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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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WORK GROUP ON SCIENTIFIC ISSUES

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FRIDAY
JANUARY 19, 2018

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The Work Group convened via teleconference, at 3:00 p.m. Eastern Time, David B. Richardson, Chair, presiding.

PRESENT:

DAVID B. RICHARDSON, Chair
R. WILLIAM FIELD, Member
JAMES E. LOCKEY, Member
WANDA I. MUNN, Member
GENEVIEVE S. ROESSLER, Member
PAUL L. ZIEMER, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
STU HINNEFELD, DCAS
LARA HUGHES, DCAS
JOHN MAURO, SC&A
JIM NETON, DCAS
DANIEL STANCESCU, DCAS
TIM TAULBEE, DCAS

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(3:00 p.m.)

Welcome and Roll Call

MR. KATZ: This is the Advisory Board on Radiation and Worker Health, the Scientific Issues Work Group. And today we are dealing with some scientific work that's recently been completed and published.

And we have the full report as well as -- I'm not sure, but I think just full report is posted on the NIOSH website, along with today's agenda, which doesn't tell you more than we're dealing with that report, really.

And the Members should also have a draft, a general summary of that. And so that takes care of it. And these things, on the NIOSH website, if you look at the NIOSH website, this is located in the Board's portion of the website under schedule of meetings, today's date. And so you can download that document.

All right, let's do roll call. We

have no issues with conflict of interest, you don't have to address that.

(Roll call.)

MR. KATZ: Okay, then, without further ado, everyone now knows the protocol about phones and meetings and so on. So, David, it's your meeting.

Work Group Discussion:

Organize a review of "Dose and Dose-Rate Effectiveness Factors for Low-LET Radiation for Application to NIOSH-IREP" by Trabalka et al. (Oak Ridge Center for Risk Analysis Inc.)

CHAIR RICHARDSON: Thank you. So this is the topic: dose and dose rate effectiveness factors. First let me check, can people hear me okay?

MEMBER MUNN: Sounds good.

CHAIR RICHARDSON: So this is a topic that's been on the table since 2012, I think, or 2011. To remind you, we had meetings and a

briefing by the contractor back in 201 where we had an initial draft of the report.

What was discussed was what's called DDREF, which is, it's an adjustment factor. And unlike many of the other factors that are in use by the IREP program for calculating Probability of Causation, this is one that you divide by, as opposed to multiply by.

So there's an estimate of the risk based on analyses of the Life Span Study. And the DDREF is a divisor that you run through that based on hypothesized reduction of risk per unit dose for lower doses, or exposures that are received at lower dose rates. It's not applied for leukemia, but it is applied for other types of cancer.

And the topic for discussion in this report is dose and dose rate effectiveness factors for low-LET radiation. So we're not talking about dose rate effects that might be considered for alpha radiation exposure or

neutrons.

Currently, what's done, there are a range of values that have been used. I believe currently there's site-specific dose and dose rate effectiveness factors. And we'd been given a report, which has now been finalized by NIOSH and has had external review as well, discussing proposed new distribution values.

So, not a single value for the DDREF, but a probability distribution that would be used for all cancers other than leukemia. And as I understand it -- well, a couple issues.

One is Wanda noted prior to the start of the meeting that she had some questions that she would like to get cleared up before we move too far into the conversation. And I think that would be appropriate. And then we could briefly talk about what our task is.

So, Wanda, do you have some questions at the start?

MEMBER MUNN: Oh, yeah, I think they

are fairly straight forward, David. And I'm really glad that you're the person who is handling this. Clearly, your background and capability is reflected in papers that are referenced by this study itself. And thank you for being here.

My questions are fairly simplistic, I do believe. It's a matter of my not being familiar with analytical methods more than anything else. It's a couple of questions that I don't know about recent activities in the field.

One of them is, I am not clear whether -- I understand that, when we talk about DS02 dosimetry, I think I understand what that collection of information does. But I understand that in recent years there's been an attempt to tighten that information up a little bit with older maps and things of that sort to try to place where people were.

But I guess my direct question to you

is, do we have more than just a reasonable method of estimating what the actual doses for these people, especially on the fringes? That clearly is the best set of data that we have with respect to exposures. But the fact that it has the additional problematic exposure from neutrons and other activating materials that you wouldn't get in ordinary exposures kind of complicates it from my point of view.

So, I guess my real bottom line question is whether you feel that we have better information now with respect to the source term for the LLS. Or LSS, pardon me.

CHAIR RICHARDSON: Yeah, that's a great question. And it impacts partly on this evaluation of dose and dose rate effectiveness factors because the approach that's taken here, and is often taken to estimate a change in the effectiveness per unit dose, is to get an estimate from the Life Span Study. And then compare it to an estimate from another population

that's either exposed environmentally, medically, or occupationally to lower doses or lower dose rates.

So we take a ratio of two values. And the validity of that ratio, that quantity, depends in part on each of those being well estimated, unbiased estimates.

And then from there we can also try and get some idea of the uncertainty. But we propagate that uncertainty because we have uncertainty in the numerator and the denominator of those two estimates.

MEMBER MUNN: Yeah, exactly.

CHAIR RICHARDSON: So that's why this is not a simple project. And then, further, we've got questions about the comparability of observations from 1950s Japan to populations in the Techa River cohort or nuclear worker cohorts.

Yeah, it's a difficult question. But specifically the dosimetry question, the recent updates have done a bit to refine information on

location by going back to maps. But a lot of what's gone in is trying to do improvements on modeling shielding through computationally intensive modeling.

And there have been attempts to validate dose estimates using other estimates of -- not of radiation deposition in human tissue, but, for example, in roof tiles.

MEMBER MUNN: Right.

CHAIR RICHARDSON: In the end, though, the LLS's have some component which relies on self-supported information about location that was collected in the 1950s. So, anywhere from five to 12 years after the atomic bombings. So that's going to be one source of uncertainty. It's the accuracy and validity of the information that survivors provided.

And from that there's been fairly complicated, and I think strong, efforts at reconstruction of the radiation field. But it requires placing people in the right positions.

But people on the fringes, if by that, you mean the people at good distances from the bombs?

MEMBER MUNN: Yes, correct.

CHAIR RICHARDSON: Actually what you get to, for a large group of those people, is sort of a default assumption that they're in the lowest dose groups.

MEMBER MUNN: Yeah.

CHAIR RICHARDSON: So, there was never as much information, detailed information about location and shielding collected. And it's sort of they're providing the reference rates for the rates in the lowest dose groups.

MEMBER MUNN: As I said, at base, it's still the accuracy of the base of information. And I'm very interested in -- I have not seen your paper or any of the INWORKS statistics that came out of your work.

But I wanted to comment, I thought it was quite appropriate that, even though I'd love

to know what the Chelyabinsk findings were, but I agree that, as the authors here have said, there are too many potential variables to place much reliance on that information from that particular source.

It is still of interest, I think, to this paper to know what the other findings of your INWORKS study were that were used in this paper. Without going back to the references it's impossible to try to identify, in my mind, what the more trusted sources of information are that are being factored in to what [identifying information redacted] et al., did.

CHAIR RICHARDSON: I think that, for this program, I agree that there's a kind of natural -- there's some relevance, for sure, to the studies of the workers of the nuclear industry. And INWORKS has got some substantial fraction of the U.S. workers from DOE sites. And then workers that are at not entirely comparable facilities, but fairly similar to facilities in

the U.K. and France.

And that becomes the basis using the dosimetry records. And some attempts to evaluate the dosimetry programs in each of the facilities, reconstruct the dosimeters, put them on phantoms so that we could do some corrections for differences in response to different energies and geometries and exposures between the facilities.

And some of that early work on the dosimetry has been picked up and used by this compensation program as well. And that provides one set of risk estimates for this evaluation.

It is a question about whether the choices of the other studies, what they chose to include or exclude in making this probability distribution, I think those are influential decisions. I think they provided a rationale, but, you know, that's something that we could comment on, for sure.

MEMBER MUNN: Most of the time I think they did present some kind of rationale, but it

wasn't always clear to me, for example, when they refer to several of the potential data bodies that they had as being not statistically significant.

I was surprised that one study including the Canadians, they had made the statement that they didn't consider that particular body of knowledge to be statistically significant. And I do not know what that means in this context. Does that have any relationship with what the words "statistically significant" mean in the popular context? Or is that something else, that the judgement was made that the results were not going to be bearing on this? Or was it that the cohort was so small that it could not be used to influence the larger body of information?

Can you clarify for me what, in this paper, the words "not statistically significant" meant in choosing the representative information they were going to use?

CHAIR RICHARDSON: I'm not -- maybe if you point me to a page, I could go back and look at that. I think most of the exclusions were not based on the significance of the point estimates, but rather concerned about data quality with some of the other recent studies which they looked at.

DR. NETON: David, this is Jim. I think he was talking about some of the earlier INWORKS results that came out, where the experts felt excess relative risk received was just not statistically significant. It was just a simple parroting of what the results of the study were.

CHAIR RICHARDSON: From the 15 countries study?

DR. NETON: Yes.

CHAIR RICHARDSON: Okay.

DR. NETON: Some of the earlier results I remember were just not finding much. I think that's what they were talking about when they made that reference.

MEMBER MUNN: Yeah, and that's my

concern, whether it was because of the size of the population or it was another one of those things that happens in modeling when the researcher just simply determines that's not pertinent to what --

DR. NETON: Well, I think the researchers in those studies made those conclusions. They're just parroting what was stated in the findings.

CHAIR RICHARDSON: And I think, well, what they ended up doing in terms of implementation here is that study, the vast majority of the information from that study, was encompassed in the newer INWORKS study, which is an update of the oldest, mostly from the cohorts from the earlier analysis. So it's updated with the follow-up for the U.S., the U.K., the French cohorts. And they're going to rely on these updated data.

There are slight differences between that and the earlier 15 countries study. But the

cohorts that were not included were all cohorts which didn't provide much information previously. Lithuania. There were lots of other countries which had contributed that were older parts of the nuclear industry which had long term follow-up where you could study mortality.

MEMBER MUNN: I think when I first encountered the question, you asked for a page reference. On Page 14, the paragraph starts at "Exclusion from our analysis are all results from the 15 countries study of radiation workers by Cardis in 2007 was based mainly on two considerations. We judged the entire Canadian cohort should be excluded from the 15 countries study because of concerns about the reliability of estimated doses to the Atomic Energy of Canada limited workers who were monitored during the early period" -- they mean '64, I guess -- "and the significant impact of unusually high risk dose workers results from the 15 countries study. We excluded results from a 14 country study from

our analysis because the estimated risk was not statistically significant."

And, you know, I can understand eliminating some of that early information, but it surprised me that Canadian information later than 1964 somehow got excluded, if I'm reading this correctly.

CHAIR RICHARDSON: Yeah, it's because the Canadians themselves have expressed their concerns about the data. And so until they feel like they've understood what the issues of data quality are, for the purposes of combined analysis, the Canadians did not want to continue to include their data.

So, there has to be a representative from the country who would speak for the validity and ownership of the data. And currently in Canada there isn't one.

So, they had initially reported their results. The magnitude of the associations in the Canadian cohort were larger than in the other

cohorts. They felt like it was due to an issue of -- some problem with the recording of information in the dosimetry system.

So, they've gone back and done auditing on that. They've identified some issues, but they don't have a clear -- at least I've not seen a clear sort of explanation of what all those issues are yet. So they're currently withholding their information from these combined analyses.

MEMBER MUNN: I'm sorry to be asking these kind of questions, but my naturally questioning nature has a tendency to make me think why are these fairly large bodies of information not being used. It is partially because the result is not one that these authors particularly adhered to?

But this is the case also with respect to the colon data that was referenced in several -- obviously, some great emphasis was placed upon those particular studies. And it's not clear, at

least it wasn't clear to me from the text, what criteria made the choice of colon so obvious as the organ, target organ, for this report here that we're looking at. Do you have information on that?

CHAIR RICHARDSON: That's been a -- for estimates of doses to different target organs.

MEMBER MUNN: Yes.

CHAIR RICHARDSON: Is what you're referring to. And their use here for the analysis to solid cancers is to use the colon. They've taken the estimate of uncertain deep dose. So, estimates for many of the specific, to most of the tissues, there would be a good deal of correlation. You know, there's slight adjustments for estimates of the photons getting to different organs.

But the convention in the Life Span Study, when they're doing all solid cancers, or even all cancer analysis, is to take the colon as

a representative of what a deep dose would be.

And that's, more recently, with the nuclear workers studies, is where there's been an attempt to adjust the film badge reading to get an estimate of organ-specific doses as well.

They've followed that, in part, for comparability. I mean, partly for the logic of the argument that was made. But also to compare an estimate that was reported in one of the official all-year publications about, say, estimated dose.

You had to take some metric of the dose. So the coefficient reported was estimated dose to the colon in solid cancers. But we could get a comparable estimate in the worker population by making a slight correction to the film badge reading to get an estimate of what the deep dose would be just to the colon. And compare those.

So, it's partly that the literature now has some coefficients based on that metric

that allow you to compare, so that you could take the ratio from the numbers on the same scale.

MEMBER MUNN: So, it's partially a determination out of the RERF and similar references.

CHAIR RICHARDSON: Right. So, they weren't wearing badges. So, you know, their dosimetry system now reconstructs doses to a relatively short list of target organs. And that's the one they're taking.

But if you look within those, if you do like a correlation coefficient estimate of a dose to the liver versus a colon versus various organs you'll find it's not so different. Or the lung, being an important one.

MEMBER MUNN: Okay. I guess my last question is one of terminology. Again, as the statistically significant terminology was, I am not clear in my mind exactly what was referred to -- some of the tables refer to piecewise distribution versus discrete distribution. And

that's not clear to me. What is the difference in a piecewise distribution versus a discrete distribution? I'm just not familiar with the terms.

CHAIR RICHARDSON: Hello?

MEMBER MUNN: I'm here.

CHAIR RICHARDSON: Oh, I'm sorry. I was on --

MEMBER MUNN: Oh, were you talking away to some other air?

CHAIR RICHARDSON: Yes, I was.

MEMBER MUNN: I'm sorry about that.

CHAIR RICHARDSON: So, I think the key point in linear distribution -- in the footnotes example on Table 1. Where it's got a value, if you imagine a triangle with the peak of the triangle at 2. And the two bases of the triangle being at point 2 and 5. You could describe that distribution by two lines. One going from point 2 to 2. And then a second line segment going from 2 to 5.

MEMBER MUNN: Yes.

CHAIR RICHARDSON: So one's sloping up, and one's sloping down. I think that's what they're referring to. The footnotes see, so Footnote B in Table 1 is a uniform distribution. It's just flat with equal probability in the range 1 to 2. Footnote C is referring to a triangle that could be described by two lines with a joined point at 2.

And then the other one is like a histogram. I think discrete values at 1.5 and at 2. Footnote E and F, I don't know if they have pictures of these distribution -- I don't think they do here. But if you imagine a histogram with its series of bars, when you say it's 20 percent probability, it's 1.5. It's 25 percent probability, it's 2, and so on.

MEMBER MUNN: Okay. I can't see the figures but I can see the --

CHAIR RICHARDSON: You can imagine that.

MEMBER MUNN: Yes, I can imagine that.

CHAIR RICHARDSON: There's basically like integrating up to 100 percent.

MEMBER MUNN: Right.

CHAIR RICHARDSON: Under a box, like a uniform distribution, under a triangle, or a series of histograms. But it always has to add up to the total.

MEMBER MUNN: Okay. I got it. And one last question and then I'll shut up. My last question is I'm never certain when I see the word kerma being used. Whether that description of kerma is the same for the user as it is for me and, because I always wonder why it's used. To me that's just a Gray. And is that incorrect?

(Simultaneous speaking.)

MEMBER MUNN: Should determine free in air.

CHAIR RICHARDSON: Yes, I mean it's the energy in air, but it's not all -- is that a gray? I --

MEMBER MUNN: I thought it was the same as the SI unit. But, it's just an energy measurement, right?

CHAIR RICHARDSON: Yes. But now it doesn't have a volume does it, that it's deposited in? It's sort of an idea of the potential in the air. So -- I'd, yes, I'll defer to you, Wanda.

MR. HINNEFELD: Jim, this is Stu. Can I get a shot at this now?

DR. NETON: Yes, well kerma is an approximation. It's initial kinetic energy with charged particles that are released. And, you know, depending on whether or not those particles deposit all their energy in the volume under consideration or not, kerma is a pretty good approximation for the dose to an area. But I don't know exactly what usage there is in this document.

MEMBER MUNN: I think he's referred to kerma a couple of times in some of the footnotes.

CHAIR RICHARDSON: Yes.

MEMBER MUNN: When I was trying to read you the footnotes to understand what the table said. But, okay, I just -- free in air, means there's no real target there. The energy's out there, free in air. Okay. I'll just accept that because I'm not at all sure it clarifies the table anymore for me or not. But it was a question that I puzzled over at the time I encountered it.

Now, I'm going to shut up and let people who know what they're doing with statistics talk about the paper.

CHAIR RICHARDSON: So, I had what I was imagining as a discussion, is we could have some open discussion about, you know, what's covered or not covered in the report.

I thought it would be useful to go back once again and consider whether there are implementation issues as you go from kind of our experience thinking about the types of

information we have for various types of claims. And how that gets implemented here.

This is sort of on the backhand side of what IREP's going to do. But I think some of the nuances are important to think about.

One of the major changes I think, from what was in the past relates to going from DREFs where there was some variation by types of cancers, to a kind of single distribution. And that might be worth us reflecting on. And --

MEMBER MUNN: I hope there's also some discussion about what I believe to be the conclusion of the paper. If I'm reading this, the conclusion is that there really isn't more than a ten percent difference in the risk at low dose rates, which I don't see clearly identified either in the papers what constitutes low dose. Everybody has a different idea about that I think.

And but that there's no more than a ten percent difference between the risk at low

dose as there is the high dose. And I'm, given the information that I've seen in other places, not sure I can buy that. So, I hope there's some discussion about that.

DR. NETON: Well, Wanda, this is Jim. There is the appendix in the Health Physics paper that has a discussion on what's considered to be low dose and a low dose rate. There's actually two factors considered in this paper.

There's the LDEF, they call it the low dose effectiveness factor, and then the dose rate effectiveness factor, DREF.

MEMBER MUNN: Yes. I saw that.

DR. NETON: Ultimately, they combine the two under one distribution. But a lot of that early discussion you had on the epi study was just one part of that analysis. That was for the dose rate effectiveness factor.

Well, low dose effectiveness factor was actually just looking at the non-linearity of the Hiroshima, the Life Span Study data. And

trying to come up with some estimates of low dose effectiveness factors based on just the Hiroshima data.

MEMBER MUNN: Yes, the final table, well not the final table, the final figure, I guess --

DR. NETON: Yes.

MEMBER MUNN: -- was very interesting to look at. What seems to be the conclusions, the outcome of the various studies that were listed. But the census risk studies were I think the most interesting of all from my point of view.

And one of the reasons that they were is they really seemed to identify what, at one, they seemed to identify where we are in terms of the various studies that try to look at everything.

And I note BEIR VII routinely stops at one. And I noted this report has potential all the way into below one, which is another issue in my personal attempts to try to understand exactly

what, not only DD is but also what the low dose effectiveness factor figures into it.

And I just, it's very difficult to try to identify in just simple numerals what that means. But this figure I think tells the whole story of what this very well annotated paper talks about. And I, you know, the assumptions that, even the time exposures that were considered, is I think fascinating.

And I don't know that I still feel that there's been any definition that I can hang my hat on. And say, okay, so exactly what are we going to use? There's a single chronic exposure, or sequence of acute exposures in five hours?

Based on what I know of recent cell studies, seems to be fairly reasonable but there's range in there of, in the non-homologous and joining that takes place in the secondary phase. Several hours, I don't know what several hours means.

And it's -- I guess what I'm trying to

say it's very hard to get a grip on which direction to expend your energies here. When the number of different aspects of really significant unknown is -- well, I don't have to tell you. You guys work with it all the time.

But it's hard still to accept the conclusion that there's no more of a difference given this type of approach than ten percent. That just does not seem, the concept is difficult given the information that's been available the last ten, twelve years, I guess.

DR. NETON: Yes, Wanda this is Jim. I'm not sure that ten percent, I really understand myself when they did this harmonic mean. I really have trouble getting my head around that.

The distribution that they've developed or are recommending has a geometric mean of 1.3 which would be applied with a -- depends on whether you use discrete distribution or model it as a log normal, but have a GSD of

something like 1.8.

In reality that is going to be applied in our situation, to almost every case I can think of. Because it starts, DDREF starts being phased in at under 20 rem currently. And that's what they're recommending we stay with all the way down to 1 rem. Most of our exposures for workers in this program are well below the threshold where DDREF would kick in.

MEMBER MUNN: Yes.

DR. NETON: So, it is going to affect almost every case I can think of, and that has been analyzed to date, if we change this value.

DR. MAURO: Ted, this is John Mauro. Is it okay if I jump in a little bit too? I've been listening and I'm very interested in the subject. So, I just want to let you know I'm on the line. And if it's okay, I do have a question, observation, question. Just wanted to know at the appropriate time is it okay for me to join in? Or is it something I should just listen into?

MR. KATZ: David's running the show. So, I think David is probably welcoming all comments.

DR. MAURO: Okay, well thank you. Well I'll make it with this one really quick. It relates to what you were just talking about. When I read that and the simplest -- and by the way, my takeaway of this is -- it's an undertaking.

The folks the people that are knowledgeable at a granular level are a very limited number in my mind. It's a very, I mean I've been in health physics certified for, I don't know, 40 years. And, you know, I read about this. I'm just going to give a little perspective, and there are different levels on which you could start to discuss this.

There's the level of granularity that unless you've lived with the Life Span Study and all of these epidemiological studies, and also these biochemical genetic studies, your whole life. You know, you really can't get into the

granularity of this and to say, you know, where there might be some problems or not.

So, my takeaway from this is that it's going to be hard for anyone unless they've lived with this their whole life to really get into the judgements that were made, what populations that were considered, epi work was considered or not.

So, I can't operate at that level, but I can operate at what I should say a more simplistic perspective of, what's really happened here? So and I think what you were just talking about, I did have one question is, one conclusion is that the DDREF that is being recommended has a range that goes from .047 to 3.6 with I guess, a 50th percentile median of 1.3.

And they do indicate when do you trigger this distribution? That you just answered my first question. Sounds like it's triggered when the doses are below, I guess the acute, the total dose is below 10 rem? I'm not sure. That was see, I was just trying to get a

practical sense. So there is a big range. It's .047 to 3.6. So it's not a --

DR. NETON: It's .47 John.

DR. MAURO: .0. Oh, it's .47?

DR. NETON: Yes, that's what I'm looking at.

DR. MAURO: Okay. You know, I took some notes and I obviously made a typo. Okay, so it's from .47 to 3.6. The spread is therefore about a factor of ten from top to bottom, with a geometric median of 1.3. Is that a log normal distribution that we're operating off? Or is I guess --

DR. NETON: Yes. They took the data and set it to a log normal distribution. I think that's in Figure 1 of the --

(Simultaneous speaking.)

DR. NETON: -- paper.

DR. MAURO: Okay.

DR. NETON: It's not clear to me whether they recommend using that fit or to do

the histogram method based on the one that generated that distribution. But either way that's essentially what would happen.

DR. MAURO: Yes.

DR. NETON: I'm thinking we'd end up putting a log normal distribution in. Seemed to be the simplest if we did this. If we agreed that this was appropriate to do that.

DR. MAURO: How does --

DR. NETON: It's a little different than what we're currently doing? If you look at IREP now, in the Health Physics Journal is a good paper that summarizes all that.

It's a histogram. I mean so many percent for, you know, at this value. So many percent at another value, and it's different. There's a separate histogram for DDREF for breast and thyroid versus all other solid cancers.

DR. MAURO: Yes. Represent the change. It's a method of change from what we're currently doing.

DR. NETON: That's true. I think that the ultimate end point would be that the DDREF, it's a simple matter as suggested here would tend to raise the PC values for our cases.

DR. MAURO: Okay.

CHAIR RICHARDSON: The histogram that you had --

(Simultaneous speaking.)

DR. NETON: Yes.

CHAIR RICHARDSON: The histogram that you previously used goes from basically, because it's basically 0.5, and you had a small amount of weight there. And you, your histogram is distributed around the range 0.5 to 5 as well. It's going to shift your mass a little bit closer to 1, where it used to be centered around 1.8. So, it's just closer to, you know, 1. And the breast and thyroid were already shifted in that way. They were, most of their mass was around 1.

But most of your claims are not, breast and thyroid.

MEMBER ZIEMER: Dave, can I raise some process questions? This is Paul.

CHAIR RICHARDSON: Yes.

MEMBER ZIEMER: First of all the original paper --

MEMBER MUNN: Did you go somewhere, Paul?

MR. KATZ: Yes, we lost Paul, I think.

CHAIR RICHARDSON: Paul, you need to go to your cell.

MEMBER MUNN: I hope you're not --

MR. KATZ: I'm sure he'll come back. I think he must realize we've --

MEMBER MUNN: He's probably talking away.

MR. KATZ: Yes, he's probably hung up on himself is what I'm thinking. But --

MEMBER MUNN: Yes, that's my guess.

MR. KATZ: Easy to do.

MEMBER MUNN: I'm glad he's asking process questions though, because that's on the

top of my mind too. What do we as a Work Group do? What are we responsible for?

CHAIR RICHARDSON: Yes, the previous product that the Science Issues Group provided to the full Board was a very brief memo that Paul actually drafted. I think I had gotten overly academic and verbose. And he did an excellent job of sort of trimming that back down to a few take home points.

He's offering some sentences about the issues. And I think sort of just offering a summarization that went to the Board. And then the Board made a decision about what our, Ted you can remind me, but basically sort of encouraging or discouraging moving forward with the report.

But we're sort of an oversight capacity. So we had some discussion about an issue, and then drafted a very brief memo about what we saw as points or issues.

MS. BEHLING: I looked for some transcripts and I couldn't find anything.

MR. KATZ: That's sounds right. I mean this was in relation to CLL, that's you're saying, David.

CHAIR RICHARDSON: That's my recollection.

MEMBER MUNN: I think so. I mean I think this group has only had one meeting before. That was years ago.

MR. KATZ: We've had a couple, we've had a couple I think. But, yes. But it's been a long time it's true.

MEMBER MUNN: Yes, we were all --

MEMBER ZIEMER: Can you hear me now?

MEMBER MUNN: Yes.

MR. KATZ: There you are.

(Laughter)

MEMBER MUNN: Welcome back.

MEMBER ZIEMER: Yes, right. I switched phones here. Maybe this will help. What I was asking was whether or not we have access to the original reviewer's questions, or were their

comments already incorporated into the versions of the paper that we have?

DR. NETON: Their comments have been incorporated in the paper we have. I have both their comments and SENES', or Oak Ridge's Center for Risk Analysis' response to those comments.

MEMBER ZIEMER: So in your judgement whatever issues were raised by that group have been taken care of. Is that correct?

DR. NETON: Yes.

MEMBER ZIEMER: Okay, then my next question, for the benefit of our Work Group and the Board, it seems to me we need some assessment of some of these matters. Particularly, you know, Wanda raised a number of questions. But there's many more like that, particularly for those of us who are not heavily into this sort of bio-statistics of these things.

I wondered if we can have some review done now? As I looked at the main paper, that is the extended one, not the summary one. There's

a lot of it that is just review of -- for example review of the status of LNT models. Just reviewing works of others, the radiological studies to estimate DDREF.

There's detail on work that's been done by others. But there's some sections like Section 2 of the big report, Derivation of DDREF, which I think would need a detailed review by someone who's capable in that area.

Section 5 on the detailed analysis that this group did on the studies of others. And I think Section 6, which is the detailed description of the development of their probability distribution method for the DDREF. Seems to me we need some independent reviews of those.

I found in this paper an awful lot of assumptions and judgements that I'm wondering -- and they give some level of justification, but there's just an awful lot of judgements and assumptions made on how to develop this

distribution to give different parts of it.

And it seems to me we would benefit from having some independent review of that. I don't know if it's something that SC&A could do for the Board? Or whether our Chair, David Richardson could do that for us?

It might be somewhat awkward since he's an author of many of the papers that are cited, so I don't know if people would see that as unbiased or not?

You know, I like the methods, the uncertainty distribution approach. But there's just an awful lot of assumptions built in it, on the basis that it covers the uncertainties.

MEMBER ROESSLER: Paul, I like what you're saying. That was going through my mind too. This is Gen. When I got back to the early 2000, I think it was around 2004, when our Committee or our Work Group was responsible for reviewing the REF, the radiation effectiveness factor.

MEMBER MUNN: Yes.

MEMBER ROESSLER: And at that time, and I tried to go back and find more information on how we handled that? How was our decision made? And one thing I did find is that we had a review panel at that time. And I jotted down a list of the people who did review that.

Similar in complexity, a different concept, and Dr. Richardson was on that panel, [identifying information redacted], who I think is no longer living if I'm right, [identifying information redacted], [identifying information redacted], and [identifying information redacted] were people we called on then, if I've got it right.

And I think we should probably take the same approach here as to try to find people, maybe as you're suggesting, Paul. It would take different people on different sections.

MEMBER ZIEMER: Well, it may be that that kind of review has already been done. That's

why I asked who reviewed it? Wasn't it Jim Neton got, I think, folks to review the document?

Maybe the review's already been done and I would, maybe we might be satisfied to -- I mean -- looked at these issues, and the judgements and the assumptions.

So maybe we would be satisfied in knowing what they have done and what their comments were, and how they were incorporated? I don't know. That's why I asked the question, because I --

(Simultaneous speaking)

DR. NETON: Well I'd like to --

MEMBER ROESSLER: Yes.

CHAIR RICHARDSON: I'm sorry, go ahead. Well, I think Paul's finished.

DR. NETON: I was going to say that we did get six independent subject matter expert reviews of the Rev 1, or Rev 0 of the document. Now, mind you, that was back in 2012.

The document sat for a while, and I

can't exactly remember why it sat for a long time. And SENES approached us and said, well, let's brush it up a little bit. Because it's 2000, I think they started a new in 2013.

MEMBER ZIEMER: Yes, one of the authors died, Jim. One of the authors died also, right?

DR. NETON: Well, yes. That's what happened, right guys. One of the authors passed away in the interim and then it sat for a long time. And so [identifying information redacted] picked it up. It sat -- you're right.

But they suggested that when they picked it up in 2016 to, there was a few studies that came out in the interim that they added to the document. So, the original six reviewers did not review Rev 1, which they did incorporate their comments. But I think that they were largely, the same approaches were used between Rev 0 and Rev 1. Just updated with some more studies.

So, I could provide the Work Group with those reviews. And SENES's response to those reviews. And maybe we could start there? I mean it's not a small undertaking to go out and try to find independent reviewers on a subject that's this narrowly focused.

DR. MAURO: Say Jim, this John. I'm listening to your concerns. And it's just really a coincidence. I recently read NCRP Report Commentary No. 24. I don't know if you folks are, it just came out I think in 2015, but it's right on target.

It's called Health Effects of Low Doses of Radiation, Perspectives on Integrating Radiation Biology and Epidemiology. And it all goes toward DDREF. And when you look inside at the authors -- so this is a relatively recent NCRP commentary. I read through it, it's spectacular. Just as is [identifying information redacted] report, you know, a lot of parity between the two.

But what I'm getting at is that this work, that went into Commentary 24, is clearly at the cutting edge of all of these. Not only the epidemiology work which David drew upon and made certain judgements, but it also gets heavily into bio-indicators and bio-markers.

And I'm looking at all the different people that contributed to that. These are all really the top tier researchers in this area. And coincidentally, I only bring this up, one of them is [identifying information redacted]. You folks may know [identifying information redacted]. I know Jim, you probably know him.

MEMBER ZIEMER: Yes, we know Ray.

DR. MAURO: Yes, there you go, he was one of them. And I only bring it up because there's really no one at SC&A that operates in this domain.

Except, when I looked at this, Ray, I signed him up as an associate for other purposes a while back. I just want to let you know that, you know,

certainly given that he was an author of this commentary. You know, he's available.

I stay in touch with him regularly. He is now semi-retired. And I get the sense that, you know, he's still very active in health physics. And I wanted to bring that up.

I've got one other thing I was hoping to throw into the mix here, if appropriate. As I said it was just a coincidence I happened to read Commentary No. 24.

And one of my takeaways after reading [identifying information redacted] report, and this Commentary No. 24 is that what's interesting is all of the DDREF distribution work that we've been talking about, all emerges from epi studies, appropriately so.

Where a selection was made on which epi studies, human epi studies, would be used and given different weights. One of the things that comes out of this Commentary 24 is very interesting.

It said that, I think it's time that bio-indicators are used to help inform -- it's almost a quote -- help inform the construction of DDREF distributions.

So, I guess the only technical comment I'd like to just throw into the arena is, when you're in this process is, it's almost, it's coincidental that at this time this commentary would say that this in fact is an opportunity. This might be the first time. You may or may not want to do this.

Is that, is it time to see if you could inform these distributions, DDREF distributions, by factoring in the vast amount of new material that is available to us now regarding bio-indicators, which is not related to epidemiologies really. It's really related to other studies, tissue studies.

I wanted to, that was one of the reasons I called in today is that I happened to have read this commentary, and I was able to have

some opinions that I just wanted to throw into the arena.

MEMBER MUNN: Thank you, John. This is Wanda. We should also be aware of the fact that this bears heavily on the topic that ANS and Health Physics joint meeting this coming September here in the Tri-Cities. It's going to be addressing the issue of the linear non-threshold theory. And it's current information bearing directly on.

So, that request for papers is actually out on that. Anybody who is interested can find it at lowdosereg.org. And as I said, call for papers is out.

So anyone who is interested in being a part of that discussion which will hopefully bear in some way on this. Since this paper says, if LDEF based on analysis overdose ranges differ significantly, it's not clear which LDEF should be used to modify a dose response based on analysis over the full dose range?

Especially when linear fits over both those ranges that are nearly the same. This particular meeting will bear specifically on low doses only, so.

CHAIR RICHARDSON: So, Paul had some suggestions in terms of the large document which we have. And a process for us as a committee to move forward with this given that we've had it for a long time. And the report has evolved somewhat over that period.

So, one had to do with some independent review. And Stu I believe had offered that he could share the responses. I've seen the Rev 0 comments on the report. And they're substantial as well. They maybe run to 25 or 30 pages at least.

With kind of a general overview, it's comments from six different reviewers. And I agree that that would be a useful thing for us to take a look at. I agree also with the suggestion that -- well for two reasons.

I don't think I would be -- either have the time, or necessarily be the appropriate person to offer a review of the work. In part it relates to my work. But sort of only tangentially drawing on it I would say, given that the goal of this report is to derive these distribution ratios. But I think that's going to be, for anybody that's going to be a really substantial undertaking to dig deeper into it.

But it would be, it might useful for us to delineate if we're going to be diligent in evaluating this. And given the impact that changing this distribution potentially could have on redoing calculations.

I think it would be useful for us to have somebody who can provide us with an oversight of that. So that would be my suggestion. Maybe that we all take a look at the independent reviews as a next step. Then consider whether those are sufficient, or whether we'd like to try and investigate the possibility

of having somebody else, whether it's through SC&A or otherwise take a review, take another look at this.

I don't know what the possibility of NIOSH is to go back and ask some of these reviewers to look again, given that the report has evolved a bit? That's another, a third approach possibly.

MEMBER ZIEMER: David, if it hasn't changed very much since Rev 0, I think I would be satisfied with the original reviews if we could sort of get an understanding of what they looked at and how it was resolved.

For example, on some of these assumptions I kind of wondered what the sensitivity would be to changing the assumption. For example if, you know, they would attribute a certain percent of the error to one thing and certain percent to another.

Did they do a sensitivity analysis and see what that would do to the distribution if

your assumption changed? But see they may have already looked at that. So, that's why I'm wondering what those early reviewers, [identifying information redacted]did? And maybe that would satisfy us if we saw that?

CHAIR RICHARDSON: Yes. You know, some of what changed between the revisions in my recollection, I think the new report is streamlined in a sense. They ended up reviewing a lot of radiobiologic literature. And yet in the end, these distributions are coming, you know, really largely informed on human studies.

Because you have more and more assumptions again, when you start to extrapolate from experimental evidence on, you know, a whole range of changes in plants, or cells, or impacts on specific molecules.

MEMBER ZIEMER: Which didn't seem to inform the cancer issue at all anyway, right?

CHAIR RICHARDSON: Right. So, it ends up that this report, you know, is a little bit

more focused on, at least on the epi literature, and they've streamlined that. I don't know that, yes, in the end that's probably not so big.

My recollection is the original distribution, you know, was also very subjective. So, that, you know, what's changed is the shifting of that distribution. But the basis for it was probably not so different than the basis for the distribution in this report.

MEMBER ROESSLER: David, this is Gen. I'd like to respond to some of this. I think it would be good for the Work Group to be able to look at the responses of those reviewers who have already done this.

But I think in a way, we might end up in the same position that those results, in that the Work Group really don't have the knowledge that they do. It might be a little difficult to do that. Although I think we should.

I like John Mauro's suggestion. I think we should consider it. Because often in

our field we look to NCRP for their studies. And I did too. I looked to see what NCRP has done on this. And I also found Report 24.

These people who worked on this have a lot of knowledge in the field. And they spend a lot of time doing exactly what we need. And I really like the idea of getting someone like [identifying information redacted] or there are others who are on that committee, who are knowledgeable, up to date on this, to see if we could have one or two of them do an independent review.

CHAIR RICHARDSON: If we can find somebody willing to do that, I think it would be useful.

MEMBER ZIEMER: You want to see these other reviews first, or do more simultaneous?

MEMBER ROESSLER: To move this along it would seem like if it's possible, that we should do them simultaneous.

DR. NETON: Well, I would like you

guys to maybe look at the reviews first. Because you might be surprised at the breadth of the reviews. And the subject matter experts, I guess I don't know, I guess with the Work Group I can release the names. Normally, we sort of redact that.

But these were high level people in this business that reviewed this document. One of them is close to with the NCRP, so to say.

MEMBER ROESSLER: Very closely probably.

MEMBER ZIEMER: Well I don't know if we need the names necessarily. I mean I would trust that you selected the appropriate people. I'm more interested in their comments at this point. But if --

(Simultaneous speaking)

MEMBER ZIEMER: -- and you may want to get the permission from them to release their names to the Work Group. But --

DR. NETON: Well, if we don't have to,

I think I have a generic version that has all the comments and SENES's response to those comments without the names.

MEMBER ZIEMER: Well, I wonder what do the others in the Work Group think, I mean?

DR. NETON: What I might be able to do is provide the names of the reviewers, but then not identify whose comments correspond to those --

MEMBER ZIEMER: That's fine.

DR. NETON: That would be okay. Because then you would know something about the pedigree of the reviewers.

MEMBER ZIEMER: Right, that's good.

MEMBER MUNN: Yes, it really doesn't matter to me where they came from. As has already been said, we certainly trust the judgement in selecting these folks. We know that they know what they're doing. Otherwise they wouldn't be cranking this kind of stuff out.

But I'm particularly interested in a

point that Paul brought up. Those of us who have ever worked with any kind of models at all know how sensitive one item can be in the way we put models together, which are, as we all know, also by definition, wrong.

But nevertheless if we can identify that someone actually has looked at sensitivity of these various assumptions, especially with respect to the weighting. You know, I have no feel for how one goes about weighting these things.

So, how one can decide, I'm assuming is background knowledge that folks who do this sort of thing put more faith in one set of data than another. But if that's been looked at, I'd certainly like to know it.

MR. KATZ: Wanda, one thing is typically modelers do their own sensitivity analysis too. I don't know that those were available from the original, from the authors. But it's a pretty common practice to run your own

sensitivity analysis on these kind of matters.

MEMBER MUNN: Yes, but for those of us who don't do it, we have no idea how deeply that --

MR. KATZ: No, no I'm just saying that the authors themselves may have those analyses available if you want to know.

MEMBER MUNN: Really like to see them. See what they've done. Whether they mention it in the comments.

MR. KATZ: Yes.

DR. NETON: And I'm not altogether sure, this is Jim, that they didn't discuss it in this 400 page tome.

MEMBER ZIEMER: I would have guessed they would have, Jim. But I guess we need to confirm it.

DR. NETON: I would agree. If you just look at the health physics summary paper, it's pretty breezy. The treatment of how they came up with these values. But I haven't read

the 400 page document in a while. But I recall that they did some good soul searching about how to approach it. And which values would be used, that sort of thing.

MEMBER ZIEMER: They certainly explain their thinking. But, you know, it's always good to have somebody independently either confirm that, or also to see how sensitive it is to a change in value. And they may have done that, but it wasn't clear that they did.

DR. NETON: I agree.

MR. KATZ: And the only other thing I just wanted to add just sort of along the lines of what, I think you mean, you were saying. Was that, I think it would be better for the -- as opposed to the parallel approach, it would be better to look at the peer reviewer's work first. Because I mean, they were well collected.

Now going and then adding, I mean they were independent in the first place. I mean NIOSH selected them, but they were independent of

NIOSH. And going and then finding another couple peer reviewers, it's just sort of adding a larger peer review. So, you're not necessarily any more independent simply because the Board would selecting them instead NIOSH.

MEMBER ZIEMER: Yes, that's why I thought we should get their comments. They're independent both from the authors and from NIOSH. So, I think it would be helpful to see them.

MEMBER MUNN: Let's do it and give us a month to think about it, to slog through them.

MEMBER ROESSLER: Sounds good to me.

CHAIR RICHARDSON: Yes, I think that sounds great. There's certainly, the people who provided the comments are engaged with this topic and with the literature. I mean they're very informed comments.

The only issue I really have is the fact the document has changed subsequent to that. But in part the changes, they are revisions in response to those initial comments. So, that's

not necessarily a bad thing.

And we'll get some sense from it. So, that sounds good to me as a way forward.

MEMBER ZIEMER: Dave, what happens after that? Let's say that we're, after we see that it gives us a good level of confidence in this document. Is it our job then to make a recommendation to the full Board to either go with the new model, or to not go with the new model, depending on our judgement? Or what happens after that? Or Ted, maybe you can tell me what happens?

CHAIR RICHARDSON: Yes, my recollection is that what we would do would be to provide some sort of short memo to the Board, basically of the issues. And maybe, yes, now don't the Board recommend to NIOSH the support for a report? Ted, what's the process?

MR. KATZ: Yes, I mean it's certainly within the Board purview to do it. It's within the Board's purview to consider any changes that

NIOSH makes based on evolving science. And if NIOSH proposes a change here, I mean then certainly it's the Board's role to make a recommendation that it believes that change is appropriate, or it has concerns about the change, or what have you, whatever you might find, yes.

MEMBER ROESSLER: I think that's what our Work Group did on the REF decision, is the Work Group made a recommendation to the Board.

MR. KATZ: And the Board did this on CLL too. It concurred that it was appropriate to add CLL and so on. So, within changing science, when NIOSH decides it wants to make a change, it's certainly the Board's role and appropriate for it make a recommendation supporting that or suggest any changes it believes might be appropriate, whatever the case may be.

MEMBER ZIEMER: And as a practical matter. Let me ask Jim Neton or Stu, if this new model is adopted, are we obligated then to go back at every previous denial and recalculate?

DR. NETON: Yes, this is Jim. We'd go back and we may attempt some sort of triage to see what the magnitude of the effect is.

MEMBER ZIEMER: Yes, in other words, yes.

DR. NETON: You're right. If we did that, we'd rerun ever case, it's not just denied.

MEMBER MUNN: Oh, holy cow.

MR. KATZ: You'd rerun every case that could possibly be changed, right?

MEMBER ZIEMER: Yes.

DR. NETON: Yes. I have a suggestion on one thing that we might want to do, on your term here, is that the Board meeting happens to be in Oak Ridge this time.

And SENES is based, or Oak Ridge Center for Risk Analysis is based out of Oak Ridge. And if it would be useful we could arrange to have them provide the Board a summary of their report if there's interest in that. I just thought I'd throw that out there.

We did this with the radiation effectiveness factors. And I thought it was pretty helpful for the Board to hear an overview of the thought process and how this all came about.

MEMBER MUNN: That's a good idea.

CHAIR RICHARDSON: I think that's the best idea. I think if we're going to provide the Board with a memo. It would be great for them to have some context, so.

MEMBER MUNN: Well yes.

DR. NETON: I can't speak that their Oak Ridge Center for Risk Analysis is available during that time period, but I can certainly ask.

MEMBER MUNN: I don't know about Kocher, or Hoffman either one, but I think --

DR. NETON: Yes, I think David actually did a pretty good with the radiation effectiveness factor discussion. And he is now the principal author of this paper with the Health Physics Journal publication.

MEMBER MUNN: Yes, and --

DR. NETON: And also for us, you know, we're talking about independent reviews again. And the fact that this was in press in the Health Physic Journal, this paper also indicates that it's has yet another round of peer review.

MEMBER ZIEMER: Right, separately, yes.

DR. NETON: Yes, so I'm just questioning how many reviews we need, you know.

MEMBER ZIEMER: Yes, yes.

MEMBER ROESSLER: It's my understanding it has not been reviewed yet by --

DR. NETON: Oh, no it's been reviewed. It's at the galley proof stage.

MEMBER ROESSLER: Oh, it is. Okay that makes a big difference.

MEMBER ZIEMER: Yes.

MEMBER MUNN: It certainly does.

DR. NETON: Yes, this is a revised version that responded to all of the review

comments they received. And now it's -- I don't think they have the galley proofs yet, but it's at that stage.

MEMBER ROESSLER: Okay, you're right. It's a big thing.

MEMBER MUNN: Very good, yes I would like very much if we could get either Kocher or Hoffman. I think they're both in the Oak Ridge area still, aren't they, I think?

MEMBER ZIEMER: I know Owen is, I think Kocher is too, isn't he?

DR. NETON: [identifying information redacted] is still there. I just found out recently reading the Health Physics Newsletter, Owen Hoffman is now President Emeritus of Oak Ridge Center for Risk Analysis. He's still involved but Iulian Apostoaei is the President now.

MEMBER ROESSLER: Yes --

MEMBER ZIEMER: And he's there too, is he not in Oak Ridge?

DR. NETON: Yes, they're all in Oak Ridge, all of them, [identifying information redacted]

MEMBER ZIEMER: Yes.

MEMBER ROESSLER: I'd like to voice my -- sending in [identifying information redacted] if we can get him. As someone said he's very articulate. He did such a good job before. I would envision the fact that if he goes back to --

DR. NETON: Okay, well if I get a positive from you guys, I'll try to arrange that, work through Ted.

CHAIR RICHARDSON: So that meeting in April?

MR. KATZ: It's April 11th and 12th I think.

MEMBER MUNN: Just enough time to look at their comments, yes.

MEMBER ZIEMER: You know, let me add something else here. I think even if we weren't

ready to make a formal recommendation at that point. It still might be useful since we are down there to let them, you know, as kind of a status report, to have such a presentation.

MR. KATZ: Yes, this is Ted. And I was actually thinking that you would not be making recommendations at that point. This would be both useful for your interrogating the issues. And as I think David said, nice for sort of bringing the rest of the Board along at least in the general sense about what's going on.

Since this is going to be sort of a reach for some of the other Board Members, that you can, you know.

MEMBER ZIEMER: Yes, Ted. So we'll have to make sure that the presentation is user friendly for the Board.

MR. KATZ: Yes, to the extent it can be, right.

MEMBER MUNN: Now [identifying information redacted] does that.

DR. NETON: Yes.

MEMBER MUNN: At the 8th grade level? Try it.

MEMBER ROESSLER: Well, to our level.

MR. KATZ: You know, I mean our Board Members have been around and are more sophisticated --

MEMBER MUNN: Well yes, but it's still an intellectual stretch to get around it.

MEMBER ZIEMER: Well, I'm not just talking about the other Board Members. I'm talking about us on the Work Group.

MEMBER MUNN: Me, talk about me.

MEMBER ZIEMER: I'm talking about me. Okay?

MEMBER ROESSLER: Wanda, don't sell yourself short.

MEMBER ZIEMER: No, no. I'm but, you know everybody thinks everybody else understands it and you know, you're the only one that doesn't. And it's not true, you know.

MR. KATZ: I mean certainly we could have another Work Group teleconference closer to the Board meeting. And sort of check in on each other and see how far along we are with understanding and considering the issues.

If you want to do that and then you can decide at that point.

MEMBER MUNN: I would certainly love to get enough information to assure myself that what I think is the conclusion here, is not actually the conclusion. Because that's a very large pill for me to swallow. But, yes, let's do it.

CHAIR RICHARDSON: Great, so NIOSH will circulate via Ted, the six reviews on Rev 0 of the report. And Ted, or somebody from NIOSH will ask [identifying information redacted] is he would be willing to present a summary to the Board. That would be great. And we will have a chance to look at those and hope to have a discussion maybe in March, after we've looked at the materials provided. And at least check in

prior to the meeting in Oak Ridge.

MEMBER ROESSLER: Should we pick a time now?

MR. KATZ: If your calendars are handy and want to now, we can do that. Or I can do that closer to the time. Whatever you want.

MEMBER MUNN: I'd prefer you do it closer to time, because I don't have any feel about how much time it's actually going to take to look at these.

MR. KATZ: Well, I mean I'd schedule the Work Group for, you know, later in March so it would be reasonably close to -- you'll have as much time as possible. But we can schedule it now or we can schedule it later.

CHAIR RICHARDSON: Do you want to send out an email Ted, and maybe we can --

MR. KATZ: Yes, I can do that, that's fine. It's easier that way, you can take your time on the material on this.

CHAIR RICHARDSON: Okay.

MR. KATZ: I'll do that.

MEMBER ZIEMER: That sounds good.

CHAIR RICHARDSON: All right. Well, that's tremendous progress for how long it's taken us to get here.

(Laughter)

MEMBER MUNN: Well, thanks for the intellectual exercise, I think.

MR. KATZ: That's great. It keeps you young, Wanda.

MEMBER MUNN: Gees. It sure keeps me off the streets. And that's beneficial to my community.

MEMBER ZIEMER: If that's what it takes to keep us young, I think I'm going grow old here.

(Off the record comment)

MEMBER MUNN: You're absolutely correct. So be it. All right, we'll hear from you, Ted?

MR. KATZ: Yes, I'll send you out a calendar next week, a calendar request.

MEMBER MUNN: Super.

CHAIR RICHARDSON: That sounds great.

MR. KATZ: And yes, we'll send out the documents. And Jim will keep us updated on [identifying information redacted]

MEMBER MUNN: Thank you, okay.

MEMBER ZIEMER: Very good.

CHAIR RICHARDSON: Great, thank you. Have a good weekend.

Adjourn

MEMBER MUNN: Everybody have a great one. Bye, bye.

CHAIR RICHARDSON: Thanks everybody.

(Whereupon, the above-entitled matter went off the record at 4:24 p.m.)