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

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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SEC ISSUES WORK GROUP

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TUESDAY
OCTOBER 28, 2014

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The Work Group convened via teleconference at 1:30 p.m., Eastern Daylight Time, James M. Melius, Chairman, presiding.

PRESENT:

JAMES M. MELIUS, Chairman
GENEVIEVE S. ROESSLER, Member
PAUL L. ZIEMER, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
BOB BARTON, SC&A
NANCY CHALMERS, ORAU Team
HARRY CHMELYNSKI, SC&A
STU HINNEFELD, DCAS
TOM LaBONE, ORAU Team
JENNY LIN, HHS
JOYCE LIPSZTEIN, SC&A
ARJUN MAKHIJANI, SC&A
JOHN MAURO, SC&A
JAMES NETON, DCAS
LaVON RUTHERFORD, DCAS
DANIEL STANCESCU, DCAS
JOHN STIVER, SC&A

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

(1:32 p.m.)

MR. KATZ: Welcome, everyone. This is the Advisory Board on Radiation and Worker Health. This is SEC Issues Work Group. For everyone on the line, the materials that may be discussed today are posted on the NIOSH website along with the agenda for the meeting. And that's under the Board section, scheduled meetings, today's date.

So folks on the line, you can follow along with the documents to the extent that they get referenced. I'm not sure they'll all be referenced today, but they're there.

We're not talking about a specific work site, so I don't think we need to address conflict of interest for any of the Board Members or for staff. So I think

that covers it.

And I already have the Board Member attendance because I have Dr. Melius, Chair, and Dr. Roessler, and Dr. Ziemer on the line. But do we have any other Board Members that are on the line? Okay. How about NIOSH/ORAU team?

(Roll call)

MR. KATZ: Okay then, Jim, it's your meeting.

CHAIRMAN MELIUS: Okay. Welcome, everybody. This is a continuation of a meeting we had in, I believe in Idaho Falls where we were just working on sort of issues related to coworker data sets. And some of these were sort of statistical issues that we had started with some time ago.

But we also felt that it was important to come up with some sort of broader, more general criteria for the development of an evaluation of coworker data sets.

So Jim Neton has been working and putting together a document on that effect with input from the Work Group and others to that. So I expect that we'll spend most of our time today on the first item here to that.

There are a couple other issues that are somewhat related to this, the second and third items on the agenda. And then I'm not sure how much time we'll have to get to those, and then we also have, we just want to, we briefly talked about what we will do for the Board meeting, which is coming up very shortly, in fact next week.

So I think I'll start by turning it over to Jim. I believe you're going to start the presentation and then we'll react to that as we go through it. I'm not sure exactly what your plans were.

DR. NETON: Sounds good to me. Okay, I just want to give an update as to what NIOSH has been about since the meeting

we had in Idaho.

And to start the discussion, well I've been busy revising the implementation guide, the draft implementation guide we talked about at the last Work Group meeting.

And then I said in my transmittal of October 13, my email that I've tried to incorporate comments from the 250 page transcript of that meeting as well as some individual input I received from Dr. Melius and SC&A.

But before I get to that, I want to talk a little bit about the evaluation and differences between strata that we discussed, as well, at the last meeting.

If you recall, NIOSH has been attempting to come up with some alternative way of determining whether or not data sets should be stratified. The first path, of course, was in RPRT-53 that talks about the Monte Carlo permutation test or the Peto-Prentice test.

And those are based on purely statistical criteria. So we set about trying to look at other ways that might be employed. And the first task after that, you remember we looked at the significant difference in dose, the 100 millirem dose.

And we evaluated that. It turned out 100 millirem didn't make any difference in the PC for all the cases we evaluated. But that didn't seem to go anywhere as well.

And then we put forth the concept on looking at the 95th percentile that we would assign for a heavily exposed worker versus applying the coworker model, a full distribution of that coworker model if the data were indeed stratified and more representative of that worker population.

I've got up in our Live Meeting here just to refresh everybody's memory, the short report that Daniel Stancescu and I put out on July 7 which kind of went through the basis of that.

And at the end result, I would just like to go back to the table that we generated which showed that, at least in our mind, that it would take at least a factor of two difference in the geometric mean of the stratified model to be more claimant-favorable than just merely assigning a 95th percentile.

That seemed to be pretty solid. And I don't know if you can all see the Table 1 that's on Live Meeting, but we evaluated all the cancers that have IREP models.

And they range anywhere from the geometric mean difference of 4.1 all the way down to the lowest value which was 2.07 that covered the urinary organs excluding the bladder. So that was sort of the worst case scenario in our mind. It would be at least a factor of 2 difference.

Now just to refresh everyone's memory, again the --

MEMBER ZIEMER: Can I interrupt? I'm not seeing anything on live meeting.

DR. NETON: Okay. Can anyone see it?

MEMBER ROESSLER: I'm not there yet. I'm still trying to get on.

(Simultaneous speaking.)

MR. KATZ: No, Jim, nothing's showing.

CHAIRMAN MELIUS: Okay. I thought it was just me.

DR. LIPSZTEIN: No, nothing is showing.

MEMBER ZIEMER: I saw something momentarily, it looked like your cover sheet. And then it's disappeared again.

DR. NETON: Okay, my desktop is off. Okay, is my desktop up there now?

MR. KATZ: Yes.

MEMBER ZIEMER: I see the desktop.

DR. NETON: I should be able to

just start this Word document, should show up, right?

MR. KATZ: Yes, it should.

DR. NETON: Okay. Is it there?

MR. KATZ: Yes, yes.

MEMBER ZIEMER: The cover sheet is there.

DR. NETON: Yes, this is the first page of the document. This is a Word document. I just wanted to scroll down through the Table 1 to refresh everyone's memory that, you know, we evaluated each individual cancer model.

And this was to determine, you know, what difference, it would have to be a geometric means for the stratified coworker model to me more claimant-favorable than just applying a 95th percentile of the distribution.

And it ranged all the way from female genitalia that had a geometric mean of 4.1 all the way down to the last cancer

model you should see on this page which was urinary organs excluding the bladder. And that geometric mean difference was 2.07.

That made at least me feel pretty comfortable that this may have some viability for looking at, you know, what needs to be stratified. But I did say in the report that it was preliminary. We used alpha exposure because in my mind, first, that's the dose that really, that's a big dose getter in our program.

I mean, most of the compensation cases, largely evolved alpha exposure to one of the either lungs or one of what we call the metabolic organs, the organs that tend to concentrate the alpha emitting material.

So that was the reason we did that, plus the alpha radiation effectiveness factor had a very widespread distribution, more widespread than any of the other radiation effectiveness factors.

Well, just to be sure we cover

all of our bases, we went and evaluated how this applied for all the other radiation types that we use in IREP. And there should be another table on the screen. Is it there?

MR. KATZ: Yes.

DR. NETON: Okay. And this is a re-analysis of that last model where for urinary, bladder excluding, this one here, urinary organs excluding the bladder. And what this is, the first line is exactly the last line of the previous report that I had on the screen where you see a geometric mean of 2.07.

And that indeed is exactly the number we got before, and it is for alpha exposures. But what's interesting about this, I'm not sure I completely understand why, for radiation type exposures that have smaller uncertainty in the radiation effectiveness factors, for example photons are greater than 250 which actually have no

radiation effectiveness factor contribution, the value is much smaller than two.

And in all cases, these values are smaller than the one that we received from the alpha, which brings this down to the point where I'm not convinced that this approach is viable anymore. I think I communicated that in the email.

It's certainly the case that for alpha emitters, it's going to be a factor of two higher if you use the 95th percentile than the geometric mean full distribution. But anyway, I just talked about this earlier because this going to affect our discussion of Section 4 of the document.

I just don't see, I can't think of any way that this is going to be a real useful litmus test for determining whether something should be stratified or not.

Okay, so now let's get on to the revisions of the document, unless there's any questions on that. Okay, I don't hear

any.

So I thought the easiest way, and I'm willing and open to suggestions, to go through the revision is actually just go through the track changes version on the screen here so that it would be pretty easy to see what changed from Rev, the first revisions we're calling Rev 1. And now we're at Revision 2.

Again, I mentioned that this incorporates, I feel to a large extent as much as possible, I think, the comments that I received at the working group meeting as well as SG&A sent a nice memo over.

In their transmittal, I believe I got the sense that we were largely in agreement on most of the issues. And we'll see how that plays out after our discussion, though.

So the introduction remains pretty much the same. That just sets the stage for, you know, why we are doing

coworker models and what sort of gives us the authority to do that in the regulation under Section 82.2. Not much changed there.

Section 2 gets into the criteria for evaluation of adequacy and completeness of the model. I've added some information here to make it clear that when we're talking about adequacy of the data, we're really talking about the technical adequacy, and is it technically capable of evaluating the workers' intakes versus the completeness which is we have, you know, what fraction of workers were monitored, if the right fraction were monitored, do we have all the data that we think we had. So that was some change in there.

I tried to beef up the adequacy a little bit. I know there was some discussions about an appendix with a lot more detail. I really couldn't see that it fit in here, so I just beefed up the language a little more to include a few more

items such as what's useful for, you know, what is a valid sample, either bioassay examples or personal dosimeter measurements.

I have a footnote in here that it allows for breathing zone air samples if they were taken, and found to be acceptable, okay. I added some information about scaling factors, if you had a radionuclides that were in a combination decision activation proxy to really clearly understand the ratio of the components in those materials.

And talked a little bit about technology shortfall, how they need to be corrected, the measure needs to be corrected to establish the model. So really, this is all about how the data technically is capable of measuring what they purport to do.

A little further down I talk about the collection, were blank samples run, and a little bit about precision. This came up in one of the reviews, I think, of

Savannah River.

If you have blank, multiple samples taken on the same individual in the same time frame, you need to have demonstration of the data fairly precise. Not just accurate but repeated measures produce in general the same value within a certain tolerance.

A little write up in here about how chelation therapy should not be used. I mean, data that were taken as a direct result of chelation therapy are probably not useful for coworker models. So just some more information in there about the adequacy of the data. Completeness, I added quite a bit of material.

CHAIRMAN MELIUS: Jim, can we just stop there and see if anybody has any comments or questions? It's easier I think if we go through section by section just like we did before. And I actually don't have any on that. But I thought the level

of detail was about appropriate.

I mean, just see it becomes a, you know, a multi-chapter book, it just becomes a paragraph. And I think the paragraph, what you have here is fine. But I don't know if others have comments. Paul or Gen?

MEMBER ZIEMER: This is Ziemer. I have no comments on this section. I think it's fine. I think the level of detail is appropriate for kind of giving the overview of what's needed in data adequacy.

MEMBER ROESSLER: I'm okay.

MR. BARTON: This is Bob Barton with SC&A. I had one thought about it, and it's kind of related to the chelation agents like EDTA. I mean, SC&A certainly agrees that it's not really appropriate to plug that value in as if it really represents what a normal excretion pattern would be.

At the same time, the reason the worker would be administered such a thing

would be because they're involved in some sort of incident and they're trying to, you know, sort of flush their system.

So I was just curious if NIOSH had given any thought as to how those incidents might be handled because again, those workers who were administered that sort of represent, you know, acute intakes that were significantly high in most cases.

And while it's correct to pull them out of any sort of coworker model because it's not about representative data point, we're sort of losing that, I guess, angle on potential exposures that maybe weren't necessarily caught, maybe not all the workers who were involved were administered the chelating agent, but then we're sort of losing those samples that sort of characterize what that incident might have been.

I was just curious if there's been any discussion or thoughts on that.

DR. NETON: Well, I guess my opinion there is that if, you know, and we could talk about this more as we go along, but if the coworker model is being developed based on what we would consider routine exposures, incidents more than likely don't belong in there.

I think we can tolerate some incidents in there, they will tend to bias results high. But when you start incorporating people who have accumulated, you have abnormally high excretion patterns which would really seriously bias the models.

So I don't think they should be in there. How we would handle the individual incident I think is not really a subject of this document.

There are techniques that one can, you know, use and I strongly suspect that people that were chelated have multiple, multiple bioassay samples that

cover a long period of time that we would use to reconstruct their dose if they were a claimant.

But again, I think that's sort of out of the scope of what we're trying to accomplish here in this document. That's my off the top of the head thoughts.

DR. LIPSZTEIN: May I?

DR. NETON: Yes.

DR. LIPSZTEIN: Hi. I tend to agree with you because actually the models won't work if the person had some therapy.

So you are trying to apply a model that will give you the intake for people that were unmonitored. So you can't use the data from people that were chelated. So I agree with it.

I have one more comment on the data adequacy, but it's just a small detail. It's that on the first paragraph, everything that's talked about, it's like if the only coworker models were developed for urine

bioassay samples instead of also for whole body counting for in vivo monitoring.

So I would add something about items to be considered like calibration of the counter and also evaluation, monitoring the progeny, and significant difference between the biokinetic behavior of progeny.

DR. NETON: That's a very good comment, Joyce. That definitely needs to be in there. I guess I, you know, we don't do that many coworker models for whole body counting or in vivo counting, but we do. And I agree. I think, you know, benefit from having discussion of that.

Plus, you are experienced with lung counting for thorium and a big topic of debate as far as coworker models go. Yes, good comment. Okay, any more comments on 2.1? If not, we'll move on. All right, hearing none.

This is data completeness. And so of course, once we've evaluated and we

passed the bar, the threshold that says the data are technically acceptable, we need to determine, you know, are they useful for bounding the population that we're trying to reconstruct?

And I don't know if I made this term up, but I've definitely called this a gap analysis that's come up in our conversations. We need to look at what data collected and on who the data were collected.

The number of monitoring samples for each category should be compared to the total number of workers, although that's not always possible because oftentimes when we get a data set, we don't have, the data set doesn't have job categories. And sometimes we don't know the total number of workers who were exposed in that job category anyway.

But if the data are there, it certainly is something that should be done.

We've added here that job category does not have to be an individual job title. It could be a category that consists of several job titles. But you'd have to establish that the exposure of those categories would be similar.

I've added a paragraph here. It says that if the number of workers in each category is unknown, it's useful to sometimes use the NOCTS data, the data that we have on claimants.

This came about in our discussion about the Nevada Test Site with their example of this where I think we had a large number of monitoring data points for folks. I think it was something like 300 people that were monitored.

But fully two thirds of those, I think, that table below, two thirds of those were radiation safety staff. So it kind of gives you some pause to think about is that really representative of the exposure

potentials considering we had laborers, welders, and miners who were certainly in exposure conditions that were equal to if not greater than the rad safety staff.

You know, it's a good example of what to think about when you parse the data out by job categories to see if the right people were monitored. So I've added this paragraph in here to cover that.

Any time there are gaps, sometimes we have gaps where we have no monitoring data, it needs to be investigated why. What are we missing? I think this happened at least one site, I can't think of the name, where there's four or five years there's just no monitoring data.

You know, is it just lost, was it taken, or was there some sort of an outage where they weren't working with radioactive materials? They need to be evaluated and explained in some way.

Just added a little bit about how

the number, I discretely identified activities will vary widely. There's not much uniformity among these sites, particularly the differences between AWEs that sometimes give one very specific task versus the large, multi-purpose DOE facilities that they had a lot of different operations. So that needed to be taken into consideration when you're looking at the completeness of the monitoring programs.

Talk about a little bit of the minimum number of data points, and we bounced around with this idea of 30. But that's certainly not a hard and fast rule. We tried to point out where there may be six workers involved and the manipulation of parts in a glove box.

And if you have three workers that were monitored, that may be enough for that operation. So it's sort of just trying to point out that there are no real hard and fast rules here. Each situation needs to be

monitored on a case by case basis.

And the last paragraph here, which I think could be expanded on but I think it's pretty important even though it's short. It talks about if you have summary databases or electronic records, some effort needs to be expended to look at, to determine if those summary databases actually have all the data.

We talked about this before where maybe these are all the routine samples, and there are a lot of incident samples stuck in a drawer somewhere in the medical files. Or even the routine samples, are they all there, or have some database manipulation accidentally removed them?

So that needs to be done to make sure you have an unbiased listing of the data collected by the site. So that's the totality of Section 2.2. I'm going to stop there and you can discuss what's in here?

CHAIRMAN MELIUS: Okay. Gen or

Paul, do you have comments on that section?

MEMBER ZIEMER: This is Paul. I'm okay with Jim's suggested revisions.

MEMBER ROESSLER: Yes, and I'm still trying to get on the Live Meeting. I've got a new computer. It didn't have Java installed, so I've gone back to my old computer. So while I'm doing this, I'm listening, and from what I've heard, I don't have any comments.

DR. NETON: Well Gen, this is, if you have the documents I sent last week or so, I'm just going through the track changes version.

MEMBER ROESSLER: Yes. And you probably, I don't see them on, I've been on travel and I haven't opened my Government computer for about a week. So I --

DR. NETON: I sent them out on the 13th, if that helps.

MEMBER ROESSLER: Okay. It says now, you are now connecting to the meeting,

so maybe I'm going to be good.

DR. NETON: Okay.

MEMBER ROESSLER: We'll see. Otherwise, I'll check my emails.

DR. NETON: Okay. And I believe they're also on the website.

MEMBER ROESSLER: I didn't see them on the website.

CHAIRMAN MELIUS: I don't think, they weren't on the website when I looked this morning.

DR. NETON: They haven't gotten there yet?

MEMBER ROESSLER: No, I didn't find that. I just found the SG&A documents.

DR. NETON: Okay.

CHAIRMAN MELIUS: I have a couple of comments. One is sort of, you sort of cover it later. But I think some comment here under data completeness, to the effect that you're usually trying to focus on sort of annual, you know, what data's complete

for a given year, because that's usually the most sort of reasonable way of approaching it in terms of how you've collected information.

You cover it later, but I think it's something someone would do if they're, you know, initially starting out looking at data completeness. So maybe, you know, just a mention of that there.

The other area that came up that I think is important is people are sort of doing the gap analysis and looking for sort of, you know, potential stratification or couldn't it be different types of exposure for people with different job titles or whatever.

But I think one of the other things that's important, again we talked about before, was sort of sufficient accuracy.

It's sort of what's the absolute level of exposure that, you know, you're

going to be more concerned about a high exposure job and as opposed to something where it's environmental exposure around the site or something like that where we know that there's not much contribution to a person's dose from that exposure.

And I would think that's, I mean, I think you generally do it because I think you want to focus on, you know, sort of the higher risk exposures. But I think mentioning it here, I mean, I think it makes some difference in terms of the number of samples that might be required and how comfortable you would be with a smaller data set or a less complete data set. Does that make sense to you, Jim?

DR. NETON: Yes, it does. I do talk about it a little bit later, but it would make sense when you're looking at completeness.

CHAIRMAN MELIUS: I'm just saying someone sort of going through this step wise

or evaluation and so forth. And that last paragraph here I thought was very good. I think it was really, really helpful for, you know, sort of going through and sort of thinking about what needs to be done there. So I would just there, I would if anything consider on expanding that a little bit.

MEMBER ROESSLER: Jim, this is Gen. I think I'm there now. I see a marked up copy. So I should be able to follow from here on.

CHAIRMAN MELIUS: Actually, I was referring to the last two paragraphs. So it's those two paragraphs at the end that I think need to be, I think are good and I thought were helpful. So just to reinforce what you said.

DR. NETON: Very good.

MEMBER ZIEMER: Jim, this is Ziemer. What last two paragraphs were you referring to, the revised one?

CHAIRMAN MELIUS: In the revised

--

MEMBER ZIEMER: The revision of 2.3?

CHAIRMAN MELIUS: No, no, 2.2 just above --

MEMBER ZIEMER: Oh, you haven't begun 2.3?

CHAIRMAN MELIUS: Yes.

MEMBER ZIEMER: Yes, yes. Okay, the new paragraph?

CHAIRMAN MELIUS: Yes.

MEMBER ZIEMER: Right, okay. I'm good on that, yes.

CHAIRMAN MELIUS: Yes.

DR. CHMELYNSKI: This is Harry Chmelynski. I have one comment about the way it's phrased in terms of the 30 samples. I would like to have it made more clear that that's for each of the groups if you're doing stratification.

DR. NETON: Yes. I was just implying, but it just wouldn't be hard to

make sure that's emphasized.

DR. CHMELYNSKI: It might be in there. It doesn't jump out at you.

DR. NETON: No, I don't think it's in there. I think I just sort of, you know, I'm close to it and I'm assuming that's what we were talking about. But it would be a minimum of 30 samples per monitoring interval or whatever, I guess, or something like that.

DR. CHMELYNSKI: Well, that's the phrasing there now at. It's just a little unclear exactly what that means.

DR. NETON: Yes. Our lawyers were asking that same question.

(Simultaneous speaking.)

CHAIRMAN MELIUS: Is that why you got a new lawyer?

DR. NETON: I think by monitored interval or evaluated interval, I was really trying to, you know, there's a generic term for, I was just kind of saying on a year by

year basis or quarter by quarter basis, you know.

DR. CHMELYNSKI: Right. I understand the temporal implication. But in terms of stratification, that doesn't clearly mean what's in that phrase.

DR. NETON: Yes, I can fix that.

DR. MAKHIJANI: Jim, this is Arjun. I think Harry also meant that these are samples for each group, each of the two groups being compared?

DR. CHMELYNSKI: Yes.

DR. NETON: Yes.

DR. MAKHIJANI: So that should I think also be clear.

DR. NETON: Well, I would say for each group that's being reconstructed, I mean, I'm not really talking about comparing at this point. And that's something that we're going to talk about later, although I can bring this up now I guess is that the way this is written here so far is if, well

actually I'm going to get to it more in 2.3.

But the idea is if you have reason to believe, if a person has reason to believe that the monitoring programs don't match up, say it's pretty clear that you have a mismatch of a routine monitoring program for this category worker and an incident based monitoring program for Category B workers.

And they're never going to be matched up. There's no real reason to compare those at all. I think they should be stratified from the get-go because, you know, you've identified, you've got the job categories, you know the monitoring programs are just similar.

There's no reason to mesh those two into one group and then start doing some statistical analyses on them. It just doesn't make any sense.

I think that, by and large, applies to most categories where if you've,

if you have a priori reason to believe that they're different and you have the ability to segregate them or separate them, I think it just should be done.

DR. MAKHIJANI: Okay, I see what you mean. Okay. That's much clearer now.

DR. NETON: Maybe that represents a bit of a change on our part, but you know, the more I delve into this, it's like well if you've got the data and you think they're different, well just go ahead and do it and let the data fall where they may. It avoids a lot of analysis, unnecessary analysis. Okay.

MR. BARTON: This is Bob Barton. There was one in this section. It has to do with this notion of evaluation periods. And normally that's, you know, one year that you calculate your OPOS value or if you have the data to do quarters then, you know, you prefer to do it as fine as possible, certainly.

But another consideration that we might want to think about is establishing these evaluation periods on more of a campaign basis. You know, for example if you had two years, we'll just arbitrarily say 1991 and 1992.

And starting in July of '91 you had shifts in a campaign for, you know, uranium processing, whatever it might be, you might want to consider breaking it up really based on operational procedures and not just a time period such as January to December of a certain year. To the extent that that's feasible I think we should look into it.

DR. NETON: Yes, I think maybe it's implied in here, at least in my mind. You know, a year is a convenient interval, and oftentimes not much changes in a year at a big facility.

But I agree with you. I mean, if there was some obvious, major change in

process or equipment or whatever and it happened in the middle of the year, yes there's no valid reason to lump those all into one.

I think that's sort of a given. But you're right, it's not explicitly stated here. Let's talk about that when you get into period longer than one year, but it doesn't call out period less than a year. Yes, that could be clarified a little bit. I wouldn't have a problem putting some language in there on that.

Okay. Anything else on 2.2? Okay. All right, this 2.3, applicability of monitoring data to the unmonitored workers really sort of gets into the meat of the issue which is what type of monitoring programs are we looking at.

And you know, nothing changed here about the three major types. You know, routine representative of the workers, routine with the highest exposure potential,

and collection of incident samples.

Let's see. I talk a little bit about how establishing the basis for the program participation, you know, what type of program was this would require. And it should involve a review of the site's radiological control program documentation.

I mean, that's where the tone is set as to how we select people for monitoring and how frequently they're going to be monitored. But nonetheless, even if you have a very good feel that the program meant to do it, I think I put in here somewhere that you need to follow up.

One needs to follow up and make sure that they actually did that. There are some cases where the site meant well, and we've seen evidence that people were not participating, either because it was voluntary and it was not really, you know, followed up on or for whatever reason. So that was added into this section.

This paragraph I'm highlighting here is something that we thought about which is a little bit, sort of a variation in my mind of a routine monitoring program where you have, you know, short duration projects for example where you'll be doing some sort of an operation and only occur for a three month period.

It may be okay just to have one sample at the end of that project, and so to allow for that. I just wanted to make sure that, you know, this wouldn't have been precluded because it's not really a routine program. It's sort of a project specific program.

Those happen from time to time, particularly at the larger DOE facilities, and very often at the national laboratories. So I added that in there.

This section here talks about incident driven samples, let's see. Yes, I mentioned this before, how I really have

come to the opinion that it's very hard to justify intermixing incident driven, workers who are only on an incident driven program versus workers who are on a routine monitoring program.

I think it's very hard to mix those two together and justify it. And that doesn't mean that the incident driven population couldn't be modeled somehow, and we talked about that at the last meeting, although there are some pretty stringent criteria that would have to be in place for that to occur.

But nonetheless, I do agree that combining incident and routine monitoring programs into one coworker general model is problematic. So that covers the additions for Section 2.3.

MR. BARTON: Jim, when we talk about that, sort of the example you gave was the three month short duration program, you know, maybe a subcontractor was called and

doing demolition work or something.

And all you have is a sample, perhaps at the end of the project. As I was reading through your write up, I sort of got the impression, would we then be looking to almost stratify where we're going to reconstruct doses for that individual who was only there for three months using those end of project values and that would be sort of a separate model aside from the general, chronic coworker model, or how would that be handled?

DR. NETON: I think at that level of detail, yes you would. But I guess in reality, I guess I can't see that happening too often. You know, you would have to look at it on a case by case basis.

But say it was a three month project and you had, I don't know, 50 workers on the project. Let's say they happened to be trace, building trades workers. I think it would be okay.

You would evaluate the 50 monitored workers. Then you would have to see based on what occurred in that three month interval what processes, you know, were involved.

Did you really even need a coworker model, you know, were the workers that weren't monitored exposed and that sort of thing. I mean, you have to look at it on a case by case basis.

I just wanted to leave the door open for that. I think there are situations like this, maybe at Savannah River, where we have a fine amount, a lot of detail on projects, project specific bioassay for some what I consider the more exotic radionuclides when you get into things like the neptunium and stuff that might have been campaign driven. It's a long answer, but I guess the answer --

(Simultaneous speaking.)

CHAIRMAN MELIUS: This is Jim

Melius. I mean, I agree with your answer, Jim. I think it's, yes, I think what we've learned in this program is that it's all, every site is different.

It's always sort of case by case and I don't think it hurts to leave open where my people are this is a possibility so that we don't sort of arbitrarily rule out doing it without, you know, thinking about it and examining the particular situation.

Yes, but it's always going to fall back on, you know, will the circumstance, are there enough, is the record keeping adequate to be able to utilize that approach. But you don't know until you look.

MEMBER ZIEMER: This is Ziemer. I agree with that. I think it makes sense to at least have that possibility in the text here. It may occur only rarely, but it may very well be needed in the future.

CHAIRMAN MELIUS: Anybody else

with comments on this section? Okay, 3.

DR. NETON: Okay. 3.0 which is the analysis of the monitoring data. This is sort of just a nuts and bolts section to allow for what was sort of to include what we do as a matter of course with these models.

Once we decided we can develop a model, we have enough data that's valid, we do generate these statistical distributions, they're fitted. I have in here allow for either log normal or we haven't done many Weibulls, but Weibull is an option. As long as it fits the data set, I think it's a valid selection.

I'm again, this is from the last time, the 95th percentile will be used as an upper bound for highly exposed individuals if the data aren't stratified. Then I've added a paragraph here to talk about OPOS, which I think we're sort of okay with. We can talk about this more.

But using a backward integrated, time weighted average analysis for the data set, and that's been included in the new Rev 2 of RPRT-53. So that's really just was added into this section.

So we want to talk about this here, or we could talk about it in the context of RPRT-53. Either way is fine with me. But I got the sense from SC&A's memo that they issued not too long ago that they didn't have any serious problems with a backward integration time weighted average approach. Seems to me to make a lot of sense.

MEMBER ZIEMER: Yes, this is Ziemer. I wanted to ask that question. It was my impression that this met the SC&A's comment. But if SC&A can weigh in on that?

DR. LIPSZTEIN: May I?

CHAIRMAN MELIUS: Yes.

DR. LIPSZTEIN: I think that the backwards time weighted OPOS is very good

improvement on the coworker models. So I think we are good on that.

MR. BARTON: Yes, this is Bob. We've had some pretty extensive discussions on this, and I think where we finally came out to on SC&A's side was that the pre-weighted, that is we're going to weight the sample by the number of data that preceded it, was really the best option on the table.

I think everyone agreed that if we had the resources and the time to do it, we would go in and do best estimate intake calculations and form our distribution based on that.

But based on the discussions we at SC&A have had, I echo Joyce's sentiment. We think it's really the best option that's currently on the table.

DR. NETON: That's good news. That's good to hear.

CHAIRMAN MELIUS: Can I just bring it back because I didn't realize until

I looked at the website that there was a revision to this ORAU document out. I somehow violated the password police at CDC and haven't been able to get online there.

But I don't know if, well, other Members of the Work Group or SC&A was aware of the revision?

MEMBER ROESSLER: Is this the one that came out October 8th or something like that?

CHAIRMAN MELIUS: Well, I see a DOE review release October 16th.

DR. NETON: Yes, that's fairly recent, and to my, I'm reasonably sure that the only things that were added were the OPOS backwards integration, time weighted average.

CHAIRMAN MELIUS: Okay.

DR. NETON: And we also added a section on how to evaluate the use of negative values. Negative values are not used in the backward integration

calculation.

CHAIRMAN MELIUS: Okay.

DR. NETON: And there's various reasons for that. I'm not sure I want to go into them today.

CHAIRMAN MELIUS: Okay.

DR. NETON: We could probably review that in the context of RPRT-53 because I didn't mention that in here, I just sort of referenced 53 because it was easier to reference a document than to explain exactly what we're doing.

And since this is a guide, I thought it would be better to just reference RPRT-53.

MEMBER ZIEMER: Is that the October 8th version?

CHAIRMAN MELIUS: It's an October 8th version and --

DR. NETON: It's been released. It's okay for public release now. SC&A asked for a copy because they saw I had

referenced Revision 2 in this draft. And I could only, at that time, release the, well I released it but it hadn't cleared DOE ADC review at that point.

MEMBER ZIEMER: Okay. But Ted sent this out to us last week, I think.

MR. KATZ: Right. But I think if Jim couldn't get into his CDC account, he couldn't have picked it up.

MEMBER ZIEMER: Oh, okay.

DR. NETON: Yes, and the non-ADC reviewed document couldn't view anywhere but CDC accounts.

MR. KATZ: Yes.

DR. NETON: Okay, but it's out there. And again, I think those are, Tom LaBone can correct me if I'm wrong, but I think those are the only two changes of any substance in Revision 2. Is that right, Tom?

MR. LABONE: Yes, those are the only changes.

DR. NETON: Yes, okay. I guess maybe we jumped the gun a little bit, but I was pretty confident that SC&A was on board with the backwards integration.

CHAIRMAN MELIUS: Now we know, so we're --

DR. NETON: So we're good to go there. If there's no more discussion on 3.0, I can go into 3.1 which will be pretty brief.

MEMBER ROESSLER: You want to make a correction, just a typo thing?

DR. NETON: Sure.

MEMBER ROESSLER: Up there, go down a little bit in your last, down to the paragraphs where you did a lot of rewriting. Third sentence, about in the middle there it says, "the use". Put "the use of" and then it will be perfect.

DR. NETON: Yes, the use of. Yes, it's amazing how many people look at this and, you know, your eyes scan over it.

Thanks. That's good. Okay, the next section, the time interval, we already sort of touched on that that we should probably use the data, the data come in various flavors. Some are quarterly, most are annual.

And I really didn't change much in here. But I do say if it's necessary to go beyond one year, changes in practices should be evaluated. That was in there before.

I kind of added the last caveat here that in general, it should not exceed a five year period unless, I'm not sure I like this word, but stringent justification. That reminds me of the surrogate data.

But you know, you certainly got to really think about a time period greater than five years, and a lot can change in five year blocks. So one needs to be aware of that, I think.

MR. BARTON: Jim, this is Bob

Barton. Could I ask where did the number five come from, because I thought I remembered from a previous revision of RPRT-53 that it was, like, it was a three year interval that was written in there for combining OPOS values. So I mean, how did we arrive at the five?

DR. NETON: I'm not sure. Which report did you see the three year in?

MR. BARTON: I thought it was version one of RPRT-53 when it was talking about strata comparison anyway. And I believe it was a footnote. I can look that up and get back to the Work Group. But I thought that it said three years, and that's the previous version of RPRT-53.

DR. NETON: Yes, I mean, I don't know. I guess I didn't remember that. This sort of just was my opinion at the time I was writing this. I'm not married to three or five, I just wanted to get the sense in there that there should be some sort of

default upper limit without really having to go to greater lengths to demonstrate that it's okay.

I mean, that's the whole point. And again, I mean, I'm not married to either five or three.

MR. BARTON: Okay, I understand. And like we said before --

DR. NETON: I take a look --

MR. BARTON: It's a case by case basis. You know, I mean, if it makes sense.

DR. NETON: I'll look at 53 and make sure we're not inconsistent with --

MR. BARTON: It might have been the previous version, I'm not sure.

DR. NETON: I don't remember. I'll take a look, though. It's a good catch if it is true. We should be consistent among our documents, that's for sure.

MEMBER ZIEMER: This is Ziemer. If you don't like the word stringent, you've got a lot of other options. Compelling

would be another one.

DR. NETON: Yes, actually for some reason, that just popped immediately into my mind when I was writing this. I've heard it so many times, I think.

MEMBER ZIEMER: The intent's the same, though.

DR. NETON: Yes, you know what I'm trying to say. Okay, if there's no comments on that, this Section 4 is where I think I need the most feedback and work on.

If you remember before, I was trying to build a case for a factor of two being sort of our cut point for stratifying or not because it would have been more claimant-favorable.

And that's probably still true because like I say, most of our exposure to alpha emitters that gets into the 50 percent range, but it's certainly not universal based on that analysis I talked about at the beginning.

So what I've done here is I've gone back to essentially what RPRT-53 recommended, which is this Monte Carlo permutation, or Peto-Prentice test.

But then I got to thinking, well because I mentioned in the earlier section if you have a valid reason for stratifying based on different monitoring protocols or different exposure conditions, then I don't know that any statistical test is really needed at that point.

If it can be done and it meets all the other criteria that we just talked about, I'm not sure any statistical test is necessary. So then I got to thinking well then do we need the statistical testing. And I'm really not sure at this point.

It seems that one should be able to test statistically under certain conditions, but I don't know. I would like to open that up for discussion.

MEMBER ROESSLER: So, Jim, is

your question at the very beginning do we do statistical testing or do we not? Or is there a list of factors that you can put out there that say here's a situation where we don't do it? I'm not sure how you're approaching that.

DR. NETON: Yes. Well, you know, RPRT-53 if you remember outlined a very, I don't want to say rigorous, but a pretty prescriptive process as to how one would go about this.

You would take the individual, let's say it's an annual basis, the data on an annual basis, stratify on that annual basis, and then compare the two strata to see if they were "statistically significantly different" or statistically different under some statistical criteria.

And if they weren't, then you wouldn't stratify. Well, you know, the argument that's been made, and it has some merit, is that the data oftentimes have such

large deviations that you would have to have fairly massive, massive is probably not the right, very significant, not a good word either, very large differences between the two before you would ever detect some statistical difference, which begs the question well then is that really the way to go.

In my way of thinking, the way we've described this now is if you stratify, if you look for stratification up front based on valid reasons of differences in job categories or exposure conditions, then I don't know. Do you have to do a statistical test to show they're different?

It's sort of an opposite approach. Do you qualitatively segregate or separate these data sets and analyze them and let the chips fall where they may if you can analyze that, if you have enough data, or do you segregate them and then look for statistical differences?

I think I prefer the previous approach which is if you can do it, do it. I don't know. I'm really kind of, I'm torn here as to how to proceed.

MEMBER ZIEMER: Jim, you would have the coworker model for each different strata then, is that what you're saying. If you can do a valid stratification to start with?

DR. NETON: Yes. If you can do it, valid stratification based on job titles or, I think to a large extent it may end up being, you know, maybe trade workers, construction trades that were more incident based.

That doesn't require statistical testing in my mind. Those are just two separate monitoring programs, period, two separate exposure conditions. So you would have two separate models. I don't know.

CHAIRMAN MELIUS: This is Jim Melius. Again, in the construction versus

production or, you know, incidents versus routine sampling, I mean, I think that's, you're precluded from doing a meaningful stratification there or appropriate one.

I think the question would be that in other situations where people are part of the same type of sampling program, you know, that I think one of the arguments you would need to have sort of a more robust data set to be able to base your coworker model on if you, I mean, depending on whether you'd stratify or not.

I mean, it would affect sort of the power of your coworker model to predict, but it really is going to be a case by case basis. Always our experience has been recently at least, and I think at least the ones we spend time on at the Board is that we don't have adequate data to place people within these, you know, job titles or whatever.

DR. NETON: That's right. I

think you're hitting exactly what I was thinking is let's say we have, you know, a situation where you have a fairly robust routine monitoring program. And at a minimum, you have data on the job titles of the people who are in NOCTS at least, you have 1,000 of those.

And you can sort of establish that you have the people that were monitored seem to be in job categories that had the highest potential for exposure, you know, in a routine process.

So now you have these unmonitored workers that clearly would fall in maybe a different exposure category, but maybe you don't need to stratify at that point because you've demonstrated a front that the highest exposed workers were the ones that were monitored, and using the 50th percentile would be totally fine.

I don't know. And then you don't, if you start stratifying down into

these lower tiered unmonitored workers, then you end up just basically giving them less dose. I mean, not that that shouldn't, maybe it should be done.

And maybe that's the point where you can determine, use the statistics to decide why it shouldn't be stratified, something like that.

MEMBER ROESSLER: I like this approach. And I was going to ask you earlier about it. What I think is going to be difficult is you're talking about a verbal or descriptive way of making a decision rather than something that's, you know, more statistical.

DR. NETON: Yes.

MEMBER ROESSLER: It might be harder to justify. You talk about job titles. At the start, that sounds like a good way, but I'm just wondering how that would work out.

DR. NETON: Yes. I don't know.

Like I say, it's easier for me to think about the routine operations versus, as Dr. Melius points out, the sort of subsets of populations like the trade workers or, you know, maybe some, I don't know, some workers that involve project specific exposures, campaigns that went on for several years that were different.

I'm not sure. This is one of those situations, and it's almost, like, you know, until you see it. Maybe I need to go back and figure out some examples. I think that might be --

CHAIRMAN MELIUS: Yes. I was going to suggest, I think that would be helpful. Maybe some of the external exposure coworker models might, where you have a larger data set or something. Or some of the ones where we've done in the past and have approved that we would need to look at.

DR. NETON: Yes. I think this is

best handled, I guess, through an example or several examples of how one might --

CHAIRMAN MELIUS: Yes.

DR. NETON: -- proceed. Again, I think routine is one set, and then pulling out the, sort of, special exposure populations to identify them and sort of determining how you're going to handle them separately.

I think there's room for statistics, of course, in here but I'm trying to figure out the best, and I probably need to talk to our folks, too. I haven't discussed this with them either.

CHAIRMAN MELIUS: Yes. Yes. I think going back to them, where some examples would be the way to sort of flush this one out.

DR. NETON: Other than that, I think it seems like we're fairly okay with the bulk of this document.

CHAIRMAN MELIUS: Yes.

DR. NETON: This last piece. And that makes me feel pretty good. Even though it's only eight pages, it's been a lot of work.

CHAIRMAN MELIUS: Yes, yes. No, no.

DR. NETON: That's all I had to say on this.

CHAIRMAN MELIUS: Why don't we just jump to the last item on the agenda. We'll come back to the other items. But my thought would be if you want to make some quick revisions to this or not make quick revisions to it depending on how busy you are, that we get this out to the full Board, anyway, and do it as a presentation for our Board meeting next week.

DR. NETON: Yes. I think that I probably won't be able to make too many -- I can, you know, most of them are fairly straightforward.

CHAIRMAN MELIUS: Yes.

DR. NETON: I tried to keep some decent notes here. I think I can put in what we talked about that makes some sense. I won't have the examples, obviously, ready for Section 4. I'll just flesh that out and say examples to follow or something. Yes, and then I can reissue it.

CHAIRMAN MELIUS: Yes, if that gets too, you know, sort of time pressed and I'm not sure it's worthwhile. It's just I hate to have you have to respond to the same comments. We'll forget what you agreed to also. So we'll be, like, asking you the same questions.

DR. NETON: The transcripts are going to come out eventually, but --

CHAIRMAN MELIUS: Yes, right, I know. But what would make, you know, whatever works for you would be fine. And I think if we had that and get sort of full input from the Board on it, that then we could, you know, sort of decide what to do

going forward.

I mean, I think the other question is, are there issues that either need to be fleshed out more or that we haven't thought of, because I think this, you know, this approach has, or our thoughts about this document have evolved.

DR. NETON: Oh, definitely.

CHAIRMAN MELIUS: Yes.

DR. NETON: I think I can make as many changes as I can get, you know, reasonably within sort of half a day. And I can probably get this out to the full Board by Thursday, given that I don't have that much time. I'm not going to lock in a change, but I'll try to just do the simple ones.

And then I can present this to the Board, yes, it's not a problem. I was thinking about doing one other thing when I do my presentation, though. And I've got it, I've just displayed it here.

I have a sense that there's a lot of people, some Board Members are not really familiar with what we really do with coworker modeling data and how it works.

And so I thought a brief presentation on an example from Savannah River might be appropriate. And what I put together here is a brief slide show that talks about a specific example from Report 81, or TIB-81 that goes through how coworker models are constructed by year, go over how the data come out.

And then specifically talk about these graphs of how chronic models are fit through separate pieces. You know, and then end up showing how it overestimates at the very end and talk about what the GSBs and all that stuff sort of mean.

I don't know. Do you think that might be helpful as part of the process?

CHAIRMAN MELIUS: Yes, I do. Gen and Paul, do you --

MEMBER ROESSLER: I think it's absolutely --

DR. NETON: I recall from the last meeting in Idaho that people were asking. I didn't have this type of information available at the time. And I think it would be very helpful to see here's a study of a coworker model.

MEMBER ZIEMER: I agree. I think that it's a good idea.

DR. NETON: I can do this piece as well as basically just go through the draft, which will be Rev 3 at that point. Okay. I can do that.

MEMBER ROESSLER: Good.

CHAIRMAN MELIUS: Okay. Thanks, Jim. I think the other, you want to do the other two items on our agenda? The first one is the ten year review. Well, ten year review on, we had talked about I think at the last Board conference call we were reviewing the ten year items. And it came

up that we, about DCAS was having difficulty, probably not surprisingly, in terms of addressing the comment about the need or the potential helpfulness of having input from other academic areas or even non-academic areas in terms of applying the policies in terms of sufficient accuracy and some of the other, and SEC evaluation types of issues.

And I think you had, Stu, you had mentioned that it was causing -- we tried approaching it, were having difficulty sort of coming up with an approach that would be, you thought would be useful or at least in terms of how to frame the issues.

And I think I responded by saying well maybe we could talk about it as part of this Work Group meeting, at least I'm not sure we have a better idea, but at least we can try to address it if we can.

MR. HINNEFELD: Okay. Thanks, Dr. Melius. Yes, this is a, of course it's

problematic. We staffed -- the vision has been staffed. Largely, the technical people in the division are health physicists and we haven't really sought out a policy team so to speak and addressed these as an issue.

NIOSH always sort of interpreted its assignment on this program as a scientific assignment. And so that's kind of how we've approached it.

I do think that over the years, the continuing discussions with the Advisory Board and the Board's contractor, while not necessarily introducing other disciplines, has certainly introduced other points of view.

And I believe over the years we have had NIOSH also, and the Board and our contractor, have sort of converged on how things will be done. So I really question, and maybe some others of you who are not so close to it as I, maybe some others have some ideas about how this might be

accomplished and its value.

But I kind of question the value of at this point in the program, pursuing other opinions that, presumably would be somewhat less informed than those of us who have been working on the program.

We do, as the years have gone by, we have been, I believe, more accepting of input from the claimant and advocate community, and try to continue to take information they provide us seriously.

I'm not so sure that ten years ago we envisioned that that would be a large avenue, but I think they've provided a lot of useful information. And we have sort of incorporated that into our work process.

So I'm really no closer than I was at the Board conference call to having a good idea about how to go about something like this, but I kind of have the same opinions that I had then that I don't think the utility, I don't see the utility of

actually going out and pursuing other disciplines.

And I believe that the intent, which is to get a broadening of the thought process to the questions brought to our program, particularly the SEC questions, I think the intent of getting that broader perspective is largely satisfied by the relationship we've developed with the Board and the Board's contractor.

So I guess I'll stop there and see if anyone has anything else they want to say. Am I still on the phone?

MEMBER ROESSLER: Yes. Stu, what's the downside at this point if you don't get input from, as you call it, other disciplines?

MR. HINNEFELD: What is the downside?

MEMBER ROESSLER: Yes. I mean, if you just say well let's not do it, what would the implication be of not doing it?

What sort of criticisms could arise?

MR. HINNEFELD: Well, the criticism would be -- NIOSH said that, you know, you have this ten year review which was, you build it as this important review of your program, this fresh look. You have these recommendations, even some you might have considered priority recommendations and you have nothing to show for it. What happened to that?

Was this a real activity or not? You know, were you really serious about taking a serious look at yourself? And you know, that criticism could arise from wherever.

MEMBER ROESSLER: And I think that's a serious criticism.

MR. HINNEFELD: Yes, I agree. So I'm still open to suggestions then about what are the kinds of perspectives we seek and how do we pursue those.

And how do we, sort of, assure

ourselves that we will get, sort of -- you know, perspectives that kind of match the thought process that we've kind of been converging on, you know, NIOSH and the Board and their contractor on this process?

So you introduced a possibility of getting things that none of us would perceive as being helpful and may cause work to address in some fashion when those of us, you know, us I mean us and the Board and the Contractor would say gee, I don't see how that could possibly be helpful to pursue that.

On the other hand, I'm speaking here, I am so close to this that certainly my judgment on that matter could certainly be at question.

MEMBER ZIEMER: This is Ziemer. Let me raise a related question. Are there specific viewpoints or aspects that people feel NIOSH or the Board has been overtly rejecting?

In other words, the outside, sort of new outside viewpoints that seem to be needed, are these viewpoints that someone has identified as being not considered or rejected or otherwise ignored?

I sort of feel the way Stu does, but I have my own bias. But I think the Board and the contractor and NIOSH itself have been pretty open to a lot of outside viewpoints. But I guess we hear from some that really are -- are there voices that aren't being heard or are being ignored?

CHAIRMAN MELIUS: This is Jim Melius. I don't think that it was meant as a criticism of, sort of, the process as much. I mean, there were other issues about, you know, input from the claimant or the claimant community so to speak that were sort of outreach issues and other issues that were included in the ten year review and are, I think, in the process of being addressed.

I think the other thing is that we all are very close to the, you know, process and so forth. And as we continually find out, it's very hard to develop very general rules for this process because, you know, everything is case by case.

So most of our decisions are made by spending a lot of time reviewing a particular, you know, site or exposure at a site to what's available in terms of data and so forth.

And that often, you know, sort of precludes the development of general rules. Thorium is different at a different site so we can't just have a -- you know, we encounter thorium and therefore it's an SEC kind of rule or it's not an SEC or whatever.

So I think that's sort of where we are, our perspective on it, my recollection of the process was that for the ten year review was that that recommendation was more in terms of how the compensation

decision process was originally set up.

And would it be useful to have, you know, input from other disciplines that might be more familiar with other compensation processes or the ethics of this type of an effort. How do we, you know, evaluate fairness, how do we evaluate something like what is claimant friendly and things that have been incorporated into this program.

And would that possibly be helpful? I think Stu's right, it's a little hard to think specifically of what that would be. And particularly when we're so far down the line in terms of the number of years that we've, you know, this process has been set up and so forth.

So I think, you know, we all may have different views on what could have been done better or might have been different approaches that should have been considered, you know, however many years ago that

weren't.

But I think our obligation, Paul, back to yours, is not are we going to be criticized, not criticized, or is it really going to make a difference or not, but is there some way of exploring that trying to better understand how that might be helpful and is it feasible to incorporate that into the effort.

Now I believe that recommendation came from both John Howard and, well from a number of people but from John, from Randy Rabinowitz as part of her review of the SEC process.

And I mean, one way of pursuing it would be, you know, at our Work Group meeting because I think most of it was directed at the SEC Issues and invite, Randy could participate by phone or whatever to explain what she meant by that.

MEMBER ROESSLER: I'm not sure what she meant, but when you think about

this, the real difficulty of involving some more evaluation at this point is that it takes us, you think about an unbiased group, it seems that we have a real, they would need a huge knowledge base.

I don't know of any group, other than some that might be considered biased, that could get up to speed enough on what's being done and then actually on a timely basis be productive.

CHAIRMAN MELIUS: Yes, I don't have any specific examples to counter that. But I think at the same time, it was a, you know, recommendation that NIOSH and the commitment that NIOSH made to address, or at least explore.

And you know, I think it's something that certainly the Board could be involved in helping to explore it. And I think, you know, I think we take it like we do everything, a step at a time and see if it helps or if it doesn't help, if it's

feasible or not feasible.

MR. HINNEFELD: This is Stu. I think maybe inviting Randy to participate in a Work Group meeting might be an avenue to pursue. And we might be able to get, at least refresh my memory on where her thought was on this.

And then the other question is something like that consistent with the regulations that were published because theoretically the regulations could have been published in a different manner.

But that goes back, I think, farther than anybody wants to try to rewrite the program. And it's a matter of well, you know, can we, within the context of how the regulations were written, can we do something along those lines of other, you know, not writing this strictly as a scientific program.

Is there something we can do that perhaps this was, there were avenues that

maybe there should have been some policy or other kinds of thought process applied starting up as opposed to saying this is strictly a scientific program.

CHAIRMAN MELIUS: But there also may be some policy or procedure modifications that would be, you know, are not as dramatic as requiring regulation changes. I don't think we can --

MR. HINNEFELD: It would be preferable not to embark on regulations in any kind of timely fashion because who knows what happens then?

MEMBER ZIEMER: Well, Stu, the policy issues are still built into the program. I mean, the Board is just one voice of input to the Secretary's office.

So there's a whole other level of input that comes into play before any kind of decisions are made.

CHAIRMAN MELIUS: So if it's appropriate for everybody, I mean, let's see

where we are at the Board meeting next week in terms of follow up on the database issue.

I'm just thinking back to the coworker issue. When we were, you know, that was also a recommendation from the ten year review. And I think many of us, I'll speak for myself, I personally thought it was necessary to, we really deeded to address it.

But I think I and others had trepidations about doing so at this point in time and how we would go about that and how potentially disruptive it could be as a program.

And I think what we found out is that, you know, we are in the process of addressing it and I don't believe it will be as disruptive as we might have imagined, at least as I might imagine at the time when we started the process.

And maybe this will be the same. And you know, maybe it's something that's

just not feasible at this point. But least we can say we've evaluated and explored it, and provided some input to NIOSH on it.

MEMBER ZIEMER: Good point.

MS. LIN: Hi, this is Jenny.

CHAIRMAN MELIUS: Yes.

MS. LIN: If I can have a couple minutes. So you know, I've been away from the program for over a year. And I haven't touched anything on this ten years review program review since I returned.

But I'm a little curious, or I just need to do more work to really place that recommendation that we've been talking about today within the context because I think, you know, you tried to introduce that recommendation broadly over the entire program.

I'm not entirely sure that is, you know, that is really what the recommendation's about. And then on the other hand, I'm looking at the EEOICPA

statute where it specifically speaks to the type of advice from the Advisory Board.

And in the statutory provision, it talks about how the advice of the Advisory Board needs to be based on exposure assessment by radiation health professional.

So I think I just wanted to sort of center it back to the statutory obligation. And then, you know, examining that recommendation when the ten years program review within that context. Thank you.

CHAIRMAN MELIUS: So we'll plan out a Work Group meeting. We can talk about it more after RLA meeting next week.

The other item on our agenda is, I don't know where exactly where it stands. Again, I got caught by the password police or something and have not been on the CDC website in a week and a half or so.

But there's a Savannah River SRS coworker model that SC&A was reviewing that

the Savannah River Work Group sort of referred over to us. And I'm actually, at this point I was understood that it was close to -- the SC&A review was close to being finalized. I don't know if it's been transmitted.

MR. STIVER: Dr. Melius, this is John Stiver. It's almost ready to go to DOE for review as of today.

CHAIRMAN MELIUS: Okay.

MR. STIVER: So I anticipate, you know, depending on how long they take, maybe a couple of weeks. But certainly it's not longer than that.

CHAIRMAN MELIUS: Okay.

DR. NETON: John, this is Jim Neton. Refresh my memory, is that TIB-81?

MR. STIVER: No. We're referring to Report 55, the trivalent actinides coworker model. That was the last of the nuclide specific models that we were looking at.

DR. NETON: Right, got you. Sorry.

CHAIRMAN MELIUS: And it sort of got, well one, we got referred to the SEC Work Group because we were dealing with coworker issues. But the fact that we were dealing with coworker issues in a more general way than that site specific, took a little bit of time.

And I think SC&A was sort of waiting for us to make some progress before they sort of knew how to go about reviewing it to make that review more appropriate for the issues we were concerned about here.

So I just wanted to keep it on the NRQ here. And that would, again, be part of a Work Group meeting we might hold after the next Board meeting. But Ted, anything else?

MR. KATZ: No, no. I think we covered everything nicely.

CHAIRMAN MELIUS: Good. Boy, I

beat my fire drill. Either that or the building's burned down around me. I can't tell or I must have not heard the sirens, bells. But anyway, if no other business, thank everybody.

Thank you, Jim, a lot of work that you've done on this effort, and SC&A and everybody also. And I guess we'll see everybody in Los Angeles next week, or hear your voices.

(Whereupon, the above-entitled matter was concluded at 3:01 p.m.)