It was no fault of anybody. It just was, from a dose standpoint, DU just didn't figure very prominently compared with everything else, and wherever low-enriched or depleted uranium was handled in the complex, it just wasn't afforded the attention and rigor and formality that everything else was.

That is just the way it was, and it wasn't given that attention until the late eighties, early nineties when a consistent standardized health physics program was put in place by virtue of the orders and the Price-Anderson regulations and the Tiger Team.

So that is when you started getting a uniform approach and attention to something that was considered -- remember my old quote from Fernald when I was there in '85: "the only way you get hurt from DU is to
have it land on your foot." That was told me
by the plant manager. I was there to do a
health physics review. So you can imagine the
dissonance there.

So I am very familiar with --

MR. HINNEFELD: I remember that.

It was not the plant manager. It was actually
the assistant plant manager.

MR. FITZGERALD: Oh, okay. So I
am very familiar with the attitude for DU, and
I can understand perfectly why, for many
years, it just wasn't considered a big deal.
As long as you were pristine on the assembly
side, a little DU wasn't going to be a big
deal on the disassembly side. However, we are
talking about trying to figure out a means to
dose reconstruct.

That is where this blind spot
becomes sort of relevant to our issue, which
is, because they didn't consider it a big
deal, they just didn't monitor for it as they
would everything else, and the data is sparse. It doesn't become plentiful until 1990, and through no fault of anybody. It is just the way it was.

The dilemma for Pantex is it was an assembly/disassembly plant, ostensibly pretty damn clean. I mean, I never lost sleep over Pantex and HP at headquarters, but from a dose reconstruction standpoint, because of that blind spot with DU, there is an issue. There is a hole, and that is what we are trying to resolve.

so I am not arguing about the rigor of the program on the assembly side. I think, on the disassembly side, you really only had -- the big issue was the 28, and I agree with that. You might have had some issues with the others, but the 28, because of the length of service and the unsealed nature of it and the fact it oxidized like crazy, it was a problem anytime you took it apart.
1 So there was exposure, and for
2 some workers who handled and were on the line
3 for 30 years, it probably was not
4 insignificant. I don't have any idea what the
5 dose value would be, but nonetheless, it is a
6 real issue.
7
8 The only question is: how do we
9 reconcile the lack of the data, because very
10 little was collected before 1990, with this
11 question about how do we come up with a dose
12 reconstruction.
13
14 So I am only raising some issues
15 that are pretty clear but, yes, because there
16 wasn't much attention, the contamination
17 surveying, the air sampling and all the rest
18 just wasn't what it should have been or could
19 have been, and didn't really get changed until
20 you had these kinds of reviews coming out at
21 DOE that said, you know, we can't live with
22 that anymore.
23
24 I think we have to treat what data
we have before that time period in that context, that yes, we have data, but how it was collected and the quality of the data and all the rest of it comes into play. And whenever we use that to corroborate something, I think you always have to put an asterisk and a footnote saying, yes, but you know, these programs were not sufficient or adequate. I am not saying it directly. I am just quoting the review from '89 when the HPs went in to look at it and said "voila." Now they were suffering from the same thing everybody else suffered. They could have said "voila" a year or two before that. They lived with the system as it was as well. So the no flaw finding that, in '89 the system woke up to the fact that DU wasn't being controlled and surveyed in the way it should have been, and that that is when you started getting the data collection that would provide this program the grist for dose
reconstruction.

I guess I would feel differently if we had more objective information that said that the sixties and seventies, even though they were 20-30 years before, didn't appear any worse than the nineties. That would be surprising because of natural evolution of practice, but I can't get past this mean excretion rate that is in the TBD nor the fact that we have interviewed people that said, yes, you know, there were steps over time where things got tighter; they hired HPs and technicians, and there was a little more attention, use of vacuum cleaners. Things got a little cleaner as time went on.

I think we are just operating in a range of uncertainty about how much and when and, to the extent that any of these values are truly representative. I think that is where we are. I don't think it can be improved.
The data that we have looked at in the past year for uranium is the same data. We haven't really added data — I am talking about the bioassays now. We haven't added to that data, and the other data, whether it is air sampling or survey, I guess I would question based on this review and findings on the quality of that information and whether it would be used to corroborate your bioassay issue.

MR. BISTLINE: This is Bistline speaking. Could I interject a few thoughts at this point?

CHAIRMAN CLAWSON: Sure, Bob, go ahead.

MR. BISTLINE: In support of what Joe is saying, the objective information that we have, the differences in the values, the mean values, and then going back to the subjective side of it, in the interviews and so on that took place, we certainly see a
great deal of difference in the process that
was taking place in the eighties, the late
eighties and early nineties versus what took
place earlier on.

As you interview all of these
different workers, and none of which were
linked in any way, you get the same story
about coming out black, black on their faces
and blowing their noses and having black in
their handkerchiefs, and working without
gloves and respiratory protection in some of
the early cases.

I think it behooves us to look at
some of the processes and the information that
is available through the interviews that have
taken place. This goes all the way back.
This process engineer that we interviewed
talking about how, in modifications those
units were taken apart all the way down to the
bare bones, and that there was a great deal of
contamination present.
I think it is important that NIOSH gets to look at the interview notes that were taken with a couple of these people and the process and the information that they had to provide to us. Thank you.

MR. HINNEFELD: This is Stu Hinnefeld. Bob, are those interview notes -- have those been cleared and redacted or are those all in Germantown? Even if some of them aren't cleared and redacted, it would be better to look at the unredacted.

MR. BISTLINE: Yes. I think those are still in Germantown, and I don't think they have been redacted as yet, and I think that is what Kathy was referring to, is that we really need some of those redacted notes, Jim, but certainly, if you could go back to Germantown and look at the interview notes, I think it would be very, very helpful.

MR. FITZGERALD: Well, maybe Kathy can clarify one more time, because the set of
interview notes up to August of last year, as I understand it, were, in fact, redacted and are available. Now the unredacted versions are available as well in Germantown. The interviews since then are the ones we have just done, and they are in Germantown, and they haven't been screened yet, in unredacted form. But there is all the Site Profile review interview notes and summaries have been reviewed and cleared, have they not, Kathy?

MS. ROBERTSON-DEMERS: Yes, and that includes SEC interviews up through, I believe, August 2010 also.

MR. FITZGERALD: And were they not forwarded?

MS. ROBERTSON-DEMERS: They should have been distributed to the Working Group.

MR. FITZGERALD: Yes, they were distributed to the Work Group maybe a month ago. They finally came out of the DOE system, and those are actually -- there is a lot of
interviews in there that support what Bob is saying, because the only thing that is lacking is the most recent ones. But the bulk of the ones that Bob is referring to are in that whole group that has now been transmitted and available.

So you can look at it now in redacted form, and you can certainly look at the unredacted version in Germantown.

DR. MAURO: This is John. Could I also jump with a very quick statement? With that concept of a framework for decision making, the kind of things that we are talking about earlier about weight of evidence, et cetera, seems to me what Bob just described is extremely important in terms of, if there is interview information that says, gee, things were going on here that are quite unusual and our understanding of the processes and activities and the contamination may not be what we think they were, that goes toward our
ability to use the 1990 data to extrapolate backward.

I think we are really getting to the heart of where the weight of the evidence lies, and looking at that information, and if that information is indicative of unexpected circumstances that might have existed at the time -- I am presuming this black dust that you are referring to is indicative of a substantial amount of uranium contamination -- that perhaps is incompatible with the understanding of the place and the fact that perhaps the 1990 data may not be appropriately applicable or bounding for the early years.

I think this gets to the heart of the matter. Anyway, I thought I would add that in as a framework for decisionmaking.

CHAIRMAN CLAWSON: I appreciate that. I would like to at this time at least take a 15-minute break. We have all got submarines in our eyes. So, Ted, if we could
just mute it, we will come back in 15 minutes.

MR. KATZ: So we will be back --

folks on the phone, we will be back around

eleven.

(Whereupon, the above-entitled

matter went off the record at 10:47 a.m. and

resumed at 11:00 a.m.)

MR. KATZ: All right. We are

reconvening after a short break, Pantex Work

Group, and we are talking about DU and related

matters.

CHAIRMAN CLAWSON: With

enthusiasm.

MR. KATZ: With enthusiasm, yes.

CHAIRMAN CLAWSON: I just wanted to

make sure that everybody had -- John made a

statement there at the very end. We wanted to

make sure that, if there was anybody that

wanted to respond to that or, if there were

questions of what he was saying, to be able to

have the opportunity to discuss that.
MR. ROLFES: Yes, I had a couple of points, I think, back to what Joe had mentioned. I know Joe made a few points, and then Bob Bistline made some points, and then John Mauro did. I was trying to wait patiently to, you know, discuss some of the data that we do have available to us that would point as indicators as to exposure potential.

Getting back, I think I mentioned earlier on, we had done an analysis of the average air concentrations within the cells in bays. Basically, our initial analysis from about three years ago evaluated 4300 air sample results.

We looked at the average air concentration changes over time. We had some bits and pieces of missing information that we now have been able to fill or are in the process of filling, and are using those just as a comparison. You know, we certainly
realize that they are not breathing zone
samples, but there are air samples which are
taken.

We have got monitoring stations
set up around the cell and the two bays and
equipment rooms in that area, looking at
potential airborne releases. We have looked
at basically the highest concentrations that
were measured and compared those to the
intakes that we are assigning based upon
uranium bioassay data.

The uranium bioassay data related
intakes are more representative of actual
workers' exposure, and they are also higher,
but that higher is likely a result of the
minimum detectable amount of the analysis and
our interpretation of that data and the
assumptions of chronic exposures over time.

That data, as we have said
previously, was collected during the time
period that work was likely the highest
potential for exposure to uranium, because that particular operation, which was conducted in '84 through '89, those were some of the oldest weapons in the stockpile. They had some of the largest uranium source term in them, and the uranium that was there was the most likely type of uranium, we will say, to oxidize, potentially.

We can go back -- there are some things that were said about the depleted uranium, that nobody cared about it. To say that, really, is not true.

Even if someone says the only way that uranium could hurt you is if it dropped on your foot or head, I am sure that was said jokingly, but the data that are available to us -- for example, from the Fernald facility, if you look at the actual data that were collected pre-1985, we are looking at hundreds of thousands of uranium urinalyses that were collected at the Fernald facility.
So it still appears to me that there was a concern about exposures to depleted uranium, because we have got quite a bit of data showing that they were monitoring workers for potential exposures.

The monitoring -- we can get into discussions of why the monitoring was done and such, but we have a Fernald Work Group tomorrow.

Let's see. Looking back at some of the historical reports and incidents that I have seen, we have reports of incidents that occurred at the site where uranium was detected. There were elevated air samples. They were investigated. The materials involved were evaluated, and bioassays may have been collected as needed.

That was determined based upon surveys of the cells, surveys of the work areas. There are numerous documents that we have available to us in that research...
database, as well as health physics appraisals from the earlier time period which indicate that personnel exposure control and radioactive contamination control are excellent.

So there is good and bad. We have discussed this before. You have to take a weight of the evidence. You have to look at everything that we have available to us. We have looked at swipe data from the 1990-forward period. We have looked at -- I think there is about 90,000-something swipes, which would show a bounding quantity of contamination build-up during a disassembly period.

If a component is clean when it is built into a weapon, the bounding quantity of contamination would be observed during the disassembly period, and the data that we have reviewed, if you look at those 93,000 samples in that characterization database that is on
the K: drive for the Work Group, you can see
that the majority of the swipe data indicates
no removable contamination.

In addition to those electronic
data that we have available to us, we had the
opportunity to review some earlier swipe data
from the 1980s on site at Pantex, and I think
we came up with an estimate that there is
probably an additional 100,000 to 150,000
surveys covering about 30-something or 40
different weapon programs.

So we have got a pretty broad
scope of survey data that shows this is what
the worst case scenario observable removable
contamination could be from this particular
part of this particular program.

In looking at the data, at least
our quick perusal, it appeared that the worst
case scenario was about 1 dpm per square
centimeter of the removable alpha
contamination. We are talking about a very,
very small amount of removable contamination at the worst case that could be observed. These can be used for dose reconstructions. However, we do not use these for dose reconstructions at this time. We are using this as just another piece of information, in addition to the air monitoring data, to say, hey, is our bioassay data in line with what we would expect. Based upon my review, it looks like it is right in line with what we would expect, and that the approach that we have taken in dose reconstruction, by using the 95th percentile, we feel that it would bound potential worker intakes.

CHAIRMAN CLAWSON: Ted, I just wanted to make sure the other Board Members and Sara were on the line. Phil, are you on?

MEMBER SCHOFIELD: Yes, I am.

CHAIRMAN CLAWSON: Okay. Bob?

MEMBER PRESLEY: Yes.
CHAIRMAN CLAWSON: Okay. Sara, did you make it back on?

MS. RAY: I am back on. Can I make a comment, Brad?

CHAIRMAN CLAWSON: Sure.

MS. RAY: You know, I have been involved with this for five-plus years, and it sounds to me today, from what I am hearing Mark say, that he has changed his mind about the way that he wants to do it.

We are at a time now when all of this should start to be finalized. I have always felt like -- what I am hearing is the records aren't there. I just heard Mark talk about accident -- incident reports. I have seen the listings of them. There are 10-year gaps. Yet all of this information, to him, provides -- to use Joe's term, I think it is objective evidence.

I heard Mark talk about interpretation. It is the interpretation.
That is subjective, to me. I have a real problem with the flip-flopping, and it seems to me that there is a certain amount of grasping at straws.

The original guidelines, and I have a copy of them on my computer, to NIOSH before any of this started was that dose reconstructions should be attempted, but should not be the cost that would ever be paid to a claimant. This has obviously not been done.

The person that all of this is hurting is the individual worker. It sounds to me today, listening, possibly that Mark has not even read the interviews. The worker history is the only, if you will, true history of what has happened, and yet the claimants have continually been left out of the process.

They are the really important part of it, and it is just frustrating to me what I am hearing today, and I think NIOSH could be
made to abide by the same rules that we are or
a claimant is.

There are guidelines, and we
follow them. We can't keep changing mid-
stream. This cannot go on for another five,
too.

I appreciate you all listening. I
would like to thank Brad and the Board and
SC&A for what they are doing, but I am highly
disappointed in what NIOSH has done and is
attempting to do now. We have got to have
constraints on what they are allowed to do.
So thank you.

CHAIRMAN CLAWSON: Thank you,
Sara. I appreciate that.

MR. ROLFES: Sara, this is Mark
Rolfes. In regard to the interpretation, I
don't remember the context of what I said
interpretation of the data, but I think what I
was referring to: when we interpret the data,
if there is uncertainty involved in that data,
we use that uncertainty to the benefit of the
doctor of the claimant during the dose
reconstruction process.

MS. RAY: Well, and another thing,
everything I am hearing you say, Mark -- it
disagrees with what the Tiger Team said. It
disagrees with the Ahearne Commission. There
were several groups that looked at all of
this, and it sounds like an attempt by you and
others possibly -- and I don't mean this
accusatory -- but we cannot rewrite history.
We have to take history as it is.

If you are saying that the DOE
reports are wrong, well, then you are taking
their record. You can't have everything your
way, so to speak. If one record is good,
another is good.

I just have a real problem with
all of this.

MR. KATZ: Thank you, Sara.

MR. ROLFES: One other thing I
wanted to speak to. We also did hear, Sara --
this is Mark again. We also did hear from the
workers the exact same concerns that SC&A
heard. We are not saying that worker
exposures did not occur. That is not what we
are saying.

We are saying that we have enough
data available to us to enable us to bound
what the worst case exposure potential could
have been, and that is what we would use for
dose reconstruction, if we had no data for
that individual.

MS. RAY: I question whether or
not that is what you need to be doing. I
doubt if there was just one incident. You
have one incident that you are -- one or two
that you are dwelling on. But what about all
of the other ones that were never recorded?

I know you are saying that the
number that you are using is higher, and it
would be -- you know, higher than a worker
could ever have over that same period of time, and I appreciate the science, that type of science. But I think you are not looking at past practice.

There was not one weapon in there. There were multiple weapons. So everything -- it is a different scenario, I think, than what you are looking at. I think you are trying to box it into a neat little box, and there was no neat little box in the 1960s.

My experience also -- the comment about the letter, those letters are often written -- the 1959 letter -- in response to something that happened. That wasn't just out of the blue: you are doing a good job. There was some reason that that letter was written, from my experiences in the complex.

I just really question that you will be able to go back and, honestly, for every single worker who had the potential for exposure, that you can go back and do a dose
reconstruction that would be fair to all workers using 1989 and 1990 operational processes and records.

I don't think that you can take a car today and compare it to my 1955 Chevy pickup. I don't think records on my Navigator are going to, in any way, compare to my pickup. It is just not possible.

CHAIRMAN CLAWSON: Thank you, Sara. Joe, did you want to respond to anything?

MR. FITZGERALD: Yes. Just taking off from Sara's comment, for the record, we are trying to talk about the normalizing of operations, and we had this conversation one or two Work Groups ago about the fact that trying to take 1990s information and -- I won't use the word back-extrapolate -- but use it backwards to cover or to bound previous operations is troublesome, and for 30 years it is very troublesome.
Sara's comments brought to mind one particular issue, which I think would have a real bearing on this. In the earlier years of Pantex, they had three, four, five units in a bay at once, handling them in close proximity, and you had, just by virtue of handling multiple units, a much higher potential for source term in that area. That, over time, got restricted. They just wouldn't allow you to have that many.

Now it got down to three, two, and I think there were some procedures where it would limit it to one, but that is just one example of evolution of operations over that time frame which would have had a fairly significant bearing on available exposure potential, particularly of, say, the Rolf W28s being handled, which was quite plausible.

There are other issues in terms of controls and what have you, but those are the kind of issues that we are grappling with when
we talk about normalization of operations in
terms of making this kind of backfit, if you
will, a viable and plausible way of doing it.

I am not going to get into the
programmatic issues. I started jotting them
down, Mark, but we covered this at the last
Work Group meeting, and covered it over about
six or seven hours.

So all I would say is that
programmatically, we can't rely on the
programs, the procedures, the good intentions,
what is written down as the basis for what
actually happened, and whether or not that
actually did happen. I don't want to get into
that discussion again, but I think at the last
Work Group meeting we spent a lot of time on
outlining why this program, I think, can't
place reliance on program assurance and
programs implementation. So on those issues,
I would stand pat.

Now in terms of the completeness
and adequacy of the air sampling data and the contamination data, we talked about that. Certainly, that data is there. I would only raise the question of how complete and valid it is as you go back before 1990, by virtue of the Albuquerque findings. In fact, they found them to be unreliable. The findings are right there, the basis for the findings are right there. So I will leave it at that.

I don't want to go back there, but I still have problems using those calibration points for corroboration, just because the programs have been found to be flawed by contemporary HP views at the time.

I guess, in general, my take on the whole thing is that anytime, I think, this program is confronted with a back-extrapolation, taking relatively current data and using it backwards -- as, I think, John pointed out earlier, we have to be extra careful about whether or not one can normalize
across those operations.

The longer the time period, the more care I think we have got to take, that in fact, you can make those assumptions. I gave you one example of a major change at Pantex where it went from multiple units to one or two, just simply because that was the change in practice over those 30 years.

There are other issues, I think, that we have raised in the past. That is one aspect, but I think also, Jim and I had this colloquial discussion back in Santa Fe about how one approaches exposure potential, and it was a good discussion.

We were trying to figure out -- Pantex was actually part of the discussion -- this notion that you have an exposure potential. How does one walk that down? I think there was agreement that one had to approach it from a quantitative standpoint. Certainly, one can look at programmatic issues
and other issues, but not as a first order, that one started with a quantitative analysis. That is required by the regulations.

Then one could go on to perhaps looking at secondary information such as air sample data and what not, and then even so far as to look at source term information in terms of characterizing operations.

That is kind of how I looked at uranium -- this issue here, you know, starting with what was actually quantitative, objective information, and we have the bioassay samples, clearly, and we have some of the comparisons that were done, clearly.

I think where we stand today is that we have a set of data in the 1990s that is being proposed as a means to bound previous exposures, and I, for one, can't see any way to demonstrate that that necessarily bounds previous exposure scenarios that go back in time, when you might, in fact, have had four
or five W28s in one cell area or you might have had a handling protocol that was less rigorous, maybe fewer surveys by rad techs, because they didn't have very many rad techs in the beginning, but the notion that one can normalize over 30 years and claim that the operations and the controls were essentially bounded by the measurements in 1990, I just don't really find credible.

With the additional quantitative information — again, quantitative information, comparing the sixties mean excretion values with the 1990s mean excretion values, and having a factor of two -- I think that raises a reasonable doubt that, in fact, you necessarily had the same exposure potential between those two periods of time.

So that is where we are, and that is where we have been for quite a while. I guess at this stage, what I am thinking about is: Is it likely that the quantitative data -
- I mean the hard data -- is going to be improved at this point? I don't think so. I think we have what we have.

We have gone back to the site and dug more, but in terms of the hard data, the bioassay data -- I am talking about the hierarchy of what we look at. We got the bioassay data, for what it is. We have a comparison of that data. We have looked at the operations.

I think there is a reasoned sense, and I understand what Jim is saying, but I could make a compelling argument the other way, that the operations did shift over time, and that you did have a changing picture in terms of exposure potential.

Now a lot of this is subjective. I think we are, at this point, moving away from what is objective, but to looking at the operations and saying that, yes, there were these changes in practice that would have
altered the potential.

Nonetheless, I think you look at that when you try to back-extrapolate, but I think there is some real doubt as to whether the operations remained the same for 30 years such that you could use the bounding analysis that NIOSH is proposing.

So without any additional data, I don't see how this really changes. I think the Work Group just has to decide, based on what it has heard, where it wants to go with this particular issue. I don't think it is going to be improved by going back to Pantex or interviewing more people.

I think we have interviewed everyone we need to, and we have dug up all the data that we need to. It is what we got, and that is what I would say at this point.

MR. ROLFES: Can I please respond to a couple of clarifications?

I think earlier on in the meeting,
Joe, you had indicated -- this is Mark Rolfes. You had indicated that the assembly operations, you believed, were clean. You said you had no concerns about assembly operations.

MR. FITZGERALD: Relatively speaking.

MR. ROLFES: If you consider the number of units that are being assembled in a certain area, they are all clean units early on. The only issue that would be of concern would be external dose rates in that area, primarily. There wouldn't really be any additional concern over an assembly of several units in one area.

The internal exposure --

MR. FITZGERALD: Put an asterisk on that, by the way.

MR. ROLFES: Yes. We need to keep that in mind, but the most significant contributor of concern for dose reconstruction
would be the external dose rates in the area. That is for assembling clean, new parts. There is not going to be an exposure potential in the air.

If you take a look at the mean excretion values from the 1960s versus the 1990s, you have to use caution, as we said on the break, because there are data that are -- I think you might have just cited one particular lab analysis from one year of the several years that we had data in the sixties.

So I would have to look back at the mean excretion rates for the other years that we have data as well to compare those to the 1990s data. The other indicator --

MR. KATZ: Excuse me. Whoever is trying to break in, Mark is speaking right now. Thank you.

MR. ROLFES: The other indicator -- if you look at the history of disassembly operations, disassemblies -- you know, there
were other plants that were involved in work prior to 1975. We had the Iowa Ordnance Plant, as you mentioned in your memo, both the Clarksville and Medina facilities. Pantex wasn't the only one in town that was involved in operations involving nuclear weapons.

Really, if you take a look at the operations conducted at Pantex, it is really not until the 1970s forward that you really see the true increase in the number of disassemblies that are being conducted, and that is all part of the picture that you need to consider in looking at exposure potentials.

To use data from the 1985-1990 period to estimate what intakes could have occurred back in the 1970s is not too far of a stretch.

So those are all parts of the things that we would need to consider in determining whether or not we could bound potential intakes to workers.
MR. FITZGERALD: Well, a quick clarification. The W28 did come back.

MR. ROLFES: Right.

MR. FITZGERALD: I acknowledge that it also went back to Medina-Clarksville, but it did come back to the Pantex for dismantlement for any of these reasons we have talked about, mods, retrofits. So that did happen in the sixties.

In terms of looking at the other excretion rate comparison, I think this is something that we talked about in Germantown. I was kind of hoping that we would be able to put that issue to bed, because that was one of the issues we focused on specifically and talked about specifically.

At this stage, I think, yes, one could do that, but quite frankly, we have a valid comparison. It is actually cited in the TBD, and as I said earlier, we can dress it up and do all kinds of statistical analyses of
that data, but as a group that data from average standpoint, mean standpoint, comes out appreciably higher than the later data.

I think that is what the Work Group has and has had, and it kind of bothers me, because we have had this issue, and we have looked at this issue, and it has been out there for five years, and I think that comparison is a valid comparison and, even if we were to find another lab that didn't find an appreciable difference, we still have that difference there.

So I don't know where to go with that, but I just wanted to make that comment.

CHAIRMAN CLAWSON: I appreciate that. I want to make a comment now, because -- and please forgive me. I am just -- I am not a political speaker, and I am not very good with a lot of words, but I will tell you something that I have got a lot of issues with, and that is, number one, depleted...
uranium showed up at Pantex way early, back in the 1954, '53.

And like from our interview, it was a problem child when it came on site, period. They used to have to clean it up. They were having to make special acids to be able to even clean it up to be able to get it to be able to even be -- two HPs in 1989 covering all the assembly and disassembly.

So that is telling me you have two rad-cons that are making sure the assembly is put together clean and goes out clean. Like Sara said, that letter that they had was probably because they had an "oh, oops" -- that is two rad-cons. We are not even looking at that.

Right after that, in 1990 time frame, they totally shut Pantex down, because they did not -- they were not meeting the DOE guidelines or anything else like that, and since that time they have quadrupled to -- the
last I heard was 80 rad-con at Pantex.  
So from 1989 back they had to --  
and I call them rad-cons. They could have  
been called rad-safe or safety or so forth.  
The issue that I am getting into is working in  
the industry myself, I have got to be able to  
look at the changes that we went through from  
1950 to 1989, which were astronomical.  

There is no way we can be able to  
do what we did back there. Our production  
rate has also decreased a lot, because we are  
not able to do what we used to be able to do.  
Every time I hear this -- and we  
are going around and around and around in  
circles -- we are at the same point that we  
were five years ago, really. You know, we  
have gathered a little bit more data. We have  
got a little bit more understanding, but I  
think the term that somebody told me a long  
time ago: no matter how much lipstick you put  
on a pig, it is still a pig.
To me, no matter how much we go around on this, we are going to be back to the same thing. There is not really good data out there, and for me to be able to take data and go back with it, I think, is totally wrong.

In all the sites -- and this is no disrespect to the health physicists or anybody else that was there -- we see this complex-wide. We see it at Hanford. We see it at Idaho. We see it at Oak Ridge. We see it at all.

Most of them didn't worry about depleted uranium or uranium. It wasn't the big player. It was plutonium, and that is what they looked at. Later on in the years, we learned more, and we come to find out that, yes, there are other players in the game.

I think of the letters that were stated, and the one that pops into mind is a letter that was sent out that the importance of monitoring workers, and we do our best, and
we won't let anything happen. Just like Sara said, it was after an incident, because the one that I read, for me, was after 13 of us went down to the whole body count with uptakes.

They take it seriously. It is just the way they do business in there. My point to this is it has been five years, and I don't think that we are going to come -- I don't think we can go out and find any more data. I don't think that we can interview anybody more.

So as the Work Group Chair -- and I hope that the rest of the Board Members are listening to this -- right now I would like to take a vote to be able to push this to the full Board, as we have slated for August.

I feel that I have done everything I can to get to the bottom of this. I feel that we have gone through every rock. I feel that we have -- NIOSH and SC&A have done a
fabulous job of trying to determine what we can do, but I think basically, what it comes down now is to the Board.

So what I would like to do is I would like to propose that we move this to the full Board from the time frame of 1958 to 1991, due to the inability to be able to deplete -- or depleted uranium to the Board.

MR. KATZ: Brad, can you just -- I don't want to interrupt too much the discussion of your motion, but can you put some flesh on the justification for the time period you are giving?

I mean, Joe just made a statement earlier today about the -- what I have heard is '84 to '90 period, that that data was very strong for that period. You are proposing a period that goes to '91, which goes well beyond.

CHAIRMAN CLAWSON: Actually, in looking at it, the other weapons, depleted
uranium actually showed up at Pantex in the
early 1950s, but the data that I found down
there is that they were dealing with depleted
uranium in 1958. I can't get into what they
were doing, but it was part of the process
with the HE, and this is what we came up with.

Matter of fact, I went through
thousands of files of the shipments to Medina
and Clarksville from Pantex.

MEMBER BEACH: Can I interrupt? I
think that '58 is probably a good starting
point. I think the question is the later
years. So why did you pick '91?

CHAIRMAN CLAWSON: Because '91 is
-- and correct me if I am wrong, Mark, but the
petitioner stated '91, and I believe that was
right. Sara, was it '91?

MS. RAY: Yes, it was. It was
1991, and that was chosen because major
changes were made in rad safety. Rad-con
manuals came about in '92-'93. That was '91,
yes.

CHAIRMAN CLAWSON: And I can't change that date.

MR. KATZ: No, but that is the petitioner's date, and I'm asking about is Joe's statement.

MR. FITZGERALD: Let me clarify, and I tried to write this a little bit in the memo I sent. I guess I sent it to you, Mark. We wanted to kind of focus on that particular question, because I think there was some uncertainty when we met in Germantown about some of that. I think the key aspect on the beginning part -- well, first of all, the key aspect is the W28.

I think we have all agreed. I don't disagree with Jim's comment that we believe that 28 in particular offers this exposure potential above and beyond the other systems.

Looking at the 28, we were looking
at dismantlement. Dismantlement is, in a sense, the opportunity for an exposure potential. Now you could argue about the degree of exposure potential, which means the degree of oxidation and the handling practices and all that, but it is something that one can't easily establish, because, again, records aren't explicitly clear on exact handling and the degree of oxidation.

We did interview an engineer who pointed out that with raw, uncased depleted uranium, it oxidizes almost immediately upon contact with air, which means even if you're at the fabrication facility -- I think it was Y-12 -- you start getting oxidation, and that just worsened over time.

So even that issue is not clear, whether it took a year or two to get to a point where you would get a reasonable amount of airborne contamination or not. I don't have that specific data.
So we just focused on: when did you have a clear record of dismantlement at Pantex of the W28, and that is where the '58 figure comes in, because that is the first dismantlement which was associated with surveillance.

Now we tried to go further, and I can't really talk about it. We tried to go further to figure out what the exposure would likely have been, but I think dismantlement is a good trigger point, that you basically -- If you have dismantlement, you have an exposure potential to this uncased depleted uranium, and we leave it at that.

We don't have a good means to characterize how much was airborne at the time, which is partly what we have talked about. It is just not easy to do that because of the lack of air samples and bioassays, but we can point to that as being the first dismantlement.
Now going forward from there, I think, as Sara points out, after this incident and after the Tiger Team, very clearly in '91 Pantex completely revamped its health physics program and its control -- particularly its control program over the W28 and other units in terms of contamination control and all of these issues that we have talked about. So the endpoint would be a completely different system in terms of routine bioassays, the whole nine yards. So beginning with the 1990s bioassays, they started getting a different regime.

MR. KATZ: Joe, I guess the thing I just want to understand is you made a strong statement earlier that the '84 to '90, all that dosimetry that was done for the 300 individuals and so on is a very strong basis for --

MR. FITZGERALD: That was Jim's comment, I think.
MR. KATZ: No, but you were saying you would be fine if all the data were that, and that your problem was with the older data. You said pretty clearly in this that the data at that later period, all that data that was developed on the 300 individuals, that was good data, good methods and all that, high pedigree, excellent, so that the dosimetry that would be produced based on that would be good, and that is in '84 to '90 period. So I am just trying to understand what the basis is for going to '91.

MR. FITZGERALD: Well, because the actual sampling in terms of the workers didn't occur until 1990. I am just saying, we were talking about the fact that the campaign was five years long, but until the workers complained, until the management investigated, and until they ordered all those bioassays for 305, it didn't become apparent that you were having this degree of contamination and
intake.

So as far as when the practice was actually changed, it wasn't changed until after the incident. This was all -- the samplement took place in 1990. It wasn't done across the five years. It was only done in 1990. Okay?

Now because of the nature of the uranium uptake, you could assume that those bioassays would tell you about the exposure over that time frame, but in terms of when you started having good data across the board for the workforce, that would be when they were doing bioassays for all the workers that were potentially exposed, not just simply the 300 that they singled out for the incident.

MR. KATZ: So I misunderstood, because I thought the argument was that that was the worst exposure -- at least contemporaneous to the '84-'85 period forward, that would have been the worst exposure, and
then you are saying, yes, and their dosimetry
that they based on '90 data but covering that
period would be good for that cohort that was
exposed from '84 to '90. Those are the most
exposed people, and your dosimetry on them is
good, and that is a bounding analysis for '84
to '89.

So are you saying that there are
other people in Pantex not involved in that
incident that would have had higher exposures?

MR. FITZGERALD: No, no. But I am
just saying there's other people that were
exposed, but for that cohort, those bioassay
samples would have been representative. Yes.

MR. KATZ: So then -- okay. I was
just trying to get -- so you have a cohort
that is the worst cohort at Pantex from the
period '84 to '90, and you can do their dose
reconstructions, so it would seem to me that
you are arguing that, really, the SEC period
would end at '84, because from '84 forward you
have this worst cohort, and you have good
dosimetry on them. So that is a bounding
analysis for Pantex, starting in '84. Or not?
I'm just trying to understand the argument.

MR. FITZGERALD: The only issue in
my mind -- That is a plausible approach. The
only issue in my mind is you have other
workers that weren't a part of that cohort,
that one would have to establish that the 305
were the worst cases.

I haven't heard that discussion,
but I would assume that, as far as this
incident, they would have been. The reason I
am hesitating is because you have a five-year
period where you have workers that may have
come in and out of the program.

I don't know if that 305 would, in
fact, represent the bounding cohort. I would
think intuitively it might.

MS. RAY: May I say something?
The DOE was not happy with Tiger Team. The
Ahearne Commission was appointed because of the Tiger Team report.

MR. KATZ: Sara, we understand that, Sara, but --

MS. RAY: They would not have thought of that as good data. I think any ending date has to come after 1990.

MS. ROBERTSON-DEMERS: This is Kathy. I think what we have been saying all along is that that data might be bounding, but we need to see the objective evidence that it is bounding for all situations all the way back to 1958.

MR. KATZ: No, no. Kathy, that is mixing another issue. We are talking only about for the period from '84 forward whether that is bounding, Kathy.

MS. ROBERTSON-DEMERS: Right, right, and like Joe was saying, we have to demonstrate that that is bounding.

MEMBER SCHOFIELD: Ted, this is
Phil. I would like to throw something in on that. Okay, we did have these people that were assayed based on that one incident. What we don't have is a lot of the crafts, a lot of the guards, other people that may have come through that area, picked up a dose, who were not on that program, who were not assayed.

As we know from their testimony -- we went through Pantex -- this is a strong possibility, that this contamination could have been spread. It was spread by people on their hands, on their booties, clothing. So that does not bound everybody at the facility saying everybody that got a dose was covered. You have so many people going through these areas that are not on a bioassay program. You have a lot of people falling through the cracks, and I could not vote and say, yes, that is great.

No, there are too many people in that time frame who, until the Tiger Team came
in, ripped them up and said you are going to change this program from top to bottom. Those people -- you don't know who went through that area in every case.

They did not keep logbooks of everybody that went past those cells. They did not keep track of every person who came in and out of there.

CHAIRMAN CLAWSON: Thanks, Phil.

Also, too, I think when Phil or when Joe and Mark were discussing the 1989 data, I think in his reference, yes, it is more scientifically proven than the early years, because, remember, we were talking about the earlier ones.

MR. FITZGERALD: I think that it's a valid question as to whether or not the values, and they are credible values that were taken, would in fact envelop those workers that worked on the W28 during those specific years.
The reason I hesitate, even though I think it is a valid concept, is in terms of solubility, and I think NIOSH can answer this maybe at the table now. In terms of solubility class and what have you, would you, in fact, be able to bound just the 305 workers and any other workers?

To answer Phil's question, until I -- and if you saw my memo, I did go into some details to other worker categories that would have been implicated, because you just can't confine contamination. There's other people, guards, maintenance people, who become exposed, but I would think -- this is sort of an open question -- that the actual hands-on operators would be bounding, because they would be most exposed continuously; whereas, maybe some other worker categories would be exposed, but not quite as directly and continuously.

Now, guards are a question,
because they clearly would be doing surveillance, but, again, I think that could be addressed as well.

So I think it is a valid question. I don't think we have actually answered that specifically, and the Board is certainly within its scope to feel that, even though that might be an open question, one could feel that you can make the argument up to '84 without any reservations. Certainly, NIOSH can answer the kind of questions that Bill, Kathy and myself would raise about the data versus the four or five years of that specific campaign.

We have not, as a Work Group, focused on that, but think it would be something -- given the later time frame, I think it could be answered relatively straightforward, but not keep the Work Group from moving forward on the pre-'84.

MR. KATZ: But just to clear about
my role, because I am not trying to engage in
the substantive. This is the Work Group's
business with its technical staff, not mine.
I just want to make sure that, when Brad gives his recommendation to the
Board, that the basis that he provides lines
up with his dates. What I interpreted you as
saying earlier, really, I understood
differently than you are pitching it now,
which is fine, but which is why I said what I
said, to be clear.

MR. FITZGERALD: Yes, I think the
'84 -- I mean, the bioassay data taken in 1990
reflecting the campaign conceptually might be
usable for that campaign, but there are a
number of questions that come to mind as to
whether or not that would be bounding for that
campaign, whether or not the 305 workers
represents the most exposed individuals,
intuitively it sounds like it might, but I
think that has to be nailed down.
MEMBER BEACH: Let me just ask a clarifying question. The 305 samples, some of those were taken after -- several months after the actual exposure time period. Isn't that correct?

MR. ROLFES: Correct. The exposure potential time period for this particular operation, which was the bounding operation for potential uranium exposure, as indicated by actual survey data, air monitoring data, worker interviews and the bioassay data that we have.

There was an operation going on for five years, roughly from 1985 through 1989. In 1989, a worker reported basically having oxide on his gloves and on his coveralls, and I will read here just a little excerpt from February --

MR. HINNEFELD: The issue here, I think, is one of timing and what the argument is based on. We are talking about the -- I
can talk about this stuff, right?

CHAIRMAN CLAWSON: Right.

MR. HINNEFELD: We are talking about the W28 dismantlement, meaning they were being retired or not being --

MEMBER BEACH: No, there is more to it.

MR. HINNEFELD: I am just talking about what the data is purporting. NIOSH's presentation is purporting that this dataset provides a bounding dose for the dismantlement of the W28, and that the W28 is the worst -- that dismantlement of the W28 is the worst potential for exposure. That is the NIOSH position. That is all I am saying.

I am not arguing it. I am just trying to restate it. So those are the right dates? They started dismantling to remove it from 1984. Is that the correct date?

MR. ROLFES: Yes, correct.

MR. HINNEFELD: Okay. So that is
then where we considered this bounding approach. So if, in fact, there is a reason to add an -- I have not heard a reason to believe that there is another set of people who would have had a higher dose.

Now, Phil, to your point, we are not saying that only the 305 people are going to receive this bounding dose. The potentially exposed people would receive this bounding dose, not just the 305. So the maintenance people and security people would be -- who had gone in there, or we wouldn't maybe know where they went, so they would get the dose.

So the fact that not everybody was sampled who was potentially exposed doesn't really relate to the ability of this dataset to bound the dose.

I am saying this stuff, and I am asking this stuff not because I am trying to take a side in the argument. But remember
that a designation decision is made by the Secretary. Okay? And I have got to advise my boss what to do and, if I can't explain to him why he should ignore his technical staff, what do you think that is going to say?

That is what I am trying to make this argument for. I am just trying to get this out there. I have not heard -- I have been trying to listen. I have not really heard a particular reason to believe, because what I have heard is W28 seems to have been the worst, because it was unalloyed and uncased, that dismantlement is the worst activity, although they did disassemble things for surveillance and modification and stuff like that.

The numbers of things done per year were certainly high when they were dismantling from '84 to '89 and when they were doing the maintenance, and I just don't hear much that tells me a reason to believe that
that is not bounding. I am just trying to lay it out here, guys, because this is not something that I have delved into. I have not looked at the technical evidence to the extent that everybody else has, but I am just looking at my path forward going down the road.

CHAIRMAN CLAWSON: I understand that, and here is my take on it. Things have changed over the years. We are taking a snapshot of time, five-year time period, and saying, well, this is the worst it could have ever been. This is '84 to '89, and this is the worst it could have been. Nobody could have ever done it, but we don't know, really, the other 30 years.

MR. HINNEFELD: My question right now is I am only talking about '84 to '89. That is all I am talking about right now, is '84 to '89. What have we said today that makes us say that '84 to '89, that this data
doesn't bound the doses?

MEMBER BEACH: We have to -- I agree. I think that we need to look at that end date, but I think we -- we haven't been focused on the '84 to '89. I think we need to take a look at that and see if that is bounding, so that we can come up with the right end date. I think that --

CHAIRMAN CLAWSON: Because at the beginning of this meeting, the process that was going to be done was we were going to take the '84 to '89 data and put it with the early 60 years. So now --

MR. HINNEFELD: I understand that. I am just talking -- you know, you are talking about -- the discussion here is about a recommendation to go to the Board with a recommendation to add a class for some time period, and you said through '91, and I haven't heard anything that says it should go past '83.
MR. FITZGERALD: Yes, just to jump in again, it is one of these things we really haven't focused on, but I tend to agree with Stu that when we did the comparison contrasting the data, it was clear that we were contrasting data that went back to the sixties, seventies, and early eighties, and I think the question of whether or not that data would bound the campaign itself, I think is a good question.

That is why I said it was a legitimate question that was raised, that Ted raised, because we have been looking at it in a different way, but looking at it from that standpoint, I would say, you know, I could see the 305 bioassay samples being bounding of those workers, other workers that weren't as directly involved with. And, yes, I think this is something that we need to do a little bit more homework on just to make sure that that approach, in fact, is valid.
I think it might be, but that would not necessarily hold back the Work Group from accounting up to '83, because I think there I would say those samples aren't going to do you much good, going back before that campaign. That campaign, I think, stands as a specific operation.

We don't get into the normalization issue as much with the five-year period that you would for a 30-year period. But I think we had to cross those Ts, because if they went from -- if there were some changes, there were workers coming in and out, I would want to at least be able to see that accounting done that would give confidence that you can back down those five years.

I think there is no equivocation before '84. I think that is where all the discussion today brings you to the fact that, yes, it would be a much bigger reach taking that same data and trying to apply it to those
earlier years.

CHAIRMAN CLAWSON: And this is what we have done at other sites, you know. Because I'll be right honest, Stu, I now understand what you were saying about it. I was taking it as this whole thing, because we have been told that these 350 are bounding for all years. Then all of a sudden, this has changed.

MR. HINNEFELD: I am not trying to change my argument. What I am saying is, what can we conclude from the evidence being provided?

CHAIRMAN CLAWSON: Right, and I agree with you, and maybe what I ought to do is --

MR. HINNEFELD: Well, I can't necessarily tell you what will be concluded, because there is still a lot of evidence before '83, and I think -- I don't know if we want to get into this before lunch, but I
think we still need to make sure that we are clear on -- you know, NIOSH's argument is that there is sufficient other evidence to believe that these doses from this '84 to '89 campaign would bound all of that work.

So some reasons to say that -- now there may be some reasons talked about here, the reasons that the Work Group chooses. You know, what convinced us that NIOSH's argument is not convincing?

If that can be set down clearly, it makes the path for the Institute going forward a lot easier.

CHAIRMAN CLAWSON: I understand. I understand fully what you are saying, and what I am looking at is: 1984 on we can address. There is an awful lot of petitioners that are leaving this earth, and I would really like to be able to see -- because these earlier years, bottom line I understand what you are saying.
So I am going to re-put this to the Board. What I am going to say is from 1958 to the end of 1983 that this go to the Board for an SEC, due on the inability to be able to monitor for -- or however we want to put it, for depleted uranium.

Is there any questions by other Board Members?

MEMBER PRESLEY: This is Bob. Now are you saying that this is all people that worked at Pantex?

CHAIRMAN CLAWSON: Yes.

MEMBER PRESLEY: That is not right.

MEMBER BEACH: Well, that is the Class Definition that we have to work with.

CHAIRMAN CLAWSON: That is the Class Definition, Bob.

MEMBER PRESLEY: So we are able to go back in and find the people that worked in that operation. You will have a few we will
have to look for, but there is no way in the world I can go for a --

CHAIRMAN CLAWSON: So you are telling me that you can go back and find the security guard?

MEMBER PRESLEY: I am telling you that there is going to be people that you can't find and, yes, we can help you, give the benefit of the doubt, too. It is not fair to go in and say that the people that worked in the cafeterias or the people that may have worked in a non-rad building are covered.

MR. KATZ: Excuse me. Wait one moment. Bob, we could hear you, sort of, but it is very difficult. I think Sara -- maybe someone has their line open, and we are listening to chatter in the kitchen or something, and it is making it very hard for us to hear one of our Board Members. So, please, mute your phone, *6 if you don't have a mute button, and that will help us a lot.
Much thanks. I still hear -- I think it is you, Sara, but maybe it is someone else. Please mute your phone.

MS. RAY: No, it is not me. I am muted.

MR. KATZ: I am sorry, but it is a woman, anyway, that we are hearing.

CHAIRMAN CLAWSON: Kathy?

MS. ROBERTSON-DEMERS: No, I am not in the kitchen.

(Laughter.)

CHAIRMAN CLAWSON: Well, Bob -- and I understand that to a point, but -- and like I say, you can vote your opinion. Your vote is just as important as mine is or whatever else, but I don't think that you can really single anybody out. I don't think you can single out the people that are bringing in shipments of depleted uranium, but this is your choice. You can air these concerns.

My whole thing is that all I am
doing is voting as a Work Group to be able to
put it before the Board and start airing these
things to the Board.

MR. KATZ: But your motion -- just
to be clear, Brad, your motion is to say all
workers.

CHAIRMAN CLAWSON: Yes.

MR. KATZ: That you're suggesting
to the Work Group to recommend to the Board.

CHAIRMAN CLAWSON: Yes.

MEMBER PRESLEY: And I don't agree
with that. I am sorry.

CHAIRMAN CLAWSON: Bob, that is no
problem. We all have these opinions, and it
is just like a lot of them. I don't agree
with them either, and that is what we can go
with.

MEMBER SCHOFIELD: One of the big
problems we have is the fact that how do we
know who went in these potentially hot areas
and who did not? Unless there is some valid
way that we can selectively say these people went in here and these people did not, but given Pantex's -- the lack of badging, the lack of bioassay for many people who still went through those areas --

MR. HINNEFELD: Let me just start on this. This is Stu Hinnefeld. If I am correct, there are pretty comprehensive access records to the various buildings at Pantex, and this is a record underlying some of these testimonies.

MR. ROLFES: I just want to clarify. I guess for the earlier years, in our last document review trip at Pantex, I found a box of records that were created in 1980-1981 time period. Any employee that was on site at that time period filled out a sheet which showed which buildings they had accessed during which years, from the beginning of their employment up until 1980.

Now also beginning in 1970, there
CHAIRMAN CLAWSON: Hold on, Sara.

MR. ROLFES: Now, beginning in 1970, there was also a system we looked at that had radiation safety training requirements, basically specific authorizations and approvals to work on certain aspects of certain weapons systems in certain areas.

It wasn't a casual operation. It may have been more casual in earlier days. However, there is documentation which allows us to identify which workers worked on which weapons systems or in which buildings.

MR. FITZGERALD: In the category of fly in the ointment, let me make one comment, because we did kind of probe that a little bit in our last site visit.

MR. KATZ: I am sorry. There is someone on the line who is speaking. Please mute your phone. Hello, hello? Someone on
the line, a man now, is talking. Please mute your phone, *6 if you don't have a mute button.

MS. RAY: This is Sara. Can I say something?

MR. KATZ: Well, right now Joe is speaking. Thank you, Sara, but you will get your chance, Sara.

MS. RAY: I am sorry. I thought he was finished.

MR. FITZGERALD: No, we were waiting to clear the conference phone. I have a short comment, Sara, and certainly bow to you.

In my memo, this is something we specifically asked for, which was what other worker categories -- clearly, operators would have been exposed, and they were, in fact, bioassayed. Who else might have been implicated, and could you confine contamination to specific areas?
Granted, this came from an interview. I have to look at the notes, but--

MR. KATZ: I'm sorry. Sir, whoever is speaking about '91 and so on, you are not on mute, and you are interrupting Joe Fitzgerald here in the room who is trying to get a few words in edgewise. Would you please mute your phone or stop talking, either one?

Thank you.

MR. FITZGERALD: Okay. What I was going to say is that we asked the question, what worker categories would have been implicated, not just simply the operators that we focused on. In terms of contamination spread, what areas should you be concerned about, and this individual -- This is kind of the first time we actually raised this in this particular way. He identified technicians, supervisors, engineers, safety personnel, handlers, support personnel.

It is sort of the usual range of
characters that would involved that would have
frequented these areas. But then he gave me
some pause, and I am only putting this out
because it is something I hadn't thought
about.

They had done a chronic beryllium
survey of Pantex, because they were concerned
about beryllium spread in the facility, and
they did this at most DOE sites. They found
beryllium in the office areas. They found
beryllium in storage areas and hallways.

And his comment was, given the
controls on depleted uranium in the early
days, he would not have been surprised that
you would have had residual contamination in
the same areas that you found the beryllium,
just basically because it might have been
tracked out. You just didn't have the
surveying and the controls, as I noted in the
Albuquerque audit, in the early days.

So there is no way to pin that
down, but just saying that that was the observation in terms of some of that. That is in the memo.

MR. ROLFES: Just to clarify, what you put in the memo -- it didn't say anything about uranium contamination, just specified beryllium contamination, though.

MR. FITZGERALD: Yes. In the context of DU, what I am pointing out is that the comment was in the context of who may have been involved with depleted uranium beyond the operators, and where you may have found similar residual contamination for DU as they had found for beryllium. That was the intent. Maybe I didn't word it very clearly.

His point was, given that finding, that we had to be careful about assuming that the depleted uranium stayed in a particular control area, that it never got out. In fact, they had that assumption for beryllium, and it was a false assumption.
So be that as it may, that is about as far as we could take it.

CHAIRMAN CLAWSON: There was also some things said in there that, I believe, got redacted out, because he got a little bit into detail.

MR. FITZGERALD: Yes. This is the generalized surviving words that I had. That is all I can tell you about that.

MS. RAY: Can I make a comment?

MR. KATZ: Yes, go ahead now, Sara.

MS. RAY: What Joe is just saying, you are talking about basically what I am hearing is individuals carried beryllium and other materials on their coveralls. The coveralls were washed at the plant site, but if someone went through the cafeteria, they took it with them to the cafeteria. If they went to 1236, and they signed a form for payroll deduction or whatever, they carried
Employees did not shower. They were not swiped at the end of shift. The utility guys, the people that do the air handlers, they changed the HEPA filters -- those are nonstandard air handlers. They had to crawl inside those. The fire department has to go in and do PMs. The guards have to respond.

I mean, there are so many people that are in and out. The people who carried the cards, and it would have been the old IBM with the cards. It would have been payroll cards that were going up. It would have been disassembly cards that go with the weapons. Those were carried throughout the plant.

That material was not contained in the bays and cells. It was not only where the weapons were. It was carried throughout the plant. It was buried. It was burned. There are documents that represent that.
So it has to include everyone.

CHAIRMAN CLAWSON: And I understand what you are saying there, Sara, because at many of the sites we have seen the same thing. What this individual was using was because they put so much effort into the beryllium, he was just showing how it traveled throughout the site, and it actually had more controls than what some of the uranium and so forth had on it.

MS. RAY: And I saw all the records on the beryllium, and it was basically in every single facility.

The other comment that I wanted to make, what Mark was looking at: the plant recognized at some point -- and my husband, Don, participated in this, but they never kept records of what programs people worked on or what facilities they worked in.

Don had maybe been working there five or six years, and they called him up and
they said, okay, we want you to write down
every program in every facility you have
worked on and every operation.

How many of you can tell me
everywhere that you went last week and track
every single step? I cannot do that. I am
old. So I get the benefit of the doubt, but
that is a difficult thing to do.

So I question what Mark is talking
about. That was a record created after the
fact. It was not one that was maintained
throughout the years.

MS. ROBERTSON-DEMERS: This is
Kathy Demers. I have kind of a simple
question for Mark. The records that you are
talking about -- are they in -- do they fall
into such a category that they could even be
released?

MR. ROLFES: They are Official Use
Only, because they contain Privacy Act
information concerning details of the
individuals' work history.

MS. ROBERTSON-DEMERS: Okay. So we would be relying on this couple -- a set of records and going into who worked on what program, we might be in another category.

MR. ROLFES: It would still contain Privacy Act information.

MS. ROBERTSON-DEMERS: What I am getting at is: is that information of such a content that it would have to be secure?

MR. ROLFES: Yes, because it contains Privacy Act information.

MS. ROBERTSON-DEMERS: No, that is not what I am getting at. So all of this information--

MR. ROLFES: To my knowledge, those records that I reviewed were unclassified, and there is no reason that the identify of a particular worker working on a specific weapon program would be anything more sensitive than Privacy Act information.
That is not my call to make, however. It is Department of Energy's.

CHAIRMAN CLAWSON: Time frames, when you start putting --

MR. ROLFES: Like I said, that is not my call to make.

CHAIRMAN CLAWSON: Right. I understand. From what we have learned, you can't -- that is where they get into problems with time frames.

MR. FITZGERALD: I guess the other issue -- I heard the end of '83. I was thinking, does that actually mark precisely the beginning of the campaign? I suspect it wasn't January 1st, '84. Five years, I think, is just our shorthand description or term for the campaign, but I would want to nail that down a little better.

Then also you -- given the solubility classes, your bioassays, would they -- you know, we are sort of saying that they
would see the five years. Of course, bioassays don't stop at the five years. I am just wondering if -- but we are confident that it would envelop those five years in terms of the results.

Mr. Hinnefeld: What I said was I didn't -- haven't heard a reason why it wouldn't. So there can be additional discussion going forward, but I haven't heard a discussion today why it wouldn't.

Chairman Clawson: My feeling on this is, just like a lot of the other sites, you know what, we haven't looked at this data to be able to use it in just this content. Like I say today, things have changed. Now we are looking at it in a little bit of a different aspect, but I personally don't see anything for 1983 that we can bound -- or '84.

Mr. Hinnefeld: Yes, '83 and earlier.

Chairman Clawson: Yes. I don't
see anything on that. My opinion is that, as I have put out to it, and then we can continue to research this data and go forward from there. If it gets extended a week forward, then that is what we can do from there.

MS. RAY: And this is Sara. I would like to request that you also continue to look at the '91 date, because there was a reason for that. I cannot tell you every single reason at this point, because I don't have all of my information. But I think that considering the Tiger Team report is an important thing. I don't think you can set it aside.

I think that Stu could go to the Secretary and say this is on DOE findings and recommendations, and that after '91 things changed. I think '90 or '91 should stay the ending date.

I appreciate the '83, but I really would like for people to continue to look at
the later date and not just dwell on this one weapon, because I know many other weapons were disassembled, even in recent years and you all, the ones who have clearances, I am sure you know which ones I am talking about. But there are many weapons that are probably not as dirty, but dirty.

MR. KATZ: Thank you.

MEMBER BEACH: So, for me, I am in agreement with moving forward with a vote between the Board Members on this Work Group to the end of '83 time period, but I would also like to ask Joe, how long will it take you to review the documents for '84 to '91 and get back to us? Is it doable before the August Board meeting?

MR. FITZGERALD: I don't think so. I think it is a new line of inquiry.

DR. NETON: I think, if you craft the designation such through the Board that you are leaving this period open, you don't
have to go through the 83.14 process. You just say we can go through '83; we are still investigating this latter period.

CHAIRMAN CLAWSON: Right.

MEMBER BEACH: Which is fine. I was just curious as to how long Joe thought the --

MR. FITZGERALD: Well, I think it wouldn't be the next two weeks for sure.

MEMBER BEACH: That's fair.

MR. FITZGERALD: So it is a new line of inquiry, and I think we have talked about some of the issues, solubility class and whether or not it envelops all the workers that would be relevant, and some of the discussion about whether you can actually locate the affected areas of the plant in terms of access information.

That all sounds like a completely different line of inquiry. It is going to some time, but it doesn't, obviously, keep the
Work Group from doing what it can do through '83.

MEMBER BEACH: Which I think is important.

MR. KATZ: Right, and then the Work Group, in its report to the Board, can talk about what is going on, give them an update on what is going on for the '84 forward period, too. I think that would be a good thing to include in the presentation of the Work Group.

MR. FITZGERALD: And, Sara, this is exactly what you are talking about, just being very responsive to making sure that all the Ts are crossed in this five-year period up to '91 before settling on that issue. So, really, trying to settle on what we can settle on in terms of what the technical information -- where the technical information takes you, and where there are some remaining questions, closing that out as quickly as we can.

CHAIRMAN CLAWSON: My one question
As a Board and so forth like that, we have always been held within the strength of the petition. So if we go past that time period, we are going to have to also designate that in this as a time period that we are looking at.

MR. KATZ: So you are bound within the period of the petition's eligibility. So that is '91, the end date, then that is where you are bound for all of your work. DCAS can go beyond that. You can't.

CHAIRMAN CLAWSON: Right, and that is what I am wondering, is how we do this because of what Sara just brought up.

MR. KATZ: If the petition goes to '91, the Board can consider '84 to '91 down the road, without any -- there's no constraints on that.

CHAIRMAN CLAWSON: No constraints on that.

MR. KATZ: If you wanted to consider, you know, 2000, of course -- and
that is not within the envelope of the petition -- then you couldn't do that without another petition, but within the envelope of the petition you can consider that full period. So you are apportioning one piece of that petition now. It is fine, and it doesn't constrain you to continue working on the rest of what is enveloped within the petition.

MS. ROBERTSON-DEMERS: This is Kathy. Are we going to continue consideration of '51 through '57?

CHAIRMAN CLAWSON: Yes. I understand what she is getting at, because in this -- when we went down there, the interviews indicated earlier years, but '58 was the only year that we could find the disassembly of the W28. So I guess we need to look at the earlier years in the same aspect.

MR. KATZ: So that is another thing that you can report to the Board, that you are still looking at the '51 to '58
period, and explain what the issues might be for the '51 to '58 period. Then they will have a snapshot of all of what is going on, and what you are putting before them to start wrestling with, the '58 to '83 period.

CHAIRMAN CLAWSON: So let's recap this to the Board Members that are on there. What we are looking at is to take to the Board in August the 1958 to the end of 1983 for all employees, due to the inability to reconstruct for uranium, depleted uranium. I guess I am looking at the -- and we will look into the earlier years, the 1950 to 1958 and the 1984 to 1991. Does everybody understand what we are doing or do we need to clarify it? Phil?

MR. KATZ: Phil, are you still with us?

MEMBER SCHOFIELD: Whoops.

MR. KATZ: There you are.

MEMBER SCHOFIELD: I think let's go ahead and just hold off on those latter
years until that gets clarified, but go ahead with an SEC, as Brad has proposed.

MEMBER BEACH: The only thing that I would make note on is that you need to clarify, like Bob brought up, who was involved. I think that that may be part of your presentation of clarifying who may or may not have been involved, and why, what our thoughts are.

MR. FITZGERALD: That bears on the breadth of the Class, which was an issue with what Bob Presley raised, and I think that might be the basis for a larger Class and a smaller Class. That is arguable, but I think that would be the basis.

CHAIRMAN CLAWSON: Well, and I think, when we bring this before the full Board, I think we will be able to write this up in a better understanding of what we are saying with that because I understand Bob's point on it and so forth, but from what I have
seen -- and this is my personal opinion -- I don't know how you would be able to do it.

MR. KATZ: So we should just get clarity though. Phil, you support the approach that Brad has put forward, the motion. Is that correct?

MEMBER SCHOFIELD: Correct.

MR. KATZ: Okay, and then let's just get Bob Presley. You position is what, in support or opposed?

CHAIRMAN CLAWSON: But also did he understand what I said? Do you understand what I propose there, Bob?

MEMBER PRESLEY: I understand what you propose, but if we vote on it -- if I vote on this thing, then how are we going to bring it to the full Board that there are some reservations on the Class?

MR. KATZ: So, Bob, you just need to state so that we are clear what it is you support or don't support, so that when Brad
reports out -- and I would suggest that Brad
share with the rest of the Work Group Members
what he is planning to present, but so that he
can accurately represent where you stand as a
Member of the Work Group.

MEMBER PRESLEY: Okay.

MR. KATZ: So that is what he
needs to hear from you now, just you can be
supportive, opposed to the whole thing,
supportive but you are not supportive of a all
workers Class. Whatever your position is,
that is what we need to hear.

MEMBER PRESLEY: My position is
that I am supportive of the Class, but not for
everybody that worked at Pantex.

CHAIRMAN CLAWSON: I understand.

MR. KATZ: Okay. Your statement
on the record before fleshes that out nicely.

Josie?

MEMBER BEACH: I am in support of
the motion.
MR. KATZ:  In support, and those are all the Members. Thank you.

CHAIRMAN CLAWSON:  Now my question is -- and this is to you, Ted -- because we have not looked at this time frame in this aspect, do we need to task SC&A to do that?

MR. KATZ:  No. So what you are asking -- this is not for the pre-August Board meeting, but yes. I think SC&A needs to scrutinize the question as it is on the table now in terms of the post --

MR. FITZGERALD:  I think Jim and I are on the same page.

DR. NETON:  I would suggest that NIOSH --

MEMBER BEACH:  NIOSH, I was just going to say --

DR. NETON:  -- because we have changed our position --

CHAIRMAN CLAWSON:  Oh, you know, you are absolutely right, Jim.
DR. NETON: -- and I think we need to take the opportunity to flesh this out in more detail. I would acknowledge that what we have on the table right now doesn't have sufficient detail to demonstrate -- clearly demonstrate it.

MR. FITZGERALD: I think we -- we put -- some of our questions I think I expressed -- I think those are the kind of things --

MR. KATZ: So this is -- this is step-wise, that's fine, I mean, so NIOSH needs to put on the table -- I mean, NIOSH hasn't necessarily withdrawn its position, as Stu said, that it can cover it all. Anyway, if it comes to this, NIOSH needs to put its position on the table as to how it would address the period of '94 forward -- `84 forward, and at that point I don't think we need a new tasking. You know, Joe, that at that point you would be scrutinizing that.
CHAIRMAN CLAWSON: And the '50 to '58, because --

MR. KATZ: That is ongoing, '50 to '58. So you don't need a new tasking. That is ongoing, and I assume you will continue marching down that road.

MR. FITZGERALD: -- that some questions came up about earlier systems. We don't know.

MR. KATZ: Right. Anyway, no new tasking needed for that.

MEMBER BEACH: So we get work product from NIOSH; SC&A reviews it.

CHAIRMAN CLAWSON: I just wanted to make sure we weren't held up with any kind of a tasking, and you are absolutely right, Jim. I apologize. I was looking at it as the tasking part of it instead of what it was.

MR. KATZ: In terms of time frame, I imagine -- DCAS can speak for itself, but they are not going to march down that road
until some of this gets addressed at the Board level because it sort of depends on what happens at the Board level how they handle that question.

CHAIRMAN CLAWSON: Right. The time here is 12:30. We are going to break for lunch. So we will come back about 1:30.

MR. KATZ: One-thirty? Is that good?

MR. ROLFES: I had a question. Brad, I don't know. What are your plans for discussion after lunch? I just wasn't -- I was looking, I think we covered most --

CHAIRMAN CLAWSON: We have still got the draft completeness. We are not even done with this paper. We got thorium.

MR. ROLFES: Okay. I just wanted to check with you.

CHAIRMAN CLAWSON: And we wanted to go over this memorandum that we have been talking about from our last site visit, and
then just an overview of our path forward.

MR. ROLFES: We have been covering a little bit of each.

CHAIRMAN CLAWSON: This is why I have been trying to sit here and -- thanks, Joe, I forgot all about thorium. So that is what we will do after lunch.

MR. KATZ: So at 1:30, we will reconvene. We are in recess now. Thank you, everyone on the line. See you again at 1:30 or hear you again.

(Whereupon, the above-entitled matter went off the record at 12:28 p.m.)
MR. KATZ: This is the Pantex Work Group. We are reconvening after lunch break, and, Brad, it is your agenda. Let me just remind everyone on the line, please keep your phones muted except when you are addressing the group. Use *6 if you don't have a mute button to mute your phone. Thank you.

CHAIRMAN CLAWSON: Do we want to check with the other Board Members?

MR. KATZ: Oh, yes. I should do that. So let me check and see that our Board Members are on. Bob and Phil?

MEMBER PRESLEY: Bob is on.

MR. KATZ: Hi, Bob.

MEMBER SCHOFIELD: This is Phil. I am on.

MR. KATZ: Great. Loud and clear. Thanks.

CHAIRMAN CLAWSON: Well, this
morning we made it through the first -- halfway through the first item. We still have -- on bounding uranium and thorium, we need to finish up the thorium part. So with that, I will turn it over to Joe.

MR. FITZGERALD: Actually, I think format-wise I guess we just would need a summary from Mark.

MR. ROLFES: In our document that I sent out last week on Pantex bounding uranium and thorium intakes, we had the uranium discussion. Then also we have got a section on thorium.

Basically, to go through some of the points that we have made, there is additional documentation that back up the summarization of this report, but basically we reviewed the potential for exposure to thorium. The potential for exposure to thorium at Pantex was much lower than a potential for exposure to depleted uranium.
We looked at a Los Alamos scientific laboratory 1976 report regarding the health physics and industrial hygiene aspects of thorium. The analysis, documented in this report, concluded that there was no airborne contamination problems associated with the thorium material because of the large particle size involved.

Pantex investigated this on their own as well and took hundreds of swipes of components which they collected and analyzed. They found that a posting of a contamination area wasn't needed to handle thorium and that respiratory protection was not needed either. They analyzed 73 worker breathing zone samples which showed that there was no airborne activity detectable in the air in the Pantex workplace.

Let's see. Just another point here, Pantex plant had been operating under a thorium-232 removable contamination
administrative control level of 40 dpm per 100 square centimeters, which was below the regulatory limit of 200 dpm per 100 square centimeters.

There is also employee-specific bioassay data for thorium, was monitored via nasal swipes, urine and fecal samples, as well as direct radio-bioassay, which would be in vivo lung counting that was done on site.

The years that are covered by these analyses were 1983 forward, roughly, and we have got some references here in the Site Research Database that have the results of these analyses.

Basically, we currently have a 40 DAC-hour intake of thorium in our Site Profile. However, based upon updated information, we have actually looked at the air sampling data available to us, and have analyzed the amount of uranium and thorium in air, using the air monitoring results.
We were able to develop a ratio of how much thorium would be present in the workplace versus how much uranium airborne activity would be present in the workplace, and we have agreed to revise our Site Profile to assign thorium intakes based upon a ratio of the airborne depleted uranium.

The analysis that we were able to do using these breathing zone samples from disassembly operations showed that about two percent of the alpha activity that is airborne during certain operations is a result of the thorium that is present, while the 98 percent that is present in air would be the result of depleted uranium alpha activity.

We also are changing the mode of thorium intake from a previous acute intake to a chronic low level exposure as well.

MR. FITZGERALD: You did say breathing zone. So these were true breathing zone samples that were taken?
MR. ROLFES: That is correct.

MR. FITZGERALD: Kathy, are you still on?

DR. MAURO: Joe, while we wait for Kathy, I got a quick question for Mark. This is John. When you say you have thorium airborne samples, I know in the past, especially when you go back in time, it was just a gross alpha count, and you are not really sure whether it is uranium or thorium-232 that you are looking at.

How do you know you are looking at thorium-232 as opposed to uranium?

MR. ROLFES: Well, what they did, they took a look at the air filters and ran ICP-MS, inductively coupled plasma mass spectrometry, and scanning electron microscopy to specifically identify which particles were uranium and which particles were thorium.

DR. MAURO: Thank you. You answered my question.
CHAIRMAN CLAWSON: Mark, who was doing these swipes and stuff like this?

MR. ROLFES: These were air samples.

CHAIRMAN CLAWSON: Air samples?

MR. ROLFES: The swipes that I mentioned earlier were done by an industrial hygienicist/health physicist at the site.

MR. FITZGERALD: I guess we had talked about trying to normalize across some of these systems and operations. How does that account for, I guess, a sufficient -- conservatisms -- envelope thorium use? You know, it wasn't as lengthy, obviously, as DU.

MR. ROLFES: Could you repeat that?

MR. FITZGERALD: Well, I am just saying, you are using these ratios, and the two percent was based on these measurements. I guess my question goes to the thorium systems that would have been handled. Does
MR. ROLFES: This particular weapons system was also one of the ones that was said to have been a worst case type potential for exposure. It was one of the -- we have identified a list of -- let me get back to it. Wanted to go back to the specific list of -- okay, here at the top of page 4 in our evaluation, the 28 program, since that was one of the ones that resulted in the highest potential for contamination. We also asked about some of the other weapon programs.

One that was responsible for the thorium was also one of the ones that Mason & Hanger-Silas Mason personnel were aware of and knew that there would be a greater potential for exposure due to contamination. So once again, it appears to me that we have chosen the bounding situation for possible thorium exposures.

MR. FITZGERALD: Okay.
MEMBER BEACH: Mark, can you give us a list of the assemblies and disassemblies for everything that had uranium and thorium in them?

MR. ROLFES: Yes.

MEMBER BEACH: From '58 through -- so you can go all the way back to the early years?

MR. ROLFES: Yes, I can tell you the source term for every program.

MEMBER BEACH: Okay. Is that available?

MR. ROLFES: That information is very, very well documented from the beginning of time of our weapons programs.

MEMBER BEACH: Okay, great.

CHAIRMAN CLAWSON: But some of them came on-line and went off-line before a lot of the more stringent monitoring came on-line. I guess I was just wondering how we were going to account for those where we don't
really have any data on them. You know, when we pulled up that chart down to Pantex, I noticed that is a classified document, but numerous ones came on-line that had the thorium issues and the thorium problems, also some other problems, but they also went away quite rapidly, too, for some of those reasons.

You know, we have got data for the `70s era and so forth, and I just -- when does the data actually start that we have the thorium smears for? Is it the beginning of the `70s?

MR. ROLFES: The earliest smear for thorium that I am aware of would have been collected back in 1968, I think.

CHAIRMAN CLAWSON: Maybe this is a loaded question or whatever, but what time frame do we really -- because in the early years they were just trying to get a handle on how to deal with thorium. What time period, I
guess, does NIOSH feel that they have a robust thorium monitoring program that they could really hang their hat on?

Many of these sites, I have never really seen the time frame when you could hang your hat on it, is basically what I am trying to say.

MR. ROLFES: What we have in our Pantex bounding uranium and thorium intakes, since we are proposing to use basically two percent -- we would have assigned a depleted uranium intake first, and then assign an intake of thorium-232 equal to two percent of depleted uranium intake on top of those. So we'd reconstruct the depleted uranium intake and then add an intake, two percent of the DU intake as thorium.

MR. FITZGERALD: I guess, going back to -- you know, we are talking about a particular system. You are talking about assigning a chronic exposure based on DU. But
wouldn't the workers who were directly involved in the dismantling of that particular system be getting more of the direct intake potential?

It sounds like what we are doing is a generic chronic intake factor of two percent for everybody. Right?

MR. ROLFES: Correct.

MR. FITZGERALD: I am thinking about the workers who are working with the unit directly as opposed to the general operator population. I am just trying to reconcile whether they, in fact, are being shortchanged by that approach or not.

MR. ROLFES: I am not following where you're --

MR. FITZGERALD: Well, I'm saying it sounds like -- maybe I am misunderstanding you. It sounds like you are assigning a two percent of the DU as being a chronic exposure for all the operators or just the thorium
operators, the ones working on this particular
system?

MR. ROLFES: What we have -- here
is what we have laid out in our Pantex
bounding uranium and thorium intakes. Getting
back to the uranium intakes, we are using the
1990 bioassay data. If an individual doesn't
have any thorium monitoring in their file, for
example, we would make the assumption that
they were potentially exposed to both uranium
and also to thorium.

So we would assign our uranium
intakes based upon the analysis of the data
from 1959 up through 1990, about 400 uranium
bioassay results which we've proposed here;
assign that uranium intake, and add in an
intake of thorium equal to two percent of the
depleted uranium intake.

DR. NETON: Can I interrupt? Am I
missing something? I thought that the Working
Group is already recommending they can't do
any DU dose reconstructions prior to 1984.

CHAIRMAN CLAWSON: This was my next--

DR. NETON: If that is true, then this is all irrelevant.

MR. FITZGERALD: I know. I am just trying to figure out just --

MR. KATZ: Well, it's not -- but that's just --

MR. HINNEFELD: Our position is we can do the dose reconstruction.

DR. NETON: Right. Right.

MR. HINNEFELD: So if, in fact, the Work Group and the Board determine that dose reconstruction for uranium isn't feasible up through '83, then if we are tying thorium intakes, then they are also -- but now we have the years from '84 forward.

DR. NETON: Right, but that's slightly different, I mean think about what quality of data we have for thorium. So what
I'll say is it doesn't seem productive for us to debate whether we can reconstruct thorium prior to 1984 until the Board meeting because if the Board accepts the Class Definition prior to '84 --

MR. FITZGERALD: This reminds me of the -- wasn't there an issue at Mound where we were going to park something, and then we couldn't do it, because -- as a matter of fact, as far as resources, why don't we wait and apply those resources maybe more efficiently by addressing this later? But just even clarifying the approach would be, I think that's all we're doing is understanding it better.

CHAIRMAN CLAWSON: Well, and I will be honest with you. Maybe this is -- you know, I feel that you guys went to some work on this, and we really haven't got into the thorium issues, and I just wanted to make sure that I understood how it was being put,
because the next question I was going to ask was, with what happened today, how will this hold together?

MR. KATZ: My point of view is just that we don't know what the rest of the Board is going to say about what the Work Group is going to recommend, but they may then have different issues about the thorium. For some Board Members, the thorium may be a more compelling issue than the uranium. I have no idea.

DR. NETON: If the uranium -- if you can't reconstruct uranium --

MR. KATZ: I know.

(Simultaneous speaking.)

MR. KATZ: That is one possibility. Another possibility is Board Members are not persuaded on the uranium question, but they may have issues with the thorium even if they are not persuaded on the uranium, in which case they would need to
understand whether the thorium can be reconstructed.

MS. LIN: -- you can reconstruct thorium.

MR. KATZ: No? I mean, what if a Board Member says I don't buy it; I think the NIOSH argument is fine for uranium, but what about thorium. Then we don't -- any advance through the question on the thorium thing.

So whether you want to just wait and have that discussion, should that arise, that is one thing. But if the Work Group wants to sort of plow that ground now so that it will have been discussed, it is up to you.

CHAIRMAN CLAWSON: I had several questions I wanted to get. First of all, I wanted to understand if this thorium -- because the way I read it, everything was tied to depleted uranium. If we decided we couldn't do depleted uranium, was there -- or do we have anything else so that we could
cover thorium? That was going to be my question, Jim.

I did not understand how the process was, plus also, too, NIOSH has put quite a bit into this, and I know that it was bounding uranium and thorium intake, but they were separated out. I wanted to make sure that I had an understanding of how the thorium process worked, and if this DU problem came into this, did we have another way or is it ultimately tied?

My next question out was going to be do we need to address this as uranium and thorium to the Board because they are ultimately tied together.

MR. KATZ: You don't need to because you have the uranium and your reasons for the uranium. You don't need to address thorium. The only other issue with thorium is that if you add a Class and some people are not covered and you want to reconstruct their
thorium doses, if you have already decided you
can't reconstruct uranium, I think it puts it
out of the ballpark because it is based on the
uranium. If you can't reconstruct the
uranium, then you can't reconstruct the
thorium either. They don't get credit for
those doses either.

CHAIRMAN CLAWSON: Right, and that
is why I was wondering if we needed to address
that along with the uranium because I want to
make sure people understand that that is part
of the -- you can't do uranium or thorium.
Everything is based on it. That was going to
be my next question.

DR. NETON: I think what we are
going to end up with here, if we continue down
this thread, is the quality of the thorium
data we have available establishes percentage
probably much better after 1983.

MR. ROLFES: Yes, there's bioassay
data.
DR. NETON: We are going to have similar issues that we have with the uranium. So I don't know that it is going to be a productive use of our time this afternoon to sit here and debate about the quality of the thorium data we have prior to 1983, if in fact it has been decided that we can't reconstruct uranium.

CHAIRMAN CLAWSON: Jim, I understand wholeheartedly that maybe this is a futile effort, and maybe it can be just summed up to me as the Work Group Chair, because my question was, and what I was hesitant about, is we had only been talking about uranium. So that is how I posed it.

I am wondering if we needed to tie thorium to it, because basically we are into the same ballpark of it. It all depends on the DU. I will be honest with you. The programmic part of this is baffling to me of what we can and how we do it.
MR. HINNEFELD: This is Stu Hinnefeld, and I just want, for clarity -- I haven't studied this or had any conversations, I guess we could have had these ahead of time. But for my edification, trying to get the dates for this data is collected -- hundreds -- in Pantex, their second bullet, hundreds of Pantex swipes of components were collected and analyzed, and that covers -- what time period does that cover?

MR. ROLFES: The swipes that I have seen cover -- you know, I have seen bits and pieces here and there of swipes in the early `60s. However, most of the data that we have available would be during the disassembly time period, which was, let's say, later on in the operating history, during the `80s, `90s.

MR. HINNEFELD: Yes, I don't know that we can talk about it, but do we know when thorium weapons, for instance, were in the surveillance program? I am sure we had to
dismantle them because I have seen the
document that shows the dismantlement
schedule. You guys apparently know which ones
had thorium in them.

So I know we know what years they
had the dismantling. I assume we have some
breadth of years that covers the presence of
thorium in either a surveillance or
dismantlement for both those sections. I know
we have dismantlement.

MR. ROLFES: Yes, we do have that
information.

MR. HINNEFELD: You say you have
seen some data in the `60s, and then there's -
- the bulk of it, you say, was later. Can you
give me a year?

MR. ROLFES: 1980s is when the
true bulk of all the swipe data that has been
collected, at least that we have seen so far.
We hear that data exists from earlier years,
but most of it has been within incident
reports. We found it all in a consolidated box from 1980 and then an electronic database from like 1990 forward.

MR. HINNEFELD: So then the 73 worker breathing zone samples that didn't apparently show any airborne activity date from what period?

MR. ROLFES: That would have been in the `90s.

MR. HINNEFELD: Did you say that if we have thorium bioassay for a person, that we would rely on that for that person? Is that what I heard, or are we relying on the ratio for everybody?

MR. ROLFES: If they don't have data, we would rely upon that ratio. If we do have data on thorium bioassay, we would use that data for that individual.

MR. HINNEFELD: Okay. Certainly, the in vivo would work.

Now then the ratio is established
by samples collected on a specific program, W55. So do we know enough about relative abundance of the items of the two materials for the other thorium series to know that that provides us something we can use for all time?

MR. ROLFES: Yes, and we selected this particular program as a result of the potential for contamination and oxidation that was based upon some of the worker interviews and also the listing of programs that were identified by the Health and Safety staff at Pantex, which were the worst ones that could have presented a contamination potential.

MR. HINNEFELD: Essentially then, the worst of the thorium-containing weapons, in terms of its potential for contamination?

MR. ROLFES: Yes. I would say that.

MR. HINNEFELD: That is kind of what we said, like W28 is the worst based on our argument as the worst.
Okay, you understand where I am going? We have a ratio set up on this one system, and if it, in fact, has the highest ratio of thorium to uranium in the thorium series weapons, then you certainly have a bounding value. If not, there is an argument that can be made that this ratio doesn't bound the potential ratios that would be encountered in other systems. That is just where I am going, just the logic of it. Things don't work out later on.

CHAIRMAN CLAWSON: That was -- part of my question was coming from of the earlier years because some of those weapons came on-line and went off-line relatively fast, and the time period we are talking about here, those weapons would have already gone away.

I am just wondering how that worked because 55 may have been the worst actor, but we really don't have any data for
the other ones. Were they worse or was 55 the one that we have the most information for?

MR. ROLFES: The 55 would have been the worst. It's not the only one that we have information for.

CHAIRMAN CLAWSON: Now this is where it really comes back to me, especially looking at the roadmap or whatever you want to call it, because it was very -- you know, get printout at Germantown, stuff like that, and said this is when it came on-line, this is when it went off-line, and this is what it has in them. We really don't have any data for those.

I just want to make sure that we are bounding it. From the perspective in hindsight, well, maybe 55 was worse or one of the others. That is where I am going at with this, and part of my question came up with that, plus also what Jim has brought up about how it affects us with the DU.
I am wondering of how to be able to handle the thorium issue. I guess that is what I am looking at because we are kind of in a conundrum right here.

MR. KATZ: I think it --

DR. NETON: The Class of workers is the same, all workers --

CHAIRMAN CLAWSON: Right.

DR. NETON: -- or all people who worked --

CHAIRMAN CLAWSON: Right.

DR. NETON: And it doesn't add anything by saying you can't reconstruct uranium and thorium. It's the same people who are going to be in the Class, unless there are people who only worked with thorium.

CHAIRMAN CLAWSON: Yes. You know, they had several different programs that they did deal with a lot of thorium. There were thorium parts that were coming in and so forth like this, and this is why I didn't want to
say that, no, NIOSH can't reconstruct thorium either because this is why I wanted to talk this out -- is, can we? Are we confident we would be able to do that without the data?

MR. ROLFES: We have proposed that we can bound both uranium and thorium intakes for all years, basically, in this report. So that is --

MR. KATZ: But from your perspective, Brad, once you say you can't bound the uranium, since it requires that uranium data to do the thorium, you are saying you can't do either?

CHAIRMAN CLAWSON: Well, yes, that --

MR. KATZ: With the exception of where they have personal monitoring on thorium for an individual -- obviously, those individuals you could use it for some monitoring. It is listed in your recommendation that you are making to the
Board that thorium is a non-starter, too.

CHAIRMAN CLAWSON: Right. This is my issue. I am sorry that I can't express myself better, but in reading this, it is hard to understand what is tied to what, when I was getting this, and when we went through what we did this morning, this was one of my concerns, but we hadn't talked about it yet, and I wanted to discuss it out and see where we go from there.

So, to me, it sounds like that, if we can't -- if the Board recommends we can't do uranium, basically, thorium is going to be right along with it. That's where I was wondering if we needed to put that into the recommendation.

MR. KATZ: I think it is fine to note that as a consequence of this, too, and to note that some individuals will have personal monitoring on thorium, and those individuals, of course, might have their
 thorium doses reconstructed even though they
won't have a complete dose reconstruction.

CHAIRMAN CLAWSON: Okay.

MR. FITZGERALD: Just one more
question. The ratios are based on air samples
that were done on the W55 in June of '96. Now
this is, unlike some of the other analyses and
samples taken -- we are talking about DU --
this was taken after they had revamped the
overall HP control program for contamination
control and air sampling and everything.
Presumably, they had down-draft tables and all
the things that they had installed.

MR. ROLFES: No, they did not.

MR. FITZGERALD: Okay. so for
thorium, they did not have the upgraded
controls?

MR. ROLFES: There is no down-
draft tables on site that I am aware of at
Pantex, you know, with an exception possibly,
but it is not going to be routine --
MR. FITZGERALD: That is interesting because we did get some documentation -- I may have to go back and take another look -- that they had upgraded the control system, including a down-draft table to reduce the dissemination contamination for the DU program -- DU system, W28. This is post-’89. I was wondering if those same kind of controls were put in place for thorium, but you are saying there was no upgrade of those kind of controls after the incident.

MR. ROLFES: I haven't seen any documentation of a down-draft table at Pantex.

MR. FITZGERALD: Okay. Well, that is something that we have a number of sources on. So the notion there was to improve the contamination control, given the fact that you had this stuff that was flying around.

What I am trying to get to in this thing -- I guess we can investigate that
further, but if the controls for the 55
program in '96 -- this is getting way down the
pike -- were much improved, given all the
experiences and lessons learned, then your
samples, I would assume, would be much lower
than what you would have found maybe 10 years
before.

I am just trying to reconcile
whether or not that would --

(Simultaneous speaking.)

MR. HINNEFELD: I think, though,
that would speak to the total activity --

(Simultaneous speaking.)

MR. HINNEFELD: -- but would
really, with the additional controls, would
they preferentially collect the thorium versus
the uranium? It would seem like it would
collect both contaminants to the same extent
in general.

MR. FITZGERALD: Maybe. I was
just trying to think how this is going to be
run out because we are taking very late samples and then using those samples to create ratios to apply back into the '60s, and I am just trying to make sure I understand how that is being proposed.

So the notion is that they would be equivalent -- the ratios would end up being equivalent, irregardless of any upgrades that may have occurred, the major overhaul of the system that took place in '90-'91, that kind of thing. This normalization question, I guess, is what I am raising.

MR. HINNEFELD: Well, it would seem to me that, going forward, there are a number of things that could happen at the Board. The Board could act in accordance with the recommendation of the Work Group, in which case everything '83 and earlier, unless a person got bioassay data that is off the table and we can't be -- then the thorium post-'83 becomes part of the overall post-'83 Class.
We know we are going to continue on. So that is one outcome.

Another outcome would be that the Board would say, well, you haven't convinced us on the uranium; maybe there -- maybe NIOSH has already -- that there is a way to do this. And at that point, where we are today is that thorium would be an open question. We haven't explored it. And this report is pretty recent, you know, people really haven't had much time to evaluate this report. So that would slide it out.

I am just trying to think of what other possible outcome could come out of the Work Group. I mean, I can't. That is pretty much it, right?

MR. FITZGERALD: To me, we could -- I'm sorry, go ahead.

MS. LIN: If the Board also decided that the '84 to '89 data was good enough to be back-extrapolated as the basis
for thorium, you can use only that five years for thorium.

DR. NETON: Right. We still have to address is the thorium back-extrapolation valid as well.

MS. LIN: So you still just have to address --

MR. FITZGERALD: That is kind of what I was driving at.

DR. NETON: The question of the uranium back-extrapolation is on the table. The Board says, well, we agree that they can back-extrapolate, but then the thorium back-extrapolation is still out there.

MR. KATZ: That is what I raised originally.

DR. NETON: And that is what I was trying to point out earlier. We could talk all day.

MR. FITZGERALD: I think that is all I am going to raise, but again it strikes
me that one contrast with the back-
extrapolation issue for DU is the fact that we
are using data that even actually comes more
recent in time and after all the other --

DR. NETON: And it has its own
nuances such as you pointed out, that it is a
ratio as opposed to an actual activity.

MR. FITZGERALD: But I would
disagree. If this is a question we are going
to know in a couple of weeks, then I would --
you know, if there are other questions, we can
certainly look at this later. I would propose
that we wait and do our research after we have
some clarity as to where this goes.

CHAIRMAN CLAWSON: And I
understand that. I'm kind of new to this, and
I just wanted to make sure, though, that what
I was addressing or bringing forth before the
Board was correct and that I could portray it
to them in the right content because there are
things that are tied to it.
When I read this, I didn't know if there was another process that NIOSH had that, no, we can do this. I didn't want to go in and say that you can't do thorium either if NIOSH did have something else. That is why I was bringing up the question, is if I ought to, in bringing this to the Board, bring it up as uranium and thorium.

Jim, I understand fully what you are saying. I guess, more for me, I was wanting to make sure that what I am presenting to the Board and also to the public is correct and not saying something that you guys might have something different.

So is there any more discussion on the White Paper of uranium and thorium that anybody wants to address? Bob or Phil, do either of you have anything that you wanted to say on thorium?

MEMBER PRESLEY: I am fine right now. This is Bob.
CHAIRMAN CLAWSON: Okay, thank you, Bob. And nothing from Phil? Maybe we can continue on.

MEMBER SCHOFIELD: No, I don't have any comments there, Brad.

CHAIRMAN CLAWSON: Thanks, Phil.

Okay, NIOSH's response to the SC&A Draft Completeness and Adequacy Review for the Pantex Plant. I guess, Mark, it is up to you.

MR. ROLFES: All right. Let me see if I can find my hard copy of this. All right.

I guess a lot of the same issues that we have been discussing have primarily been related to, you know, our dose reconstruction methodology for the earlier years. How do we account for the lesser numbers of uranium bioassay? There were some questions regarding external dosimetry.

I don't know if we want to go through each of the issues that are presented.
I mean, the first issue here was that not all workers were monitored, and we understand that. There is a reason why early workers weren't monitored, really because there were no radioactive materials on site. They were focusing on the production of high explosive components.

The only people who were monitored in those very earlier years were the radiographers. As you see fissile materials coming onto the site in the late 1950s, you see an increase in monitoring, external dosimetry, and then also as well you see the bioassay sampling program beginning in 1959.

I guess on to -- I don't know. Would you like for me to go through each of the sort of summary issues?

CHAIRMAN CLAWSON: I guess we could question that not all the workers were monitored and so forth like that.

MR. FITZGERALD: Can I address one
thing, though?

CHAIRMAN CLAWSON: Sure.

MR. FITZGERALD: This is one of these "ships passing in the night" issues. The Data Completeness Report -- you remember our discussion in the May Working Group meeting was held up in DOE review for about four months, and didn't actually get forwarded until about the time of the Work Group meetings. We couldn't address it there.

During the Work Group meeting, going through this revised matrix, we actually closed some of the issues or at least dispositioned some of the issues, and that account is in here, I believe. Yes, down below, if I can refer you to this package.

So this is kind of a curious situation. We had issues back in December -- November-December when this White Paper was put together on data adequacy, and it got into the system and popped out of the system after
six months. In the meantime, we had a Work
Group meeting where we engaged on some of
these issues and reached some degree of
closure.

I just want to provide that
backdrop because I would certainly hate to re-
fight some -- or not re-fight, but re-discuss
some of these issues that, frankly, we've
already were able to disposition at the last
Work Group meeting, and that is all, I think,
laid out in this matrix.

So what I would suggest, because I
sort of recognize this issue now because of
the timing question of these different
reports, is that perhaps we could review this
-- we have only had it for a couple of days
anyway; so we haven't had a real good chance
to look at even the reference documents and
data that are behind this -- and disposition
it with a view toward what occurred at the May
meeting.
So if it has been closed, sort of God bless, you know, we are fine, and whatever issue we might have had originally we were able to work out, and just kind of narrow it down to what may still be outstanding issues and maybe see where we are going with that. I think in the Germantown discussions, I think we narrowed it down to issues like uranium and thorium, and felt that some of these other issues, while legitimate, were -- and I think you say this in your piece -- were beginning to tilt toward Site Profile.

So maybe we need to take a good look at this, array it against the results of the May Work Group meeting, and see what is left. I don't think, frankly, there are a lot of big issues left. There are some issues that we need to disposition.

MEMBER BEACH: Joe, with that, when did you -- the latest matrix I have was May 2011.
MR. FITZGERALD: Yes.

MEMBER BEACH: Is that the most recent?

MR. FITZGERALD: That is the most recent, and --

MEMBER BEACH: Has that been updated to reflect the minutes or what we discussed in the May meeting?

MR. FITZGERALD: This was updated May 25th. So, yes.

MEMBER BEACH: Because mine still says March. It says cover letter written May 2011, and then in the body of it, it says at the bottom -- it says March 27, 2008. So that is why I was questioning whether I --

MR. FITZGERALD: Yes, I had the one that has updated May 25, 2011 on the lower righthand corner.

CHAIRMAN CLAWSON: You are right.

You guys haven't even had time to be able to really look at that.
MR. FITZGERALD: We can go through it. I just want to suggest that, you know, this is maybe a little confusing, given the progress that was made on some of these issues at the last Work Group meeting, and the fact that the White Paper on data completeness was held up so long that, by the time it did get in, we had made some progress on some of those issues, and that is laid out. I am sure it is laid out in the minutes, but I think, based on the update of the matrix, that maybe it will take care of it from that standpoint. It is up to you.

MR. KATZ: I am just thinking, between the new paper and Mark's presentation on that and the matrix, you are going to need to, at this Board meeting, also cover that topic. So to the extent that -- some of it may not be resolved yet because you haven't had a chance to dig into the new material, but to the extent that things have been resolved
and then you know what work there is to do, it
would be good to be telling the Board where
that all stands. So whatever discussion is
useful for that, you should have.

MR. FITZGERALD: Yes, except we
haven't had a chance to really go behind the
new analysis, given the fact we have had it
for a couple of days. I am just saying that
we can listen to the presentation, but I don't
think we have anything to clarify or respond
to at this point. It is just too early.

It is up to you, Brad. What do
you want to do with this?

CHAIRMAN CLAWSON: I don't know
really what benefit it would bring. We have
got several things that have come to closure.
This was, at the last Work Group meeting, I
believe, was something that we were waiting
for from NIOSH, and you guys haven't even had
a chance to review what has been said.

I think it would be -- I don't
think it would do us any good to go through this unless there is something that Mark wanted to bring out specific that changed. I notice that there was a few things in there that NIOSH had decided to change and, I guess, give Mark -- I guess my biggest thing was to make sure that Mark had an opportunity to express this paper that they did all this work on off-line.

MR. ROLFES: Well, thanks, Brad. I appreciate that. I think everything -- you know, the majority of everything -- as Joe did say, we came to agreement on just about everything. It was the basis for reconstructing uranium and thorium intakes, which was the primary issue that we left with at the last Work Group meeting.

I think we have come to agreement or decided that, you know, the other issues aren't SEC issues, that we are able to bound doses for any of these other issues, and so we
sort of left it as Site Profile type issues.
Which correction factor do we apply for this
or which correction factor adjustment do we
make for that sort of issue?

I don't believe, you know, there
is anything -- the most significant things
that we put together for this meeting were
really the information that allows us to bound
uranium and thorium intakes for all
operational years.

CHAIRMAN CLAWSON: Then, to me, I
guess this is basically in your hands, Joe.

MR. FITZGERALD: Yes. I'm just
saying, for efficiency's sake, since we have
actually made progress on these issues and,
for example, tritides and some of the other
ones, that I think we have agreed that these
have tilted toward Site Profile questions.

I think for purposes of the
meeting coming up, we can go through what
happened at the May Work Group meeting, you
know, certainly reflect this White Paper and share with you back where we think things stand on the balance of the issues outside of uranium and thorium, and then try to characterize that as accurately as we can, understanding that there were a lot of things that were going past each other at the same time.

So we will take that on to go ahead and work that issue, and be able to put in your hands collectively now here is what it looks like as far as where these came out. I don't think we got transcripts for May, did we, or did we? We do now? Okay.

CHAIRMAN CLAWSON: Yes, there was kind of a mix-up. It got kind of lost.

MEMBER BEACH: No, the January one did.

CHAIRMAN CLAWSON: The January ones got lost.

MEMBER BEACH: But they are both
out.

MR. KATZ: They didn't get lost, but anyway --

CHAIRMAN CLAWSON: No, excuse me. They didn't get -- they hadn't reviewed them, and I just got a deal yesterday to review them and go from there, and I thought, well, wait a minute, I just took these and -- well, then --

MR. FITZGERALD: I will certainly make use of the May transcripts just to make sure that everything seems to be where it needs to be as far as what came out, so there is a record, a public record that brings us up to date. But that would be, I think, the way to deal with this one.

CHAIRMAN CLAWSON: Okay. Well, and I also know that you haven't had time to see it much.

So we can continue on. We have a summary, and I know that we worked on this down at Pantex.
MR. FITZGERALD: Yes, let me make sure. Is Kathy on the line? She put her heart and soul in this piece. I don't want to shortchange, if she had any questions for Mark. I don't think she is, though. Okay.

CHAIRMAN CLAWSON: I know that we went through quite a bit to be able to get this memo out, and I wanted to know if you -- we have gone quite in depth through it, but if you want to give us a summary of what we found down there and what we learned.

MR. FITZGERALD: Yes. We focused on the W28, just because out of the Germantown meeting, given the timing of the on-site visit, it was natural to try to pin down some of the questions that came up in our Germantown meeting as far as gaps in our knowledge and timing questions and scoping issues.

So I wanted to highlight that in this note that we sent through DOE to make
sure it was clear. These are just basically
the highlights. The originals are in
Germantown, and I would invite you to look at
the transcripts of the interview we did.

The interview was pretty good. We
talked to HPs and rad techs, but we finally
got around to talking to a sort of operating
site engineer who seemed to really have his
finger on a lot of the operational issues. We
found that quite valuable and got a lot of
insights out of this. We allude to some of
the comments that he had on the W28, but he
also had a number of general comments on
operations.

At any rate, the highlights on
this thing, again, is that -- I think I said
this earlier -- is that you did have the 28s
coming through on a continuous basis,
different types of dismantlements, that he in
his view felt there were at least two more
incidences or releases before the one that we
have been talking about that were, in his view, as bad, if not worse, from the standpoint of contamination. But, again, that was a recollection.

MR. ROLFES: Are you aware of which incidents they were? Did he mention --

MR. FITZGERALD: No. We were trying to -- obviously, trying to pin down, but off the top he couldn't remember exact dates. But what he was trying to say is the same thing that, I think, we have been asking, too, which is what can you recollect 20 years ago in terms of these incidents.

His take, though, on the '89 -- is it '89 or -- '90 is the data; '89 was the release. His characterization, and he was there -- he was assigned to the 28 program at that time.

His take on it was simply, you know, we had these releases in the past, and what changed in '89 was the environment that
was going on in the Department in terms of the scrutiny, and the workers were becoming more mindful and they finally raised their hand, and management in that time frame really decided to take action.

It wasn't so much the incident that pushed things. It was just, frankly, the circumstances that the plant was under and the fact that DU was finally seen as a radiological issue that had to be addressed. He felt that that is what kind of made this thing as noteworthy as it came to be, and that other than that, it was business as usual.

Before that, they had releases of the same nature, and there wasn't that cloud. You know, the circumstances weren't such that the workers would have raised their hand, and it is tough for a worker in the circumstances of Pantex to complain about contamination, as you can imagine.

So, yes, the circumstances in '89
were such that this happened, but his point was it could have happened well before that, given the actual contamination situation with the 28.

CHAIRMAN CLAWSON: And, Mark, he did call out that it wasn't numbers; it was the inner parts of it. He said, these problems will but, you know as well as I do, we are not going to be able to talk outside of the confidential area.

When we put it out -- and what came forth to me was when we said, and we understand that the 28 was really the worst one, he said, no, it wasn't; there's just more who were working on it. We had this, that it is just the change of the environment in this time that it became more brought forth. He says, in my personal opinion, we had several ones that were a lot worse. But he tied them to an object and stuff, you know, that we won't be able to discuss.
MR. ROLFES: That is why I was asking. You know, I wondered what the specific occurrences were because we do have documentation of other incidents similar to the B28. That is why I wondered. I wanted to get a sense of which ones he was referring to, so that we could do a comparison or a source term analysis to basically ensure that the W28 exposure pathway is truly bounding. That is why I am trying to ask for what specific sources.

MR. FITZGERALD: It was the 28, but he couldn’t pinpoint the actual release time frame or, if there was an incident, what was the incident that might have been worse. We were trying to get some more specifics, just so we would have that, but he could not remember. And to be fair about it, 20 years -- I just wanted to understand, did this stand out from a magnitude standpoint to be the most significant release or -- and his response...
was, not from a magnitude standpoint, just from the standpoint that management -- it was significant from a managerial standpoint, and management responded. That was the difference with that particular incident, more than anything else.

MR. ROLFES: I am just saying, I have seen similar responses earlier on besides the W28 incident. That is why I wanted clarification because there are bioassay data collected as a result of other contamination incidents that occurred with different weapon programs surrounding this time period as well.

MR. FITZGERALD: He did say it was the 28, but he just couldn't pinpoint any particular event as a certain date or -- that was all we could get out of him at that point.

MR. HINNEFELD: And you interviewed this person this year?

MR. FITZGERALD: This was the last trip, right. The raw notes are in Germantown,
and the name is there, too. So it is all --
He is down there. He is still working.

CHAIRMAN CLAWSON: Actually -- and
this is what is interesting about him -- he
lives and works at Pantex and has for numerous
years, but he works for Los Alamos now, as
being there. This is why he didn't show up as
a Pantex employee, but he is there at Pantex.

MR. ROLFES: And there is Sandia, Livermore, Pantex people down there.

MR. FITZGERALD: That is why he
didn't jump out earlier because he wasn't
listed as a Pantex employee.

The other thing that he indicated
-- and this goes to the earlier discussion we
had on the pre-1958. He sort of said, well,
you know, just so you know, while the W28
series was apparently the earliest series that
was viewed significant at Pantex, units
containing DU from earlier series -- the Mark-
15 Mod 2 was an example -- may have been
handled earlier at Pantex.

So he was just cautioning that, as far as DU significance, there were systems predating the 28 that we ought not just forget about or not at least address that early on. So he did mention that.

MR. ROLFES: That is pretty much the same kind of program as the W28.

MR. FITZGERALD: And you can read the memo. I mean, they were in disassembly and heavily oxidized units.

The initial work was done in the absence of gloves and respiratory protection. If a worker requested a dust mask, the company provided one, and this is the approach pretty much through '89, at which point down-draft tables were installed.

So I actually read that. Before we interviewed him, I had read that elsewhere. So that would be something I would love to know for sure because I have heard it in more
than one place that down-draft tables were installed after the incident for purposes of contamination control.

We talked about worker categories.

MR. ROLFES: Joe, could I ask a question about the previous statement there?

The initial disassembly of all both clean and heavily oxidized weapons doesn't involve gloves or respiratory protection because you have got a complete unit coming out of the field. You know, the initial work is done outside of a Gravel Gertie where there is essentially no potential for exposure until you really start breaking the thing down into pieces.

So all initial work -- I mean, the first initial work might be, you know, some loosening of some screws or bolts or something. So I just wondered if -- what you meant by the initial disassembly. I was going to say, the initial disassembly of any kind of
MR. FITZGERALD: Well, I didn't say initial disassembly. I said during the disassembly of heavily oxidized units.

MR. ROLFES: The initial work?

MR. FITZGERALD: The initial work was done in the absence of gloves and respiratory protection.

MR. ROLFES: I am just saying that both heavily oxidized as well as clean, you know, because it is not every unit is oxidized, obviously, and the majority of them, in fact, aren't. So I just wondered. You know, the initial handling of any component would be done without gloves or respiratory protection.

MR. FITZGERALD: Well, I think in terms of dismantlement down to the DU, what we are getting from the interview and from the documentation was just that they did not have the respiratory protection. They could, in
fact, get some of these items, but they had to request it. So that was just the distinction that was being offered. It wasn't a rigorous procedure to wear that or don that.

MR. ROLFES: Okay. I just want to point out that we wouldn't reduce the intake that we assign based upon respiratory protection factors.

MR. FITZGERALD: No, no. I didn't put it there for that intention.

The next issue was simply to point out the one we talked about, that the types of workers -- again, this is from the interview -- types of workers that would be implicated in terms of the DU contamination were the ones I have listed.

Not surprisingly, they involve sort of a broad range of people that would frequent those areas, not just the operators, but technicians, the maintenance staff, safety people, handlers, folks that actually moved
things in and out of the bays, and that we talked about the beryllium as an issue, just as a marker, if you may, for the possibility that, in fact, the DU was likewise spread in the same way.

That was the comment that was made again by the engineer who was familiar with this thing, and he brought that up. You had a question?

MR. ROLFES: I was just thinking. I mean, you know, it is hard to compare, because I don't know if -- you know, there were different requirements for fissile materials and high explosives where that work had to be done. So beryllium work might have been done somewhere outside of the bays themselves.

So it is not really a direct -- I am just trying to think. You know, it could speak, obviously, to contamination being found in other parts of the plant, but it doesn't
necessarily say that uranium would also follow.

MR. FITZGERALD: No, I think it is just a cautionary note that before they did the beryllium survey, they felt the beryllium was confined to the actual handling locations, and it turned out it was actually fairly widespread. But that is not uncommon at other DOE sites, actually.

In this case, I think the issue is what would have precluded spreading that kind of contamination outside the cells when you have, as noted in the Albuquerque audit, that you really didn't have a rigorous contamination program and self-monitoring and other things, egress monitoring, that would have guarded against someone having it on their coveralls and going out into the hallway or the office areas, and over time that, obviously, would have contaminated those areas. So just again an observation.
It actually bears on this question of what workers would be located in this.

This one is baffling to me, because -- well, I will have to go back. We did request the documentation. We had material, actual material, at Pantex that should be in Germantown that talked about the spectrographic analysis of black powder.

This came up in our discussion, as you recall, a couple of times before that, that even though workers were complaining about being covered with black powder -- I think, Mark, either you or somebody, maybe it was Mel, was talking about, yes, but this was hardly depleted uranium; it could have easily been a lot of other things, including lead.

So we were looking for that when we down to the site, and actually we did find a document that nailed it down because when they had the incident, they actually did a spectrographic analysis following the incident
to establish what the composition was.

I don't know if I read a different document, but what I reported was predominant presence of uranium followed by smaller concentrations of lead, chromium, cadmium, and other metal compounds.

Jim, what you were saying seemed to be the opposite. So that is puzzling to me, and when I get back to Germantown, I will go back to that document and get you a specific reference. It is not cleared yet, but --

MR. HINNEFELD: It will be in the holdings in Germantown.

MR. FITZGERALD: Yes. So the spectrographic analysis following the incident -- the black powder, I think, is very germane, but what I recorded, it was mostly depleted uranium but did, in fact, include other concentrations of metals.

DR. NETON: Well, that's
inconsistent with how they followed up the incident, though, because they took blood lead samples because they felt that the blood was going to be a more sensitive indicator because the lead was higher than the uranium.

MR. FITZGERALD: Like I said, I say it is baffling because it is baffling.

DR. NETON: I am not saying the blood level's the right way to go.

MR. FITZGERALD: Well, there might have been enough lead because I had lead recorded second. It might have been enough lead.

DR. NETON: It was -- two percent, and uranium was under one percent.

MR. ROLFES: There were also zinc protoporphyrin samples that were taken as a result of that incident.

MR. FITZGERALD: And this is a secondary issue because we are not talking about how big the source term is per se, but I
again wanted to hit that issue while we were down there because of the questions that were raised about the black powder.

The last item, you know, we did look at Medina-Clarksville, and I don't know if that is germane for this work -- probably not. So I will leave that out, but again there is some information that was coming out of that, I guess the Work Group did -- but it is not germane to the SEC question. So I am going to leave that out unless you feel otherwise, Brad, on Medina-Clarksville. It is in the memo.

CHAIRMAN CLAWSON: Right. It is just that it pertains a little bit to Pantex because at this time Pantex was shipping to Medina and Clarksville, and probably this Work Group -- you know, we are kind of hitting all three of these, and the one good person that came from -- from Medina?

MR. FITZGERALD: Yes.
CHAIRMAN CLAWSON: -- to Pantex
that was still there that is seventy-something

MR. FITZGERALD: Yes. He was the
interviewee.

CHAIRMAN CLAWSON: Just in terms
of the DU because -- the question being, Ted,
is because the earlier years DU was there a
lot sooner than '58.

MR. KATZ: At Medina, you mean?

CHAIRMAN CLAWSON: No, at Pantex,
but we couldn't -- we found shipments of HE
which -- along with, but we couldn't find
anything that called it right out. They
called it by a part number, and we couldn't
find anything to tie it to that.

You know, we can keep that with--

MR. KATZ: Exploring that early
period.

CHAIRMAN CLAWSON: Right, with the
earlier periods.
MR. FITZGERALD: But there is a considerable amount of information that was collected on that interview as well as the documentation on Medina and Clarksville that is also in Germantown. So I think a lot of progress was made on that, too. So just for the record.

That is kind -- again, that is the tip of the iceberg. That is all we could get through the system for this meeting.

CHAIRMAN CLAWSON: And that took a lot of effort, which I have been in contact with Mr. Lewis on, and he is trying to assist me. I know that we have kind of taken a hit with the problems with Michael, too. So I am sure that Isaf will help us through that, and we will go from there.

I guess my question is is we have got the Work Group's recommendation for the full Board --

MR. KATZ: Yes. I would just like
to suggest that Joe help you put together some
flesh on the bones for when you make your
recommendations, so that you have sort of some
bullet points on what is the basis behind for
what you have discussed today, so that you can
-- you know, because the Board -- this is all
coming out of the blue to the Board. So they
are going to need some context.

I am planning to send to all the
Board the documents that we have that we can
provide.

CHAIRMAN CLAWSON: Right.

MR. KATZ: As well as transcripts,
so that the Board can review all that material
and have some background when they hear from
you, but I would try to do a reasonably robust
presentation because, again, they are coming
at this pretty new, to be frank.

And likewise for DCAS. You know,
there's two stories to tell, and DCAS will
want to tell a story, too, to help the Board
get up to speed and perspectives on these
matters.

MR. ROLFES: Just as a matter of --
if it is for the next Board meeting, though,
the problem is our presentation deadline has
already passed for the meeting. So if we were
to give a presentation to the Board, it would
just have to be like a verbal thing because it
is sort of past our --

MR. HINNEFELD: I can waive that.
We will get something together.

MR. KATZ: Okay. I think it is
important that the Board get some context.

MR. HINNEFELD: What we presented
here was essentially a weight of the evidence
argument. Here are some pieces of things, and
these are our reasons to believe that what we
presented is a bounding approach for the work
there. Okay.

So our presentation should be very
concisely that, you know, not a lot of other
stuff, not a lot of talking about -- you know, just these are the pieces of information that we are using to conclude that this period was the bad period, and we can bound it based on this data set, and that's it. That is essentially the extent that we would do.

I don't see it being particularly long, but we really should provide, because you know, our Evaluation Report still says dose reconstruction is feasible. We need to provide a concise description of why we think that is true.

MR. KATZ: Right. And, again, they will have the transcripts from the Work Group meetings that we have had previously. They won't, obviously, have the transcript for this one, but that is part of what will get accomplished in the summary that Joe helps Brad with. Circulate it if you can -- if you have time.

MR. FITZGERALD: Right. I think
it certainly needs to be up by early next week, and I will use words that are already on the table and have been screened by DOE -- or not screened by DOE, but -- yes, screened by DOE, so I don't have to send this presentation through. It won't be ready by then.

MR. KATZ: And it should be completely derivative.

MR. FITZGERALD: Right, derivative and focused, as Stu is pointing out, on strictly the issue at hand which is the depleted uranium.

MR. HINNEFELD: Yes, and particularly why is something -- I think we have presented the various things here. We should try to get it together pretty quickly because your discussion is going to be why is this not convincing to us.

MR. KATZ: Well also touch on thorium and touch on completeness of data, too.
DR. NETON: Why don't you believe we can extrapolate before --


MR. KATZ: Just so that they have a sense of what was discussed here in context with the transcripts that they get that lead up to this meeting.

CHAIRMAN CLAWSON: The January transcripts -- I haven't reviewed them for accuracy yet.

MR. KATZ: But that is fine for the Board. The Board can get them before -- it is not a problem for the Board.

CHAIRMAN CLAWSON: They are online.

MR. KATZ: It would be good to get them, be able to review them, the public -- well, the public gets it, too, PA cleared. They just don't get the one with your stamp on it.

CHAIRMAN CLAWSON: Right. I just
wanted to make sure that you knew that these just came to me, and I am --

MR. KATZ: No, that is fine. So it is not a worry that the public has it, too, in one version.

CHAIRMAN CLAWSON: Okay. We are through the agenda. I want to make sure that everybody has had an opportunity to voice what their concerns are, or if there are any questions on our path forward.

I have several, to make sure that SC&A is covered by their tasking or whatever, but there shouldn't be any ongoing problems, and when we bring this up, we are going to look at the earlier years and on, but what we are going to bring before the Board is '58 to the end of '83, and that we will continue, because I want to make sure that the petitioners understand that things have kind of changed a little bit today and that we are still looking at that. I just want to make
sure that we are there.

My question -- and I know -- I
guess my question is do we tie -- how we tie
thorium to this DU concern. Do I need to
bring that up? Do I need to change what I am
bringing forward to the Board and add thorium
to it or would it be understood that thorium
and uranium --

MR. KATZ: I think you just need
to give the context that this will also mean
that thorium can't be reconstructed because
thorium is dependent on the uranium
reconstruction, the DU reconstruction. I
think that's probably adequate.

CHAIRMAN CLAWSON: You know, we
came out with Medina and Clarksville on here,
and I just wanted to help everybody understand
why this kind of played into it. It is
because this is probably going to be the Work
Group that is able to do those, but also, too,
when we go to these sites, it better utilizes
our time, because it is interesting that Los 1
Alamos and Sandia and everything else like 2
that is a large amount of the data for these 3
three sites, Pantex, Medina and Clarksville. 4
There is a lot of data there, and that is why 5
SC&A sometimes gets double things that they 6
are looking at down there. That is why we got 7
into that.

MR. KATZ: I would just also note 9
for your thought about the Work Group taking 10
on then the new assignments and so on, and we 11
have Sandia coming up, too. Right? A Work 12
Group on Sandia is getting assembled, I 13
believe. Right? Jim has asked for volunteers 14
for that, and we actually have a couple of 15
more Board Members who will get cleared, who 16
will get clearances to work on these Work 17
Groups, too. So that is going to be helpful.

MR. FITZGERALD: And by the way, 19
that is going fairly well with Sam, just to 20
kind of weave us in on it. So we are not
starting from scratch.

MR. KATZ: Yes. So that has been very good, I think, the coordination on that.

MEMBER BEACH: So can we go over action items? I have a couple of them listed, but I wanted to make sure I didn't miss anything.

For -- because I know NIOSH is going to look at the uranium for the years '84 to '91 and then '51 to '58. Correct? I think that was the only assignment you guys have.

DR. NETON: Fifty-one to '58 is what it is.

MEMBER BEACH: That one is not going to be included?

DR. NETON: Well, I had thought that our only action item was to go and put forth our model for -- analysis model for 1988 '89 -- based on the 1990 data.

MR. HINNEFELD: Yes, it would be '84 to '90, actually, and then presumably
there's a routine --

DR. NETON: That was our action item, to shore that up and explain our rationale.

CHAIRMAN CLAWSON: And SC&A was going to, after they get that, review that, but they were also going to continue the earlier years. When we were down there, we -- that kind of falls into SC&A for the earlier years.

MEMBER BEACH: And then SC&A is going to review the responses for NIOSH on the data adequacy and completeness paper?

CHAIRMAN CLAWSON: Yes.

MEMBER BEACH: And then the matrix?

CHAIRMAN CLAWSON: Right.

MR. KATZ: Very good. Thank you, Josie.

CHAIRMAN CLAWSON: And Joe is going to help me put a slide together.
MEMBER BEACH: I put that down on my list for you.

CHAIRMAN CLAWSON: It is not on my agenda to do. Also, to -- and this is kind of SC&A's and also NIOSH's -- is to make sure that all the documentation that the Board would need -- do you want us to run that through you and disseminate it?

MEMBER BEACH: Ted said he was going to send it.

MR. KATZ: I am going to send to all the Board Members transcripts and the products that have been delivered to the Work Group that can be disseminated. So all those things I will send, and I will also ask Zaida to put it in the folder, so it is in the Board's folder, if it is not already there, and it will be on the memory sticks for the Board's computers. So I will take care of that.

CHAIRMAN CLAWSON: Yes, I just
wanted to make sure that they have the information.

MR. KATZ: I am going to do the same for Fernald, for that matter, tomorrow. I will talk about that tomorrow.

CHAIRMAN CLAWSON: Okay. And we have gotten the up to date matrix?

MR. FITZGERALD: Yes. It is up to date as of the May 4th Work Group meeting.

MR. KATZ: Was that distributed to the whole Work Group? Okay. So then I have that. So that would be part of what I will -- they will get everything -- everything that the Work Group has had to consider.

MR. FITZGERALD: Before that, I think there was a March 10th piece that was an email that was sent out.

MR. KATZ: There's two -- Right. There's two memos and whatever.

MR. FITZGERALD: Whatever, different types.
MR. KATZ: You will all be copied on that. So if I miss a piece somehow, by all means, let me know when I send that. If there is another piece that I have missed, please cover me.

CHAIRMAN CLAWSON: Yes. I just wanted to make sure who was going to do that or so forth because I want to make sure that gets out to the Board as soon as possible.

MR. KATZ: How many transcripts -- we only have three transcripts, three meetings on Pantex?

MEMBER BEACH: January, March.

MR. FITZGERALD: Three Work Group meetings.

MR. KATZ: Right. That is what I am saying. Including today or this is the fourth?

MEMBER BEACH: I think it is today, including today.

MR. HINNEFELD: There won't be a
MR. KATZ: No, there is no transcript of that.

MR. HINNEFELD: A series of redacted interviews was sent to us within the last month or two ago. It was redacted and provided to the Working Group. Did Nancy send them? I can't find the message from Nancy about that.

MR. ROLFES: It was last month. It was from July.

MR. KATZ: Yes, I recall it.

MR. HINNEFELD: And it is a series of interviews?

MR. FITZGERALD: It is all the interviews up through --

MR. KATZ: August of last year.

MR. HINNEFELD: Okay, thanks, Mark. I couldn't find it.

MEMBER BEACH: So it is July 2011?

That is the document? It is July 2011 is the
Pantex site, expert interview summary.

MR. KATZ: Right. Correct.

MR. HINNEFELD: Okay. I'm sorry,
I have that one.

CHAIRMAN CLAWSON: So everything
that Nancy sent out and stuff like that will -
- Okay.

With that, is there anything else
that anybody feels we need to discuss? Phil
or Bob?

MEMBER SCHOFIELD: No, not on my
part.

MEMBER PRESLEY: I'm in good
shape.

CHAIRMAN CLAWSON: Okay.

Appreciate that. As soon as we get something
together, I will send it out to the rest of
the Work Group, my presentation and so forth,
and if that is it, we are adjourned.

MR. KATZ: Thank you, everybody.

(Whereupon, the above-entitled
matter went off the record at 2:50 p.m.)