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SCIENTIFIC REVIEW PANEL ON TOXIC AIR CONTAMINANTS
AIR RESOURCES BOARD
STATE OF CALIFORNIA

PUBLIC MEETING

TRANSCRIPT OF PROCEEDINGS
Friday, April 26, 2002
10:07 A.M.

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I N D E X

LOS ANGELES, CALIFORNIA; FRIDAY, APRIL 26, 2002
10:07 A.M.

PROCEEDINGS

CHAIRMAN FROINES: So we have a quorum. So we'll officially open the meeting on April 26, 2002 -- the meeting of the Scientific Review Panel. We're changing the agenda slightly.

But before getting to the agenda, I had Peter give each of you a letter we received from Paul Gosselin. We cannot take up the letter today because it's not -- it was not put on the agenda. So we'll take it up next time. But it concerns

follow-up to the exposure discussion we had in January.

So we can -- we will take it up -- I got the letter yesterday at 3:00 o'clock. And so there wasn't any chance for us to put it on the agenda ahead of time. So we'll take it up next time. And the letter is extremely important because of the close attention to the issue of exposure assessment and monitoring that the panel has focussed on with respect to pesticides. And so we'll want to take it up at the next meeting.

We're changing the agenda. We're

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going to take up methyl isothiocyanate as the first agenda item because we really want to try to get through MITC and we want to be able to discuss the prioritization document. And so if something had to drop off at the end, it would have to be Melanie's noncancer chronic -- her chronic RELs.

And but we definitely want to get to carbon disulfide. And, finally -- I'm forgetting something. But why don't we go ahead? So Tobi, Andy -- who's going to be the lead? Welcome.

DR. RUBIN: Is this one working? Hello? Okay. Okay. I'd like to bring the panel up to date on the status of the MITC 1807 health assessment. As we're all aware, completion of the SRP proceedings on MITC hinges on consideration of a revised draft of MITC 1807 document.

A number of changes have been inserted into the document since it was last considered and accepted by the panel back in May of 2000. I've detailed those changes in a memo dated January 29. I believe the panel got copies of this memo. But if not, there are some available here out on the table.

For your reorientation -- next slide -- I've summarized the changes in the first overhead. I'll just read through this very quickly.

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I intend to concentrate today only on Number 1. But just -- and then, if there are any questions on any of the other changes, you can ask me afterward or even now.

First of all, subchronic risk is now estimated using the newly submitted 4-week rat inhalation toxicity study of Klimisch as opposed to the 13-week rat inhalation study of Rosskamp.

2. There's a more detailed account of the critical human eye irritation study of Russell and Rush, which underlies our acute evaluation. And this account emphasizes, in particular, the robustness of the results at the LOEL value -- at the LOEL dose.

3. There's greater methodologic detail regarding the calculation of ambient and application site exposures. And they've been added as footnotes to Tables 9 through 12.

4. 1- and 8-hour acute air concentration estimates and the resultant risk calculations have been provided to accompany the 24-hour data that appeared in earlier drafts of the report.

5. Application site studies that would not currently be legal under recent technical

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information bulletins and product labels are explicitly recognized in the document.

6. First, the uncertainties inherent in after-the-fact modelling of MITC air concentrations after the July, 1991, Sacramento river spill are emphasized. And the results of modelling calculations done by DPR, OEHHA, and the metam sodium task force are presented in the document.

Second part of 6: The results of the Kreutzer et al. epidemiologic study on the spill are presented. This didn't add any appreciable new information but filled in some detail.

7. There's a complete description of the Earlimart MITC exposure incident of November 13, 1999.

8. The CDPR illness surveillance data have been updated to 1999.

And, 9, in some issues that have come up only in the last two to three weeks -- and these are not yet in the document -- there is now going to appear a treatment of benchmark dose modelling of the subchronic rat inhalation toxicity study as well as some emendations relating to the statistical power of the study.

These arose out of discussions that

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occurred between DPR, OEHHA, and members of the Scientific Review Panel.

Finally, there is a correction or a flushing out of some data on an acute study that doesn't really affect any of the NOELs or LOELs in the study, but I just wanted to bring you -- bring that to your notice.

CHAIRMAN FROINES: Andy, I have a procedural question.

DR. RUBIN: Okay.

CHAIRMAN FROINES: Elinor, this is important. Has the panel been given Andy's emendations? Or is it only Paul, Stan, and myself who have seen those proposed changes?

DR. FANNING: I think everybody received it in Jim Behrmann's e-mail of this Monday.

PANEL MEMBER FUCALORO: Yes. Downloaded it. Yes.

CHAIRMAN FROINES: Well, the reason I ask the question is we obviously can't approve our own findings if there is something of substance to be added. As long as the panel has seen the proposed -- the proposed changes that are going to go into the

document and approves the document with an understanding that those changes will be

incorporated, then we're okay as a procedural matter. And I think we are okay.

Okay. Go ahead.

DR. RUBIN: I've also brought --

PANEL MEMBER BYUS: I don't mean to interrupt you. I have one brief question. I've got the January 31st document. And you just handed out a January 29th document. I'm just trying to read them to determine which -- what's the difference?

DR. RUBIN: Oh. Is that the --

PANEL MEMBER BYUS: Should we be going by the 31st or the 29th?

DR. RUBIN: Is that the changes?

PANEL MEMBER BYUS: The changes. They look like they're similar documents. But there is a later one than the 29th.

DR. RUBIN: I wrote -- I may have gotten -- it's probably my fault. I wrote -- it's exactly the same document, except I think the later one has the exact pages --

PANEL MEMBER BYUS: No. It's somewhat -- it has some differences. I just wondered what they are. That's all I'm asking.

DR. RUBIN: I don't think there are any differences --

PANEL MEMBER BYUS: Okay.

DR. RUBIN: -- of any substance.

PANEL MEMBER BYUS: Okay.

DR. RUBIN: And the benchmark dose and the statistical power emendations, I've also brought copies of here, in case anyone doesn't have them. And they're out on the table.

The issue that's commanded most of my attention has been the subchronic endpoint in regulatory NOEL determination. In previous drafts of the MITC 1807 Health Evaluation, the critical subchronic NOEL was based on the 13-week rat inhalation study of Rosskamp, which we've already discussed at some length in these proceedings.

However, as Rosskamp was extremely problematic on technical, reportorial, and study-design grounds, we were pleased to have the opportunity to examine another inhalation study, this one a 4-week study by Klimisch et al.

While the Klimisch study was not perfect by any means, as if there is such a standard, it was notably superior to the Rosskamp study in its ability to supply a supportable regulatory NOEL.

I've got on this overhead just a brief review of the study design. And I'm going to go

through this as quickly as possible. This was a study conducted in Wistar rats by animals per sex,

per dose -- 3 doses along with the control -- 1.7, 6.8, and 34 ppm.

It was a 4-week study, 5 days per week, 6 hours per day. These were whole-body exposures. MITC aerosol was generated by a gentle thermostatic heating of liquid MITC. Analytic determinations were done by adsorbing the MITC in 2-propanol, measuring by gas chromatography. There were 6 samples done per group per day.

The analytic aspects of this experiment were under very well -- very good control, particularly when you compare it to the Rosskamp study. The observations -- fairly standard toxicologic observations. Body weights done on a weekly basis. Clinical signs done daily; serum chemistry and hematology, just before sacrifice; and histopathology, following sacrifice.

Next overhead.

Just to give you a sense that, at the high dose, these animals, particularly the males, were adversely affected. You can see, even by Day 7, in the males, that the animals are losing weight. None of the other dose groups were differentiable

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from controls. The weight loss -- this, by the way, is a slide of body weight gain, not absolute body weight.

So throughout the 28-day period, the high-dose animals were unable to even get back to their starting weights.

Females, interestingly enough, did not show statistically significant difference at the high dose. There certainly was a tendency there. In fact, 3 females did lose weight in the first week.

However, 1 of the 5 females steadfastly refused to lose any weight. And she was the one who made sure that this was not a statistically significant effect, but the tendency is there. And, again, there is no strong tendency at any -- at either the low or the mid-dose for weight loss.

The conclusion we draw from this is that the high-dose males -- less so the females -- are not well at the high dose, while the animals appear unaffected at the low and mid-doses.

Next overhead.

Clinical Signs. High-dose animals full of clinical signs. Mucous membrane and respiratory tract irritation evidenced by reddish

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nasal discharge, salivation, and eye discharge resulting in a changed breathing pattern and whooping respiration, intensified cleaning behavior, stretched posture, eyelid closure, somnolence, ruffled fur.

Certain of these signs, the ruffled fur and the respiratory sounds, eventually became irreversible. In other words, at the beginning of

the experiment, at the high dose, the animals would exhibit these signs at the -- for the duration of the exposure but would soon recover. After a few days, they didn't recover anymore.

At the mid-dose, we saw eyelid closure, somnolence, and ruffled fur. These types of signs are difficult to interpret. The only conclusion that I draw from this is evidence of failure to thrive. No other strong conclusions possible. But the animals certainly affected at the mid-dose. And no clinical signs at the low dose.

Next overhead.

Hematology Results. Hematology did evidence some response, particularly in the males at both the mid and the high doses. This is a table of neutrophilic polymorphonuclear granulocytes.

PMNs are considered primary nonspecific respondents to infection. In this case

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the response may be to some sort of tissue damage. This effect was considered evidence by the contract lab itself of lung inflammation, though it must be regarded as inferential, as the measurements were from the general circulation, not from the lung itself.

Nonetheless, we do see a statistically significant effect in males at the mid-dose. Granted, it's only about maybe a 30, 40 percent effect. It goes up to more than a tripling at the high dose. Females -- the statistically significant effect is only present at the high dose.

Next overhead.

Now we get into the area that's caused a lot of discussion, shall I say, between ourselves -- among ourselves in the medical toxicology branch, between DPR and OEHHA, and ultimately with the panel as well.

The histopathology results on the lung and on the whole respiratory tree. At the high dose, rhinitis, which is a nasal mucous membrane inflammation; metaplasia of the nasal epithelium; tracheal epithelial proliferation and single-cell necrosis; bronchopneumonia; bronchial and bronchiolar epithelial proliferation; emphysema; and nasal

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epithelial atrophy.

These animals are highly affected in the lung at the high dose.

At 6.8 and 1.7 ppm, I'm going to give you evidence that nasal epithelial atrophy was increased at these two -- at the mid- and the low doses. It is not overwhelming evidence. And our conclusions require a lot of -- required a lot of thought, a lot of discussion. And I'll try and show you the evidence now.

Number -- next overhead.

CHAIRMAN FROINES: I would stick to the

objective evidence and leave out some of the subjective talk about the evidence.

DR. RUBIN: Okay. Okay.

What the study did was to take histopathologic -- four histopathologic sections from each animal, numbered S1 through S4. If you -- the first thing you notice, when you look at these, at the incidence data, is that there was no nasal epithelial atrophy in Section Plane 1 for some reason never explained in the study.

The only conclusion that I could come to was that, for whatever reason, S1 was not influenced in this particular effect. Either the

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proper -- the appropriate cell types were not there, there was something about how the MITC was entering into the nasal cavity -- we just don't know.

If you look at S2, you'll see we have it divided up as focal and nonfocal nasal epithelial atrophy. In males, the very top line there --

I don't have a pointer here. Is there a pointer here? Okay.

-- you see an increase -- from 1 animal of 5, to 2 animals of 5 -- between the control and the low dose, no further increase at the mid-dose, and going to zero at the high dose.

Same with females -- from 1 to 3, to 1 to zero. If that were the only incidence that we were looking at, I would have said right away that nothing happened in this experiment with respect to nasal epithelial atrophy.

However, if you look at nonfocal atrophy, which we are interpreting as a somewhat more serious level of atrophy -- in other words, the focal islands of atrophy, if you will --

Have it now? Oh, thank you.

Let's see here. We're looking at the Row 2 here. The focal islands of atrophy have now spread and become indistinguishable from each other,

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if you will. That's an interpretation on our part. You'll see that in males, it goes 1 in 5, 1 in 5 -- whoops. I'm highlighting the water in here -- to 2 of 5, to 5 of 5.

So, by the high dose, all the animals are experiencing nonfocal nasal epithelial atrophy and the same with the females. What we're interested in here is that, since the focal atrophy is going down and the nonfocal atrophy is going up, we are persuaded that there is an increase in severity between the low dose and the mid-dose and that, in fact, between the control and the low dose, there's an increase in nonfocal atrophy, which eventually, as you go up in dose, becomes nonfocal.

In neither of the remaining two section planes do you see a strong effect in this direction. You do see some evidence in males at

the -- in S3 for an increase in focal. No evidence in females. And absolutely no evidence in Section Plane 4.

My interpretation of this was that Section Plane 2 was the farthest out toward the air and may have received a larger effect of MITC dose. I fully recognize that we're dealing with very low numbers here. So I want to make sure that you're

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aware of this.

If you look at the data on a per-rat basis -- next overhead -- in other words --

PANEL MEMBER FUCALORO: Excuse me. You had some uncertainties about interpretation of the paper, I mean, that you mentioned.

DR. RUBIN: Yeah.

PANEL MEMBER FUCALORO: Was there any attempt to contact the authors?

DR. RUBIN: No. I never -- this was a study done in 19 --

PANEL MEMBER FUCALORO: 86.

DR. RUBIN: -- 86? Yeah. Well, put out --

PANEL MEMBER FUCALORO: Well, published in '87.

DR. RUBIN: Well, put out in '87.

PANEL MEMBER FUCALORO: Sure.

DR. RUBIN: It was done in Germany.

PANEL MEMBER FUCALORO: Ich spreche Deutsch.

DR. RUBIN: Yiddish.

PANEL MEMBER FUCALORO: And that too. I'm from Brooklyn.

DR. RUBIN: Yes. So -- no. The answer is no.

PANEL MEMBER FUCALORO: Okay.

DR. RUBIN: However, I will say that the study

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authors never even mention nasal epithelial atrophy as an effect. This came out of the toxicological analysis of the study done by DPR, in particular Dr. Tom Moore, who did the initial analysis of this study. It was concurred upon by OEHHA, as well.

The next slide -- overhead -- Number 8.

PANEL MEMBER FUCALORO: Let me just follow up. I mean, what you're getting at -- I mean, rather than just, you know, we're getting this line of reasoning -- you're getting at how you chose a LOEL --

DR. RUBIN: Yes.

PANEL MEMBER FUCALORO: Isn't that basically where you're going?

DR. RUBIN: Yeah.

PANEL MEMBER FUCALORO: And you're opting for the lower concentration, I can gather from -- infer from your comments.

DR. RUBIN: Yeah.

PANEL MEMBER FUCALORO: Okay.

DR. RUBIN: This slide is the last data slide.

Instead of looking at the data with respect to section plane incidence, you combine the incidence from each section plane and express, instead, on

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per-rat basis. You see the same pattern, particularly if you look at the total. In other words, you combine males and females. It goes from 3 of 10, to 5 of 10.

And then, as we now expect, the focal starts to go down a little bit and then goes seriously down by the high dose; whereas, the nonfocal atrophy goes up all the way through the experiment until all the animals are experiencing this at the high dose.

Now, if you combine both focal and nonfocal atrophy -- which, I believe, is valid because I believe them to be part and parcel of the same process -- you'll see that it goes from 3 of 10, to 6 of 10, stays at 6 of 10, and goes to 10 of 10. This is not a statistical -- at the low dose or the mid-dose is not a statistically significant effect.

I've now, at the urging of the panel, put in a back calculation, if you will, of the Fischer exact calculations -- what would it -- how many animals would it have taken to generate a statistically significant effect at the low dose?

And by my calculation, it would have taken anywhere from 18 to 22 animals to get a statistically significant effect.

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If, instead of looking at the data on a per-animal basis, you look -- you just count up all the sensitive or the susceptible section planes, you see that it goes from 6 out of 30 total section planes, to 11 out of 30, stays at 11, and then goes to 30. In other words, all the section planes showed nasal epithelial atrophy.

Next slide. Conclusion: The incidence of nasal epithelial atrophy rose at the low dose of 1.7 ppm.

Next slide. Here, I just tried to summarize the strengths and weaknesses.

I'll start with the weaknesses. For the most part, statistical significance was not achieved until the high dose. 2 of the 3 section planes showed little or no evidence of response at the low and mid-doses. And there was only inferential evidence of increased severity, but it's one that I support strongly.

In other words, that focal-to-nonfocal progression that occurred between the low and mid-doses in Section Plane 2 was evidence of the increased severity.

The strengths of pointing out low doses as the LOEL -- first, that such an effect was

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plausible in view of the known irritant properties of

the compound and, I would add, in view of what happened to the animals at the high dose, which is severe irritation.

2. The most clearly affected section was Section Plane 2, which was the closest to the outside air and thus likely received the higher effective MITC dose.

3. Females showed a statistically significant -- I forgot to show you this with the section planes. Females showed a statistically significant increase in total atrophy at the low dose when incidence was expressed as the fraction of all the section planes exhibiting this character.

4. Focal and nonfocal atrophy represented a progression in the severity of a single pathologic process. Thus it was legitimate to consider their incidence rates together as representative of total atrophy. Thus we can accept that there was an increase in severity when going from the low to the mid-dose.

And, 5, total atrophy when expressed on a per-rat-incidence basis was increased in both sexes at the low dose.

Next slide.

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PANEL MEMBER FRIEDMAN: Can I interrupt? I have a question regarding Number 2. I thought I heard you say before that you surmised that the Section 2 was closest to the outside. Did the authors actually state that it was?

DR. RUBIN: The authors never state this. It was a deduction that I did based on the appearance of the data. There's a fair amount of inference in this data. I would not even attempt to hide it.

The fact that we saw an effect in Section Plane 2 and not in section plane -- and less so in 3 and less so, again, in 4 was some evidence.

I would add that metaplasia of the nasal epithelium, which is something that was only seen at the high dose, only occurred in Section Plane 1. So that also was some evidence that Section Plane 1 was closest to the outer edge.

I practically memorized this study looking for some evidence of -- some statement of the direction of these cuts but --

PANEL MEMBER BLANC: But there's no inference that it's directional? You're just not sure which direction it is, but there's clearly directionality?

DR. RUBIN: Yeah. That's correct.

PANEL MEMBER BLANC: So that's not a

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presumption?

DR. RUBIN: That's correct.

PANEL MEMBER BLANC: So that's important and consistent with a biological mechanism.

PANEL MEMBER FRIEDMAN: What do you mean "directionality"?

PANEL MEMBER BLANC: Sequence. Either it's the closest in or the farthest away. And either of one of them, you could invoke the biological mechanisms. That would make sense.

What wouldn't make sense is if it was some random pattern where it was 2 and then not 3 and then 4. That would be far more difficult. But there are various hype- -- biologically consistent mechanisms why something that's closest or farthest may be the most affected.

PANEL MEMBER FRIEDMAN: But we don't know whether it's --

PANEL MEMBER BLANC: But I'm saying it doesn't matter. What matters is that there's directionality.

PANEL MEMBER FRIEDMAN: I think we didn't know where those cuts were made.

PANEL MEMBER BLANC: Well, I think -- I think that any scientific -- I think it's rational that, if their order -- if they're in some direction, that the

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ordering isn't random of the planes. You may not be able to say, directionally, which plane was -- with complete certainty, which plane was closest to the outside air.

But there's no reason -- it would be illogical to approach the data that the sections -- if they're numbered 1, 2, 3, 4, 5 -- are in random sequence.

PANEL MEMBER FRIEDMAN: Then why isn't Number 1 closest to the outside or farthest in? Why is it Number 2?

PANEL MEMBER BLANC: Oh, I don't -- he's not saying that Number -- he is saying that Number 1 precedes Number 2.

Is that your statement?

DR. RUBIN: Yes.

CHAIRMAN FROINES: Quite frankly --

PANEL MEMBER FUCALORO: There's nothing there.

CHAIRMAN FROINES: -- that I think we should stop talking about what is closest because we haven't the slightest idea which is closest.

PANEL MEMBER BLANC: No. But I'm saying it doesn't matter.

CHAIRMAN FROINES: I agree with you. I don't think that's an issue. But I think we -- well, I

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want to get away -- I want to stay away from speculation. I want to stay with what we know rather than speculate what may be and draw conclusions based on what we know rather than --

PANEL MEMBER BLANC: And all I'm saying is that we don't need to invoke a discussion of what's closest and what's farthest as long as there's a systematic effect.

And I would also say that I actually disagree with Number 1 under the "Weakness" column as a relevant point because the data, in the same way

that there's directionality, in fact, there's a dose response.

And what surprises me in all of the discussion that you present, although it's inherent in the benchmark approach, is that, in fact, these data are statistically significant in chi square test for trend.

And that's true if you separate them by gender or if you do summary chi square stratified by gender with a summary statistic. And, in fact, it's less likely to be chance when you look at it as a test for trend than if you look at the highest versus the lowest and the middle versus the control and the lowest versus the control.

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So to me it doesn't matter that, in isolation, the lowest exposure dose is not, in and of itself, statistically significant because I don't think that would be the correct statistical analytic approach in any event. And I would appreciate Stan's comment in that regard.

PANEL MEMBER GLANTZ: Yeah. I totally agree with that. And that's why I was encouraging you to use the benchmark dose approach because that allows for a dose-response relationship.

So I mean -- in the other thing, I mean I think that the point that you've already made about there being low power in the study is another important point.

I mean I did a similar kind of calculation to what you did. Just asked, you know, "If we had the same pattern and the results, how big -- how big would the study have had to be?"

And I came up with about the same. I came up with, I think, 15 rats. But really it's about the same as what you did -- 15 or 20.

PANEL MEMBER BLANC: By my calculation, the "Keisware" test for trend with a strat -- adding the stratification male-female -- P-value of .004.

DR. RUBIN: That's pretty -- fairly

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significant.

PANEL MEMBER BLANC: And if you a priori combined the males and females into a single 2-by-4 "Keisware" test for trend, it has a P-value associated with it of .003. So but -- so there isn't even a need to do the a priori combination of the males and females in order to choose statistical significance.

PANEL MEMBER GLANTZ: Yeah. I think that ought to be added. I hadn't thought of doing a test for trend. But I think that would be worth it -- the points that Paul just made ought to be added to the report.

DR. RUBIN: Okay. Point taken.

Can I move on? Just a couple more slides. Since we are interested in ambient--

CHAIRMAN FROINES: I think that, at some point, we should have a larger discussion about this notion of statistical significance. I come from UCLA, where we have people like Sander Greenland, who never would accept a statement about statistical significance.

And certainly Paul and Stan, I think, are in the same camp. And so this notion of tying decision-makings in public health context to a simple

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P-value, I think is -- we should try and avoid that simplicity.

DR. RUBIN: Right. I mean I totally agree with that. And I think this analysis is evidence of my opinion on that. You -- we are interested in, for the 1807 process, in ambient and in application site air.

When we get a study -- when we get an inhalation study, it's an intermittent exposure -- this one being 6 hours per day, 5 days per week -- and we feel it necessary to make some kind of transformation of the data to estimate what would be the equivalent toxicologic effect were the animals exposed or were people, as it were, exposed all of the time -- in other words, 24 hours a day, 7 days per week.

We use a Haber's law extrapolation to do this. In other words, we basically make a proportionality going from 5 days a week to 7 days a week and from 6 hours a day to 24 hours a day. And that cuts the LOEL from 1.7 ppm to 300 ppb.

This is not -- I actually called Peter Witschi on this point. Not everyone would agree wholeheartedly with invoking Haber's law in a situation where you have an irritant, but it is a

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default that we are using so that we can estimate an effect level for people that are constantly exposed.

Due to the considerable uncertainties inherent both in establishing an effect at the low dose and in the applicability of Haber's law in the case of a subchronic irritational response, an uncertainty factor of 3, instead of 10, was used to estimate the critical subchronic NOEL of a hundred ppb.

Next slide. Okay. This slide grew out of all the --

PANEL MEMBER FRIEDMAN: I'm sorry to interrupt again. If there's greater uncertainty, why would you use a smaller uncertainty factor? Because of this great uncertainty, instead of using a factor of 10, you use a factor of 3. I don't quite understand that reasoning.

DR. RUBIN: Well, I -- my view of a straight Haber's law extrapolation is that this would be -- in my view, anyway, this is probably -- possibly an overestimate of the effect.

PANEL MEMBER FRIEDMAN: I see. So you're worried about uncertainty in that direction, not in either direction?
DR. RUBIN: Right.

CHAIRMAN FROINES: I don't understand what --
PANEL MEMBER BLANC: I think -- let me see if I understand what you're saying. What you're saying -- and I think it's not unreasonable.
What you're saying is that, because you're dealing with an irritant response, which is likely to be more potent with a high -- with a high dose and not linear in its response in that way, that smoothing it out using Haber's law is extremely conservative --
DR. RUBIN: Yes.
PANEL MEMBER BLANC: -- because it's -- with an irritant response, one might presume that it's far more important to have a short-term, high-level exposure than a longer-term, low-level exposure. And since that, in itself, is quite conservative, that further multiplying that by a factor of 10 would be less appropriate than using a factor of 3. Is that a --
DR. RUBIN: That's a fair statement.
PANEL MEMBER FUCALORO: Yes.
PANEL MEMBER GLANTZ: Yes. I actually thought about that too. And I agree with that too, although I did have to think about it a little bit. It might help the document to just take the -- use what Paul

just put forth and just add a sentence in the document in the appropriate place 'cause it did -- when I first read it, I had exactly the same reaction Gary did.
DR. RUBIN: I'll have to go back and read what I actually said there.
PANEL MEMBER GLANTZ: Yeah.
DR. RUBIN: Okay. On benchmark dose modelling, the benchmark dose approach offers an alternative to the conventional NOEL-LOEL approach to determining regulatory doses or concentrations.
The benchmark dose is -- I'm quoting from a US-EPA document. Crump was the first author on this document. "Benchmark dose is a statistical lower confidence limit on the dose, producing a predetermined level of change in adverse response compared with the response in untreated animals."
A major advantage -- or it's possible that it could be a disadvantage in some cases -- benchmark dose values are established by modelling the dose-response relationship using the entire data set from a toxicologic study as opposed to using the single determining LOEL dose relied upon in the conventional approach.
Now, that means, of course, that

what's happening at the high dose is influencing the slope of the curve at the low dose where we're interested in establishing regulatory NOEL values. I'm not sure at all that that's biologically justifiable, but it may be. We just don't know.

In the current case, using the per-rat nasal epithelial atrophy incidence data -- the 3 in 10, 6 in 10, 6 in 10, 10 of 10 -- and using Haber converted air concentrations -- in other words, instead of converting the final effect, we just convert the concentrations that they were exposed to -- we found that they -- that these data were best approximated by a probit curve model.

This was also pointed out to us by OEHHA in their critique of our document. Using this model, the 5 percent lower bound effect level was 75 parts per billion, and the 10 percent lower effect level was 148 parts per billion. These values effectively bracketed and supported the conventionally derived NOEL of a hundred ppb.

So we feel that's fairly strong support. In other words, if you invoke the entire incidence curve, you get a number pretty close to the LOEL value that we calculated. There are uncertainties in both approaches, but they support

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each other, in my opinion and in the opinion of Dr. Glantz, if I may say.

PANEL MEMBER GLANTZ: Yeah. Yeah. I think this really strengthens and complements the previous discussion. And also I think -- the one thing which I told Andy I won't, like, hammer on -- but I really think it would be helpful to put the graph in the document to illustrate that it is, in fact --

PANEL MEMBER FUCALORO: I think so.

PANEL MEMBER GLANTZ: I mean there's very limited data here. So it's not dazzling. But I think putting that, the graph showing the fit in, would be a nice thing to add. I won't, like, insist on it. But I would like to see it in there.

And I don't know what other people think or thought about it. Okay. I guess you don't have to put the graph in there. No one else is jumping up and down. But it would make me very happy.

CHAIRMAN FROINES: It seems to me that there's an interesting policy dilemma that you have. Your standard -- what do you call it? -- your conventionally derived value is the methodology that was established by the Food and Drug Administration in 1957.

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DR. RUBIN: Uh-huh.

CHAIRMAN FROINES: And then we have the benchmark approach that Kenny Crump first described in 1983.

DR. RUBIN: Uh-huh.

CHAIRMAN FROINES: And here we are in 2002. And we're using the conventional approach, which is now 45 years old, as the method of operation.

And it seems to me that, when we have data that -- we've spent so much time talking about the benchmark approach with Melanie because, in some cases, they have data and can use the benchmark approach and, in some cases, they don't have the data to use the benchmark approach.

But where they have the data, they consider it the better approach to developing risk assessment for noncancer endpoints.

And so it seems to me that it would be valuable for DPR to move to at least 1983 on this issue rather than sticking with the Food and Drug Administration, who originally designed this approach to define how much filth should be allowed in food.

And it seems to me it's time to move past that because you -- because at some level, OEHHA and -- both you, using the benchmark approach, come

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up with a value of around 75.

DR. RUBIN: Yeah.

CHAIRMAN FROINES: You then -- what you call the 6.7 percent effect level in your document -- but the .05 level -- the 5 percent level -- is the 75 percent level.

So, in essence, you're make a policy decision to accept a less -- a greater effect, if you will, greater percentage effect than the 5 percent level. So you are not taking the same level of methodologic conservatism that the benchmark approach gives you at the 5 percent level.

And it seems to me that one should say, "Okay. Now, why is 6.7 percent an acceptable value when by, in general, people would accept 5 percent?"

So that there's a contradiction, it seems to me. And to say, "We're going to stick with the 6.7 percent effect level because that's the way we've been doing it from time immemorial," probably is not the best answer to the question.

So it's not something that we need to resolve today, but it is certainly something we need to resolve over a period of time because clearly the benchmark approach, where there is adequate data,

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seems to be the better approach.

Tobi?

ASSISTANT DIRECTOR JONES: Could I speak to the panel? And for those of you who have forgotten who I am, I am Tobi Jones, Assistant Director of Division of Registration Health Evaluation.

And I think what Andy can tell you in a lot more detail than I, as the generalist manager of the division, is we're in the process of evaluating, using benchmark dose as a methodological

tool, working very closely at -- OEHHA uses it.
I think the discussion that Andy has had with members of the panel on this particular study has been very useful to him in seeing how it's applied.
I don't think we're quite ready to make that entire conversion. And I think, if -- you know, if you would like further discussion here on where we are in that deliberation, Andy is probably perhaps the better one to discuss that. So, yes, I think we take your comments to heart, John. But we're not quite there in making that wholesale conversion. Okay?
PANEL MEMBER GLANTZ: Well, if I could just -- I think, as a practical matter and as Andy just

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pointed out, you get about the same number either way. And given the uncertainties that are implicit in all these calculations, I don't think it's worth having a huge fight about. So I think you've moved a little bit by even agreeing to put it in the report.
But I really strongly agree with John.
I think that the benchmark dose approach, when you have the data to do it, is a much, much more dependable way to approach these issues because it makes use of all the data at once, because it's based on a more plausible sort of model -- mathematical model of what's going on, and because it doesn't have the problem that you always have when you do these NOEL-LOEL calculations where the results are a strong function of what dose you happen to study. And so --
PANEL MEMBER FUCALORO: That's right.
PANEL MEMBER GLANTZ: -- you know, if the Klimisch group had picked different doses for their study, you'd have come up with different numbers. And that's -- I think it's a real problem.
Whereas, when you use the benchmark approach, as long as you're assuming a reasonable mathematical function for the dose-response relationship, the results are going to be much less

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dependent on which specific doses you study.
CHAIRMAN FROINES: And one can look at different mathematical formulations for that dose response.
PANEL MEMBER GLANTZ: Yes.
CHAIRMAN FROINES: One doesn't only have to use the probit model.
PANEL MEMBER GLANTZ: Well, that's true too. And, you know, it's always nice if you do that and you find the results are not too -- not relatively insensitive to that assumption.
So, you know, I think in the context of this report, you know, I don't think we gain anything by hammering on this, at this point, especially since the department does seem to be

thinking about moving into the 80's on this issue.
But I really think it would be much better if that was the -- I mean, if I had my druthers, that's how you'd do it.
PANEL MEMBER FUCALORO: Yeah.
PANEL MEMBER GLANTZ: And, in fact, just for the record, since we may not get to Melanie's REL document, there's one place in there where she actually did it your way. And I heard her saying, "No. No. Go back to your benchmark model."

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CHAIRMAN FROINES: I'm not arguing that you make changes, but I just wanted to make the point that --
ASSISTANT DIRECTOR JONES: I wanted --
PANEL MEMBER GLANTZ: I guess what we're saying -- it would be nice, when the next document comes, if you could do it.
ASSISTANT DIRECTOR JONES: Yeah. But I think one thing to keep in mind is we work very closely with US EPA. They do not wholesale use the benchmark test approach.
PANEL MEMBER FUCALORO: What? I can't hear you.
ASSISTANT DIRECTOR JONES: They do not use wholesale on anything --
PANEL MEMBER BLANC: Well, it is lucky -- it's lucky for federal EPA, then, that they don't have to come to this panel. But you do. And I would take very seriously what John Froines --
PANEL MEMBER FUCALORO: You're tough guys, Mike.
PANEL MEMBER BLANC: -- said because we will not be generous if a document comes to --
ASSISTANT DIRECTOR JONES: My point being, though, that because we work closely with them, in

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doing risk assessments on pesticides --
PANEL MEMBER GLANTZ: Right. Right.
ASSISTANT DIRECTOR JONES: -- of course, we're very interested in using the most applicable methodology. But at the same time, you know, completely ignoring what EPA's doing all the time doesn't maintain a good dialogue.
PANEL MEMBER FUCALORO: May I ask a question? Let me say is there any controversy that, when the benchmark approach is applicable, it is superior to the other? Is there any controversy in that statement?
If there's none, it seems to me that the benchmark approach would be the one you should be moving toward. I mean that's that --
DR. RUBIN: I'm not sure that there's --
PANEL MEMBER FUCALORO: Or, in fact, if there's even a superior one -- you said the 80's. Is there something even better yet? I'm not a statistician. But if, in fact, there were -- it

seems to me that what's being said here, I agree with. If you can use the benchmark approach because it is a superior approach, it gives us better values, by all means.

I think that's what you're saying, though. I understand. I understand that you have --

ASSISTANT DIRECTOR JONES: I'm not --

PANEL MEMBER FUCALORO: I understand that you also have to converse with EPA --

ASSISTANT DIRECTOR JONES: I'm not a -- I would leave the question of superiority of a technique like that to Andy to comment on. I would say, from Andy's supervisor -- Dr. Pfeifer -- I don't think we're quite there yet in assuming in all cases that it is the superior approach. But Andy's the toxicologist.

CHAIRMAN FROINES: Why don't we leave it to -- well, I mean I'm at fault for raising a general issue but --

PANEL MEMBER GLANTZ: Well, I actually just want to pound on this slightly more.

PANEL MEMBER FUCALORO: Keep going.

PANEL MEMBER GLANTZ: No. I think this is an important point. I mean the -- we're not saying that you should always use it mindlessly. But I think, in this case, it would have been better. And when you can, you should.

As for the EPA, the federal EPA -- and some of my best -- I have lots of friends at the federal EPA. But, you know, this panel has a long history of considering what the federal EPA says and thinks but not being the least bit bound by it. And we like to think often we do a better job than they do --

CHAIRMAN FROINES: I think that the --

PANEL MEMBER GLANTZ: -- or California does a better job than they do.

CHAIRMAN FROINES: There is -- there are -- since Stan went a little bit ahead, I'll go a little ahead too.

PANEL MEMBER GLANTZ: He and I are, like, a really bad influence on each other.

PANEL MEMBER FUCALORO: Let's not go over the top here.

CHAIRMAN FROINES: But I'll make -- I just want to make one comment. And that is that obviously one of the limitations of the benchmark dose is the quality of the dose data. If you have poor exposure information, you have a hard time using it.

Oftentimes we have very bad or very weak exposure assessment data. And that comes from the fact that the studies that we have to work with did not do an effective job, either in toxicology or in epidemiology, to do adequate exposure assessment.

Then what happens is people come in

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and comment and say, "The exposure assessment data wasn't very good; so therefore you can't take this study very seriously."

So we start to reward poor exposure assessment. And that's obviously contradictory. It seems to me that we have to do what we can to get as good exposure data so we can use the more advanced models.

The more -- this is not -- I mean this is a probit model looking at dose response. This isn't the beeswax you, you know. I mean this is a fairly simple-minded innovation when you think about it.

So that but it does depend upon the exposure information, which does take us back to the larger issue of "How adequate are the studies that we have to work with?" And that's a topic for another discussion, I think.

PANEL MEMBER BLANC: Can I ask a procedural question?

CHAIRMAN FROINES: Sure.

PANEL MEMBER BLANC: Andy, in terms of the other points that, in your first overhead --

DR. RUBIN: The 9 points?

PANEL MEMBER BLANC: Yeah.

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-- at what point would you like us to engage you on those?

DR. RUBIN: I'm -- at any time you wish. I realize we're -- there's a time factor here and whether we can get through the other presentations, but I'm, you know -- I'm prepared to answer any questions.

PANEL MEMBER BLANC: Well, will you clarify how the other presentations will deal with this document? Or are the other presentations on another topic?

DR. RUBIN: The other presentations today on another topic.

PANEL MEMBER BLANC: So you're the only presenter?

DR. RUBIN: This is it on MITC.

PANEL MEMBER BLANC: You finished your presentation? Okay. So then I have a rather substantive area of discussion related to your presentation.

DR. RUBIN: Yeah.

PANEL MEMBER BLANC: And it would help me for you to clarify, because it's been a while since the last presentation, to what extent does the current health assessment revision in terms of including this

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critical data from the Earlimart incident differ from the last document? It's not formatted in a way that I can actually see what's a text change. Was that included at all previously or --

DR. RUBIN: No. No. It's a totally new section.
PANEL MEMBER BLANC: Can I, then, that being said --
DR. RUBIN: Yeah.
PANEL MEMBER BLANC: What didn't -- what I could not grasp easily in the document -- and let me walk you through this.
So -- I want to make sure I understood the data -- based on mathematical modelling of the exposure in this real-world incident, it was felt that most of the people who were symptomatic had experienced acute exposures of between .5 and 1 part per million --
DR. RUBIN: That's right.
PANEL MEMBER BLANC: -- is that correct?
So in other words, at 500 to a thousand parts per billion but certainly as at low -- at as low a level as 500 parts per billion, 80 percent of the people seemed to be symptomatic or some number like that.

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DR. RUBIN: Yes. In other words, several days after the incident, a DPR team went in there and interviewed people in the neighborhood.
PANEL MEMBER BLANC: Right. Right.
DR. RUBIN: It's not -- they didn't attempt to cover the entire neighborhood. But they interviewed -- I don't know -- a hundred and seventy people or something like that and found that a very high proportion of those people experienced irritation-type symptoms and other symptoms -- dizziness and things like that.
PANEL MEMBER BLANC: Right. Right. Just talking about the irritation, let's take that which is the most straightforward in those kinds of incidents. And yet the NOEL upon you which you base your acute exposure value was 220 parts per billion, based on a laboratory experiment with fairly pure --
DR. RUBIN: Yes.
PANEL MEMBER BLANC: -- MITC.
I felt very uncomfortable with 220 parts per billion being the NOEL if, at 500 parts per billion, 80 percent of those exposed were having acute irritant symptoms.
DR. RUBIN: I looked at this -- it's funny because I looked at those data as being quite

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supportive of that, of the LOEL and NOEL. In other words, those people were exposed at the very lowest concentration that the people in the lab were exposed -- now, that was a human study, if you remember -- and experienced similar symptoms.
In my book, that was supportive of the LOEL determination.
PANEL MEMBER BLANC: Well, perhaps you should walk us through exactly what the NOEL and LOEL were

in the laboratories. The reason why it's critical is because, in the real world, of course, when you have a metam sodium release, people are not only exposed to MITC, they're exposed to hydrogen sulfide and MIC. And all three of those are clearly mucous membrane irritants.

DR. RUBIN: Right. Well, the human eye irritation experiment was done using MITC under highly controlled conditions -- humans. U.C. Davis Medical Center established a LOEL dose of a 800 ppb based on subjective eye irritation -- in other words, "My eyes feel irritated."

And they had a statistical method for determining how, you know, whether --

PANEL MEMBER BLANC: Right.

DR. RUBIN: -- it was something there or not.

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PANEL MEMBER BLANC: And what was the dose below 800?

DR. RUBIN: 220.

PANEL MEMBER BLANC: Was the next low dose at which they had --

DR. RUBIN: The next lowest dose --

PANEL MEMBER BLANC: But wouldn't your data from Earlimart suggest that 500 parts per billion, not very conservatively, is the lowest effect level if you're saying your modelling was absolutely correct and that there was not anything like the linear response? There was some kind of threshold.

But doesn't that suggest that the lowest effect level was not 800 but 500 parts per billion? And certainly you don't have a 500-parts-per-billion exposure level in the experimental study.

DR. RUBIN: Yeah.

CHAIRMAN FROINES: How many people were in the experiment?

DR. RUBIN: There were 9 to 16 people per dose. So it was one of the -- and it's mentioned in the assessment that that's one of the weaknesses, that this is a rather low number and that, even at 220, where we couldn't make any statistical

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statements about different -- being different from controls, it is possible that the level of response may have strayed -- may have gone above control levels.

But we could not make any --

PANEL MEMBER BLANC: Well, what was the --

Wait. Wait. Now let me follow up.

What was the level of the prevalence of irritant symptoms in the 220-parts-per-billion exposure group?

DR. RUBIN: There was no irritation.

PANEL MEMBER BLANC: None? Zero?

DR. RUBIN: Yeah. But at 800, all of the participants in the experiment experienced

irritation.
PANEL MEMBER BLANC: And not a single person in the 200 --
DR. RUBIN: And not a single person -- if you go back and look at the traces, you might think that "Oh, possibly this person may have experienced something," but based on the noise factor, even among the controls, we couldn't make a distinction there. We're looking at eye-blink responses --
PANEL MEMBER BLANC: I see. I see.
DR. RUBIN: -- and subjective responses and so

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forth.
PANEL MEMBER BLANC: But they also had a subjective questionnaire or a score?
DR. RUBIN: What they did wasn't a questionnaire. They were presented with a line in front of them.
PANEL MEMBER BLANC: Right. A visual analog scale?
DR. RUBIN: Well, it's called a Lykert scale. You probably know more --
PANEL MEMBER BLANC: Well, a Lykert scale isn't a line. It's actually -- a Lykert scale is an ordinal -- "very strongly agree," "sort of agree" -- it's numeric. An analog scale is a line where you mark between 1 and 10.
DR. RUBIN: Yeah. Well --
PANEL MEMBER BLANC: So it's not exactly a Lykert. But anyway --
DR. RUBIN: That's what they did. They put a mark on a line based on how serious it was. And then --
PANEL MEMBER FUCALORO: What you're saying is that there was no distinction between no dose and the 220 -- and the people who responded to 220?
DR. RUBIN: Right.

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PANEL MEMBER BLANC: No statistical difference? Or were they mathematically the same?
DR. RUBIN: They were not differentiable.
PANEL MEMBER BLANC: Did they present the numbers in the paper? I mean I haven't seen the paper.
DR. RUBIN: Oh, yeah. They're all there.
PANEL MEMBER BLANC: So they presented it as a mean with a standard deviation?
DR. RUBIN: Yeah.
PANEL MEMBER BLANC: A mean score? I mean this is kind of critical because you've got a group of people exposed and you model that it was between 500 and a thousand parts per billion. So certainly 500 was the -- and at that, 80 percent of the people had symptoms, not 10 percent of the people.
PANEL MEMBER ATKINSON: But surely one of the problems is that the 500-to-a-thousand ppb is a mere estimate. I mean there were no actual measurements

apparently taken.
PANEL MEMBER BLANC: Well, okay. But we're supposed to be public health conservative. It's a little hard -- do you see the contradiction between these two data sets? I don't think it's supportive of the -- you took it as supportive of the 200

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NOEL --
DR. RUBIN: Yes, I did.
PANEL MEMBER BLANC: -- and I take it as making me very uncomfortable. It's much too close to this level at which --
CHAIRMAN FROINES: Tobi?
ASSISTANT DIRECTOR JONES: Paul, could I -- again, with my generalist hat on -- I think Roger's comment is very salient.
I think you're -- I think you're placing an accuracy on our modelling efforts and because there weren't monitoring data to go along with that, the 500-to-a-thousand parts per million, or ppm range, may not, in fact, reflect what occurred.
We use -- and Randy is a better modelling expert than I here. But I think that one shouldn't consider the results of our modelling as an absolute against which we compare a human subject study.
CHAIRMAN FROINES: Yes. But Paul's right. We are -- in general, have a tendency -- tend to want to make public health decisions that are protective.
And so Roger's absolutely correct. Your modelling may have uncertainty on both sides and

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that, in fact, the distribution of exposures may be log -- it's probably logged normally. It may be logged "normally distributed." And so who knows where the -- what the best central sense of the central tendency is.
But if there's uncertainty on both sides of the value, then to make an a priori assumption that 500 may not be correct, with the implication that there's only one side of that uncertainty distribution, I think, is a mistake.
I think one has to assume that you may be off by a factor of 10 or a hundred or what have you on both sides. And then you have to make a decision about how protective that you think you need to be. And in some respects, that would argue for taking a lower estimate of exposure as your -- as your LOEL rather than a higher one.
PANEL MEMBER GLANTZ: Uh-huh.
CHAIRMAN FROINES: Isn't that -- am I --
PANEL MEMBER GLANTZ: I have a question. This is a point I completely missed, I have to admit, when I read the report.
But let's say that the only information you had was the results from that spill

and you knew that, at your best estimate of 500 parts
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per billion, 80 percent of the people had a response. If you then applied your usual kinds of uncertainty factors, what would you come up with as your NOEL?
DR. RUBIN: Basically if we had a LOEL of 500 ppb is what you're saying --
PANEL MEMBER GLANTZ: Yeah. Yeah. Where you had an 80 percent response.
PANEL MEMBER FUCALORO: 500 to a thousand. Yeah.
DR. RUBIN: Yeah. So we would take the lower part. We would take 500 and divide it by 10 --
PANEL MEMBER GLANTZ: Yeah.
DR. RUBIN: -- to generate a NOEL.
PANEL MEMBER GLANTZ: Yeah.
PANEL MEMBER FUCALORO: I think that's Paul's point.
PANEL MEMBER GLANTZ: Which gets you to 50.
ASSISTANT DIRECTOR JONES: One other consideration about the analysis of the Earlimart exposure is it most likely was not pure MITC, as the human study was, because it was the result of metam sodium application. And others here -- Randy and folks from the Air Board -- can speak to the possible contributions of H2S and MIC.
PANEL MEMBER BLANC: Well, that's why I think
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it's more applicable.
PANEL MEMBER FUCALORO: That's what he said --
ASSISTANT DIRECTOR JONES: But it also -- that trying to draw conclusions about an on-the-ground incidence relative to MIC becomes difficult because you have more complicated factors than you do for the laboratory study.
CHAIRMAN FROINES: But the basic flaw --
PANEL MEMBER GLANTZ: The way to look at it is it's much more relevant to the real world though --
CHAIRMAN FROINES: Well, it's not only more relevant --
PANEL MEMBER GLANTZ: -- because complicating factors exist in the real world.
CHAIRMAN FROINES: The problem with the regulatory approach to science, it seems to me, is that we do this chemical by chemical at a time. Nobody's exposed simply to MITC. People are exposed to H2S.
In fact, if you look at your document in terms of breakdown of metam sodium, it breaks down to H2S and MITC. So, in fact, what we should be dealing with here is a document that assesses the risk from the breakdown products that are MITC, MIC, H2S, even carbon disulfide.
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And the fact that we're only addressing a single chemical leads to underestimating risk rather than overestimating risk.

DR. RUBIN: I agree with that.
CHAIRMAN FROINES: I mean, in fact, we should have a document that says, "Here's what people are exposed to. And here's the risk associated with that."
So the single chemical-by-chemical approach is fine in the abstract, but it's not the real world.
PANEL MEMBER GLANTZ: Well, so, Paul, what do you think should be done?
PANEL MEMBER BLANC: So, Andy, can we walk this through? So, then, if you took 500 parts per billion as the LOEL and assumed you had no NOEL --
DR. RUBIN: Yeah.
PANEL MEMBER BLANC: -- then you would divide that by a hundred?
DR. RUBIN: 10.
PANEL MEMBER BLANC: Divide it by 10? And then what --
DR. RUBIN: Because it's a human study, there's no extrapolation from animals to humans, assuming that there's a tenfold difference in

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sensitivity in the human population.
PANEL MEMBER BLANC: And then? Is that what your --
DR. RUBIN: That would be a NOEL -- 50 ppb.
PANEL MEMBER BLANC: And then if the NOEL were 50 ppb, the REL would be?
DR. RUBIN: Would be 5 ppb.
PANEL MEMBER BLANC: Instead of 22.
DR. RUBIN: Right.
PANEL MEMBER BLANC: Don't you think that would be more conservative?
DR. RUBIN: Well, anytime you go lower, you're more conservative.
PANEL MEMBER BYUS: Is it more accurate?
DR. RUBIN: I'm very reluctant myself to use the after-the-fact modelling for regulatory values. I -- just so many uncertainties there -- what the flux rate and the wind direction and all the kinds of things that had to be done in order to estimate those numbers.
And I fully recognize that, in the real world, you are exposed to more than MITC. However, I have held in this document that we have analytically established values in a human experiment. To me that was a very, very -- that's a

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very strong argument for using that experiment to --
PANEL MEMBER BLANC: Except that there's a problem with that experiment, which is that you have one dose with nothing and one dose with a hundred percent effect. And, in fact, actually, I'm not sure that it's nothing because I haven't seen the data.
So there is a critical point as to whether or not it comes -- it's exactly parallel to

this discussion of the test for trend if, in fact, the mean symptom score was 5 compared to 3 but was not statistically significant and there was a big standard deviation.

That's quite a bit different than if the mean symptom score was 5 and 5 compared to nothing. So --

DR. RUBIN: Yeah. Well, I did look at that data pretty hard. And I mean I admit it's been a while.

PANEL MEMBER BLANC: Right.

DR. RUBIN: So I -- the controls show a splay of response, if you will -- the ones that had goggles on and just air passing through the goggles --

PANEL MEMBER BLANC: Right.

DR. RUBIN: -- some people, when they put the goggles on, marked some level of irritation to begin

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with --

PANEL MEMBER BLANC: I understand that. But I'm asking what the mean score was, not what the standard deviation. You're addressing the variability.

DR. RUBIN: Yeah. Yeah. It -- I think that's in the document.

PANEL MEMBER GLANTZ: You can look in your report, if you want.

PANEL MEMBER FUCALORO: You can stop and look it up if you want.

DR. RUBIN: Yeah. Let's just take a look at the document.

PANEL MEMBER GLANTZ: We don't expect that you have memorized the entire document.

DR. RUBIN: I thought I had it memorized. Yeah. Let's see.

Okay. So what I have here in the document as far as the subjective, the Lykert scale -- mean responses at those times, at 1 and 2 hours, expressed as the percentage of the full Lykert scale indicated by the subject were 25 percent, plus or minus 14 percent, and 26 percent, plus or minus 14 percent. That's at 1 and 2 hours.

1- and 2-hour, air-only controls

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exhibited responses of 6 percent, plus or minus 9 percent, and 5 percent, plus or minus 8 percent.

PANEL MEMBER BLANC: So you're saying -- okay. So what you're saying is that, in fact, there was a response. It just wasn't statistically significant. In fact, you're saying there was a fourfold increase in the --

PANEL MEMBER FUCALORO: It seemed like a big response.

DR. RUBIN: No. That's at the LOEL, at the LOEL dose compared to air-only controls.

PANEL MEMBER BLANC: Oh, at 800.

DR. RUBIN: Yeah.

PANEL MEMBER BLANC: I'm sorry. And what was it at the NOEL?
DR. RUBIN: 800 was at, say, 25 or 26 percent.
PANEL MEMBER BLANC: So it wasn't a hundred percent of the people had symptoms.
DR. RUBIN: This is -- that is not -- that's not an incidence. That's a percentage of the full Lykert scale.
PANEL MEMBER BLANC: Okay. Okay. Okay. Okay. And what was the percentage at the NOEL?
DR. RUBIN: And at the NOEL -- at the -- no. These are the controls --

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PANEL MEMBER BLANC: What page are you reading from?
PANEL MEMBER FUCALORO: What page? Yeah.
DR. RUBIN: Page 19.
PANEL MEMBER BLANC: Oh, "Health Effects"?
PANEL MEMBER GLANTZ: Part C.
DR. RUBIN: I'm looking to see if there's any 220 in here. Page 19 about 6, 7 lines down. Let's see. At the bottom of Page 18 -- "In a 4- and 8-hour test, subjects exposed to point -- to 220 ppb MITC did not amount to statistically significant irritation response to the test material."
I actually don't have the --
PANEL MEMBER FUCALORO: Boy, a table would have been helpful.
CHAIRMAN FROINES: What page? I --
PANEL MEMBER GLANTZ: Bottom of Page 18 up to 19.
CHAIRMAN FROINES: 18?
PANEL MEMBER GLANTZ: Part C, the very bottom of Page 18.
DR. RUBIN: I did not put in there the numbers for the 220. I just said that they not did not exhibit a statistically significant response. I can put them in there, if you'd like.

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PANEL MEMBER BLANC: Well, I think I'm suggesting more than putting them in there. I'm suggesting that, if, in fact, there was a difference, albeit not statistically significant in and of itself, that perhaps you should be using a benchmark approach and calculating a dose response.
DR. RUBIN: My recollection of the data is that there was no difference, statistically --
PANEL MEMBER BLANC: Not --
DR. RUBIN: -- statistically and biologically. In other words, looking at the data, I could differentiate 220 from controls. I mean obviously I'm going to go back and look at that to verify it.
PANEL MEMBER BLANC: Well, I mean, absent -- absent other data, I would say, Andy, that I'm going to strongly urge that the panel reject an acute REL of 22 and suggest that the data support a REL of 5.
PANEL MEMBER GLANTZ: Well, given that you

have expressed that very strong feeling, do you think it's worth, maybe, to get whatever else we can out of the way and then table this discussion and maybe you can get somebody up in Sacramento to fax down the paper so we can get the numbers rather than just speculating?

I don't think it's fair to Andy to

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expect him to memorize it.

PANEL MEMBER BLANC: No. No. I'm just --

PANEL MEMBER GLANTZ: But I think then we could have a -- we could kind of conclude the discussion based on the actual numbers because it would be nice to bring this to closure today but to give them a chance to get the stuff, rather than to just speculate. Does that seem reasonable?

CHAIRMAN FROINES: What do you think, Andy?

DR. RUBIN: I'm willing to go back and dig the numbers out and fax them out to you, if you'd like.

PANEL MEMBER GLANTZ: No. What I was suggesting is that somebody back in your office that you could get on the phone with and tell them how to rummage through your desk and find it and fax it down here today.

DR. RUBIN: Oh, today. Oy.

PANEL MEMBER GLANTZ: If we sort of finish -- if we tabled this specific discussion for now, deal with any other issues, and then go on to another agenda item while you have someone rifle through your office and --

DR. RUBIN: What they would have to do is actually go through the study itself --

PANEL MEMBER GLANTZ: Yeah.

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DR. RUBIN: -- which is on my desk.

PANEL MEMBER GLANTZ: Okay. Well, you would could talk them through it.

DR. RUBIN: But they would have to -- it actually takes some doing because you have to go back into these traces and so forth. I'm not confident that that's going to be possible today.

PANEL MEMBER BLANC: So, Andy, isn't there a published paper, though?

DR. RUBIN: No.

PANEL MEMBER BLANC: No?

DR. RUBIN: This is a contract study.

PANEL MEMBER BLANC: I see.

PANEL MEMBER FUCALORO: Can I --

PANEL MEMBER GLANTZ: Well, maybe. But I think -- I think that maybe to try -- maybe we can't get it today, but it is an important point and if you could get at least try to get somebody to fax down the relevant pages, then maybe we could have a more informed discussion.

CHAIRMAN FROINES: Is that -- I mean given that we're going to close here at 2:30, quarter to 3:00, what makes sense?

PANEL MEMBER FUCALORO: I can't leave at quarter to 3:00.

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DR. RUBIN: I'm not of --

CHAIRMAN FROINES: Before we assume, I think we need more input from the panel on Paul's comment because the panel needs to decide how it wants to proceed as a whole.

PANEL MEMBER FUCALORO: Andy -- this is another issue -- is the data presented -- can you present the data in tabular form for easy -- you wrote it up here --

DR. RUBIN: Yeah.

PANEL MEMBER FUCALORO: -- in the report. I read it. In tabular form -- "This many -- this concentration. This many hours. This percentage along the line" and so on and so forth -- can that be presented in tabular form for ready evaluation?

And I think what Paul's suggesting is that, regardless of statistical studies, we should be able to see a difference or we might see a difference between 226 or whatever that number was -- 220 -- and another -- and no dose, for example, you know, either yes or no.

And that would show up in some tabular form that you could present. Does that seem --

DR. RUBIN: No. That's not unreasonable.

PANEL MEMBER FUCALORO: Okay. Now, what

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you're suggesting, though, it might not be possible to do today, as Stan suggested, because one would have to --

PANEL MEMBER GLANTZ: Well, I'm just suggesting he try. If they can't --

PANEL MEMBER BLANC: Well, let me -- maybe there's another way of approaching this that would be helpful. You know, I could be an outlier here and --

PANEL MEMBER FUCALORO: Not you.

PANEL MEMBER BLANC: -- and -- what?

PANEL MEMBER FUCALORO: Not you.

PANEL MEMBER BLANC: Yeah. And if I am, then, although it would be nice to have this other data, it's not that important fundamentally. You would stay with the 220 value, which then yields a value of --

PANEL MEMBER FUCALORO: 22.

PANEL MEMBER BLANC: -- 22. If there is a strong feeling on the panel beyond my own view that it actually wouldn't matter and even if there were no biological or statistical difference in the laboratory control study of 220 parts per billion compared to 800 parts per billion in eye irritation, the epidemiologic data with mathematical modelling is more compelling and the 500 value should be used as a

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low effect level, then there is no need to get the other data.

It would be nice to see everything running, you know, together. So I think that it's important -- it's important for me to hear what other people are thinking.

CHAIRMAN FROINES: Well, let me summarize -- Would you like to make a comment?

PANEL MEMBER FRIEDMAN: I just feel, now that the issue has been brought up, that I would like to see those data too.

CHAIRMAN FROINES: Okay. Let me --

PANEL MEMBER FUCALORO: I'm sorry. Gary, what did you say? I didn't hear you.

PANEL MEMBER FRIEDMAN: I said I would like to see the data, too, now that the issue has been brought up. I think it's now sort of on public record; and now that we're all facing that question, we should see the data.

PANEL MEMBER FUCALORO: We should see the data.

PANEL MEMBER GLANTZ: Why don't we go on to the next item?

CHAIRMAN FROINES: Let me summarize, if I can. The issue that drove this discussion was, in fact,

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the Earlimart episode where the modelling seemed to indicate that the exposure levels may have ranged from 500 parts per billion to 1 part per million.

And but the agency's position is that there are significant or some uncertainties within the modelling. And one would be they would be hesitant to draw conclusions and set a REL and a NOEL based on those modelling data.

And then that, too, goes back to the basis of the data which was the human study and that the precise question is the lower dose level in that human study.

And so the panel is saying that they would like to see that study in order to look at the low-dose data in relationship to control and high dose to see if, in fact, there's any kind of relationship that needs to be taken seriously that would, in a sense, support the Earlimart conclusions that there is a lower NOEL -- LOEL than 800 parts per billion.

And so it seems to me that the panel is saying they would like to see that and, based on that analysis, then decide to stay with -- more or less stay with what we have or consider a change.

DR. RUBIN: So my question now -- what format,

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if you will, would you like to see that data? Would you like to actually see the study itself? Or would you like me to go through it and give you the results at the -- here I just said there was no statistical effect at the low dose.

You want to see what the low dose -- I mean I should have put those numbers in the -- and I

can see now that they should be in there. Is that what you want to see or --

CHAIRMAN FROINES: Well, I think the panel is also concerned with what came up earlier, which is that the trend test was significant and that the simple statistical significant test wasn't and so that the statistical approach -- that the panel had questions about that.

So I think, it seems to me that I think it's up to the panel what they want. But I would argue that the panel probably wants to see the data as well as see your interpretation.

PANEL MEMBER GLANTZ: Yeah. What I would suggest, as a practical matter, is that the -- what I'd like to see today is what you can get us in an hour. And then we can decide if that's enough or if we want to carry this over, you know.

But I would hope, based on my

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understanding of your description of what you have, I think, hopefully, there are a couple of pages out of this report or a few graphs that you could show us that would answer the questions.

And then we can we could decide either -- as John said, we either go with the things the way they are or we say, "This is compelling enough that we recommend a change." Or if it's not obvious, we to have would have to come -- bring it back to the next meeting.

But what I'd, again, propose is that we either go on -- that we go on to any other issues about this report and then start to work on something else to give you a chance to get that or just stop the discussion now and give you a chance to go get it.

CHAIRMAN FROINES: Okay. What's -- unless there's other comments, my question to you is what's practical from your standpoint?

DR. RUBIN: I'm not confident that we can do that because I know that study. It's page after page of data -- various different concentrations all in the back. It would mean that somebody up there in Sacramento -- probably Keith Pfeifer -- would have to go to my desk; pick up the volume; figure out which

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volume it is, first of all; go back there and figure out what it is that Andy's asking him for.

And I'm not confident that can be done -- it might take him an hour just to -- or two hours, even -- just to figure out what it is that they want.

PANEL MEMBER FUCALORO: So what's the alternative approach?

CHAIRMAN FROINES: Well, I would prefer -- I would prefer, since this thing has been going on for such a long period of time, that we bring it to closure. And I think Stan feels the same.

DR. RUBIN: Yeah.
CHAIRMAN FROINES: My sense is that what I would suggest is that we proceed on the assumption that -- proceed on the assumption that we're going to stay with the values that we have, pending a review of the document and if, in between now and the next meeting, if it appears as though Paul's point is correct, then we will bring it up at the next meeting.

Otherwise, we will approve the document or approve our finding at this meeting, pending that review.

PANEL MEMBER FUCALORO: I agree.

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PANEL MEMBER ATKINSON: I'd like to comment that I would be very uncomfortable using the Earlimart study as anything to hang any quantitative data on, given the modelling.

CHAIRMAN FROINES: I don't think -- that's not being proposed. What's -- I mean I think that the Earlimart started us off. But at this point we're talking about the actual laboratory --

DR. RUBIN: Yeah.

CHAIRMAN FROINES: Paul, what's your --

PANEL MEMBER BLANC: What's that?

CHAIRMAN FROINES: Well, what's your -- I'm proposing an approach.

PANEL MEMBER GLANTZ: Are you happy with what he's proposing?

PANEL MEMBER BLANC: I think you answered my question.

CHAIRMAN FROINES: Gary?

PANEL MEMBER FRIEDMAN: That sounds good.

CHAIRMAN FROINES: Craig?

PANEL MEMBER BYUS: (No audible response.)

PANEL MEMBER FUCALORO: What day is it?

PANEL MEMBER BYUS: I -- yeah. I mean the train load and the dumping in the river is not what I'd call "real world," by any stretch of the

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imagination.

DR. RUBIN: This is Earlimart. This was an agricultural application.

PANEL MEMBER BYUS: And just reading this over, it would be nice to see this data presented.

DR. RUBIN: The eye irritation data?

PANEL MEMBER BYUS: I realize there's time limitations but --

CHAIRMAN FROINES: Let's proceed on the assumption that we will review the -- you will review and we will review this study and if there are -- if there are differences of opinion about its showing low dose -- lower-dose effects, we'll take it up at the next meeting, but otherwise we'll continue on pace today.

DR. RUBIN: Okay.

CHAIRMAN FROINES: And so I'll go back to Paul

because we're -- in essence now, we're in the stage of the panel discussion on this issue.

(Brief interruption.)

(Off-the-record discussion.)

CHAIRMAN FROINES: Elinor --

We haven't asked Elinor for her -- any views --

PANEL MEMBER GLANTZ: Let him show his last

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slide.

CHAIRMAN FROINES: What?

PANEL MEMBER GLANTZ: Let him show his last slide.

DR. RUBIN: One last conclusion slide. Back to the seasonal, the subchronic, the REL for the seasonal effects was set at 1 ppb, in other words, the NOEL of a hundred ppb just divided by an uncertainty factor of a hundred.

Ambient MOEs. These are margins of exposure, range between 28 and 166,667 with 3 of 14 studies falling below the -- quote -- "health-protective benchmark of 103 additional studies falling right around a hundred." This is a concern for us for seasonal exposure.

Application site MOEs ranged between 1 and 50, in other words, not 1 application site monitoring was not a concern. In other words, they were all a concern, all under a hundred.

As I understand it, toxic air contaminant listing occurs when MOEs are below a thousand. I recommend that MITC be listed as a toxic air contaminant both on acute and subchronic -- on the base of acute and subchronic analysis.

CHAIRMAN FROINES: Okay. Thank you very much.

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PANEL MEMBER BLANC: That's all -- I'm not done yet. I have some other questions that aren't quite as substantive --

DR. RUBIN: Okay.

PANEL MEMBER BLANC: -- just need clarification. In terms of the immuno -- potential immunotoxicity of metam sodium in its breakdown products --

DR. RUBIN: Yeah.

PANEL MEMBER BLANC: -- there was a recent article in the "Journal of Toxicology and Environmental Health," which is a review article, not a primary data article and, in fact, refers to some extent to California data, Health Department data.

But one of the things that I was -- that's particularly relevant from the article is that the two authors -- their particular area of interest is immunotoxicology --

DR. RUBIN: Yeah. Pruett --

PANEL MEMBER BLANC: Pruett and Keil and some of their relevant citations, particularly Pruett, aren't cited in the health effects document. The

Keil article is, but the Pruett articles aren't.
And I just want to ask you to go back through, just to double-check. I just want to make
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sure we didn't --
DR. RUBIN: I think the reason for that is that Pruett -- I think he's the head of that lab. I think it's at Louisiana state.
PANEL MEMBER BLANC: Uh-huh.
DR. RUBIN: And he's not the first author. So there's Keil -- I mean I discuss Pruett's data in this assessment. Generally he exposed animals at much higher levels to get immunologic effects. I can't remember what his other --
PANEL MEMBER BLANC: Well, the reason why I bring it up is because, although his exposures are -- they're oral studies and they're to metam sodium -- in the discussion in this review article, since they're discussing their own data, they emphasize that this is -- that these are probably important MITC effects.
And they specifically say it wouldn't matter whether it was by oral exposure or by inhalation. You would probably see the same effects. So it's a -- if it were just a review article, I would -- it would be, perhaps, less relevant.
But since it's a review article where they're reviewing a lot of their own data, it carries a little bit more weight, I think.
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DR. RUBIN: Yes.
PANEL MEMBER BLANC: And Pruett is the first author on at least one of the articles that they cite heavily in the review.
DR. RUBIN: Yeah. That's actually in the metam sodium risk assessment. In other words, I've got an entirely separate risk assessment. We actually submitted a version of this a couple of years ago to the panel. But it's evolved quite a bit from there.
PANEL MEMBER BLANC: I mean maybe it could be --
CHAIRMAN FROINES: Wait. I'm -- what are you talking about? A metam sodium risk assessment?
DR. RUBIN: Yeah. We have developed two separate risk assessments, one on MITC and one on the parent compound metam sodium. MITC is the one that's being considered in front of the panel, obviously.
But there is -- and I actually submitted this to the panel a couple of times ago -- the document as it existed then. The toxicology of metam sodium is notably different than that of MITC. But I haven't brought it to the panel because it's not --
PANEL MEMBER BLANC: But the authors here, you
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know, make the explicit point that, in terms of the

immunologic effects they're talking about, it's probably contributed equally by the parent compound and MITC.

DR. RUBIN: Yeah.

PANEL MEMBER BLANC: So therefore it becomes relevant for MITC by virtue of what they're saying here --

DR. RUBIN: Right.

PANEL MEMBER BLANC: -- and then citing their own work.

DR. RUBIN: Yeah.

PANEL MEMBER BLANC: So I think it needs to be addressed -- it should be addressed explicitly, maybe in 2 sentences. I don't know. It may be very brief. But I'm just a little bit uncomfortable with --

DR. RUBIN: I could bring over another, you know -- whatever references you're talking about. I think they're in the metam document. But I do have at least the one reference here that was, in my opinion, was --

PANEL MEMBER BLANC: More relevant.

DR. RUBIN: -- more relevant --

PANEL MEMBER BLANC: The Keil -- the Keil references.

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DR. RUBIN: -- more relevant to MITC. Yeah exactly.

PANEL MEMBER BLANC: So that that's -- I don't think it's a major issue. It's a -- but I'm always nervous when I, you know, if I see someone making a big deal over particular, you know, body of work that, then, isn't appearing in our document. So that's why I bring it up.

And the other point is very, very minor but may have other applicability in other reviews that you do. And that is you do cite the Kreutzer reference that was on your --

DR. RUBIN: Yeah.

PANEL MEMBER BLANC: -- summary -- the 1994 --

DR. RUBIN: Yeah.

PANEL MEMBER BLANC: -- reference which is, I assume, an article within a monograph?

DR. RUBIN: Yes.

PANEL MEMBER BLANC: And there was a later publication by Kreutzer which was in a peer review journal with some of the same co-authors but other co-authors -- the community-based epidemiologic study of health effects from a metam sodium spill on California's Sacramento river from 1996.

DR. RUBIN: Who's the first author?

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PANEL MEMBER BLANC: Kreutzer. And --

DR. RUBIN: That, I'm not -- I'm not aware of.

PANEL MEMBER BLANC: Yeah. And I would just say that, in general -- and I'll provide you with that reference. But I would say, in general --

DR. RUBIN: Yeah. Usually peer review --

PANEL MEMBER BLANC: It's probably overlapping reports. But I think, in general, a citation from the peer review literature is preferable to a citation from a monograph, if, let's say, that it's equivalent data information.
DR. RUBIN: Okay.
PANEL MEMBER BLANC: I'll give you this, if you don't -- do you have this, this review article by Pruett?
DR. RUBIN: Yes, I do. In fact, he cites us in there, I believe.
PANEL MEMBER BLANC: Yeah, he does.
DR. RUBIN: I've got -- you know, I've got all the papers, all those papers.
PANEL MEMBER BLANC: Well, it's pretty recent. That's why I asked.
DR. RUBIN: Yeah.
PANEL MEMBER BLANC: I'm done.
CHAIRMAN FROINES: Gary?

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PANEL MEMBER FRIEDMAN: (No audible response.)
CHAIRMAN FROINES: Craig?
PANEL MEMBER BYUS: Nothing.
CHAIRMAN FROINES: Stan?
PANEL MEMBER GLANTZ: I'm okay.
PANEL MEMBER ATKINSON: I have 2 or 3 minor comments, which I'll -- they're misstatements, but they're very minor.
CHAIRMAN FROINES: Okay.
DR. RUBIN: That you'll give me in writing or --
PANEL MEMBER ATKINSON: I'll give you them.
DR. RUBIN: Oh, okay.
CHAIRMAN FROINES: Does everybody have the proposed findings?
Elinor, do you want to take Andy's place?
DR. FANNING: Sure.
PANEL MEMBER BLANC: I'd like to have a 10-minute break.
CHAIRMAN FROINES: I think we'll take a break right now to give the reporter a chance to --
(Break.)
CHAIRMAN FROINES: We are now going to take up the panel's findings.

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Elinor, do you want to comment? Would you comment on -- the panel has two copies of the findings, one of which is -- shows the changes that occurred this week, and one is a clean copy of that.
And then we have made some additional changes, and Elinor's going to tell you what that is -- what those are.
DR. FANNING: Okay. The version that was sent to the panel, which is the strikeout version in front of you -- these findings have actually been significantly modified from the version that you

approved in May of 2000. This -- yeah.
PANEL MEMBER BYUS: It's fine.
DR. FANNING: Amazing that it's been so long. So a number -- because of the nine or so items that you see from Andy that have changed in the document, we've got gone back and tried to make your findings reflect those.
There were some additional kind of cleanup points that came out of the May, 2000, meeting, where you had given -- panel members had recommended changes at that time. Those changes have all been made, and the changes from that meeting do not show in strikeout.
So, just to simplify things, the only

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changes that show on strikeout are the ones that pertain to the new document version. Okay? Now --
CHAIRMAN FROINES: Is that clear?
DR. FANNING: Yeah. Did I confuse you? Or did that make sense? Part and part. Okay.
I think also, as I was listening to your discussion this morning, it's quite clear that we're going to need to make some further modifications to this document.
First of all, one of the main areas would be that this findings document version does not reflect the benchmark dose modelling that has been -- that was discussed this morning 'cause we didn't have that yet.
So the addendum on benchmark dose and the comments that were made by panel members on the trend, the statistical trend in the Klimisch data -- those are not reflected in Finding Number 36. So we may wish, when we get to 36, to discuss the language there in detail.
I think the second major area coming from this morning's talks for this document's going to need further work pertains to Finding Number 21 on the acute effects of MITC. You may wish to add language there and also to -- there are some

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conclusions that we make in Findings 46 and 47 about the mixed exposure that results from metam sodium application.
Those are just some that I can see right away that we will probably want to change as we go through this document.
How do you want to proceed? Should we go through and look at each of the findings that has changed since your last version?
CHAIRMAN FROINES: No. I think that -- I think we have to assume that the panel's read the document and will take comments --
DR. FANNING: Okay.
CHAIRMAN FROINES: -- from the panel rather than spending an hour to walk them through it, unless somebody wants to walk through it. Gary?

PANEL MEMBER FRIEDMAN: No. I have comments. I don't want to walk through it.
CHAIRMAN FROINES: So continuing, Paul, you're the first.
PANEL MEMBER BLANC: First?
CHAIRMAN FROINES: You're the first person that I come to, to my left.
PANEL MEMBER BLANC: Oh, okay. Going in a --
CHAIRMAN FROINES: Clockwise.

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PANEL MEMBER BLANC: -- clockwise direction, shall we say.

Question for you: One of the things that struck me where you talk about dates of use, in terms of usage, is with the revisions of the documents coming from DPR, is 1998's the most recent year, then, for which there's usage data that they cite?

DR. FANNING: I know more recent data exists. I believe the current document cites up to '98. And I think it used to cite to '97. So there was -- there was a change on that. And I didn't put it in strikeout 'cause I considered it minor. I will verify that.

PANEL MEMBER BLANC: I mean I don't think our findings should be ahead of what's in the document. So if the document doesn't go beyond '98, I don't think we should. I think I would like to be reassured that there hasn't been a fivefold increase since 1998.

DR. FANNING: Right.

PANEL MEMBER BLANC: I mean is the difference between 1998 and 1999 and 2000 -- is it sort of --

DR. FANNING: I think, in fact, it may have decreased. I think --

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We're talking about the pesticide-use data.

And I think -- I did -- I actually looked it up and went to the pesticide-use reports. Yeah. The current document cites up to 1998. And I did go look at the '99 data. And I'm afraid I didn't write it down. But this reflects accurately what's in the document.

PANEL MEMBER BLANC: Right. I don't -- you know, again, I don't think we can cite things that aren't there. But I'd --

DR. FANNING: Yeah. That's what I thought.

PANEL MEMBER BLANC: What's that?

DR. FANNING: Use seems to have a slight declining trend.

PANEL MEMBER BLANC: Okay. So it's not a huge increase in the --

DR. FANNING: Right.

PANEL MEMBER BLANC: Okay. That's fine. Should I just go in order, John? Is that what you want me to do?

CHAIRMAN FROINES: Uh-huh.
PANEL MEMBER BLANC: These are, you know -- mostly these are minor points unless it's something that I've already brought up in the other discussion.

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On Point Number 5, where the last line, where it says, "In practice, degradation to MITC, MIC, and hydrogen sulfide is favored" -- the implication there is not that there's degradation to MIC that doesn't go through MITC, is it?
DR. FANNING: No. What the sentence -- that sentence was actually modified by panel feedback at the May, 2000, meeting. "Favored" means -- there, means "soil conditions and pH" and so on, so that it's meant to say that the pathway producing MITC, MIC, and H2S is somewhat dominant over that pathway that produces --
PANEL MEMBER BLANC: I didn't have a problem --
DR. FANNING: -- carbon disulfide and methyl --
PANEL MEMBER BLANC: -- with the "favored." I had a just -- the only part about that that's confusing is I think that, if the MIC were in parentheses and if it said, "In practice, degradation to MITC -- parentheses -- and then to MIC -- end of parentheses -- and hydrogen sulfide is favored" -- because the metabolism or the breakdown to hydrogen sulfide is independent of the MITC but the MIC only comes from the MITC; correct?

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DR. FANNING: Yes.
PANEL MEMBER BLANC: And so I wasn't clear if you were applying something --
CHAIRMAN FROINES: We'll fix it.
DR. FANNING: Yeah.
PANEL MEMBER BLANC: Next point. Number -- newly Number 6, the very last phrase -- "metam sodium is the dominant source of MITC and MIC in California air." Yeah. It's the dominant agricultural source.
DR. FANNING: Okay.
PANEL MEMBER BLANC: And I think it's really the dominant source altogether, but the issue there is that it's the dominant agricultural source. Yes?
DR. FANNING: Okay.
PANEL MEMBER BLANC: Because theoretically somebody could be releasing MIC if they have a big, you know, chemical manufacturing --
CHAIRMAN FROINES: Well, there is a pesticide plant in Southern California. I don't know if they make MITC.
PANEL MEMBER BLANC: I don't think this is -- again, none of these are going to be make major things, but I'm just going to go through it if that's what you want. Number 17.
CHAIRMAN FROINES: We don't want to ever

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repeat the lead Stan Glantz event which took a whole day of -- but I think it's probably --

PANEL MEMBER GLANTZ: Hey, that was important.

PANEL MEMBER BYUS: That was good.

PANEL MEMBER BLANC: This is new wording that you have on Point Number 17. The whole point may be new, for all I know. The next-to-last sentence -- "Air-dispersion modelling by DPR estimated that air concentrations of MITC were mostly between .5 and 1 part per million."

"Mostly"? What does "mostly" mean to you in there?

DR. FANNING: Well, I did take that language from the summary of Earlimart incident that was prepared for the document, for the new version. And I think "mostly" is actually their word.

I believe -- and DPR staff can correct me if I'm wrong -- that this reflects different modelling scenarios that were used, different distances from the field, different times so that there was an estimate of 3 ppm exposure very near the field but the majority of people evaluated would have experienced, by their model, the concentrations between 500 ppb and 1 ppm.

"Mostly," maybe, is not the best word.

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PANEL MEMBER BLANC: I mean should the word "mostly" be dropped? I'm assuming that the models didn't suggest that the range was really, you know, that, for 75 percent of the people, the range was .5 to 1 but that a lot of -- but actually the range was even broader than that.

And that would, coming back to our earlier discussion, would be rather unnerving. So I'm assuming that the word "mostly" just should be dropped.

PANEL MEMBER ATKINSON: You could replace it by "to which people were exposed."

DR. FANNING: It looks -- the text from the document says that "MITC concentrations in the populated area of Zone A" -- so within a certain distance; and, by the way, 80 percent of those exposed were exposed in Zone A -- "Zone A concentrations were estimated to fall, for the most part, between .5 and 1 ppm." Okay?

So then there are other zones, other neighborhoods that were more distant. And then there was the near field edge. So I don't know what the broader range -- say, for Zone A, for that neighborhood -- would be.

PANEL MEMBER BLANC: John, do you understand

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my confusion here?

DR. FANNING: Yeah.

PANEL MEMBER BLANC: Andy, can you help us with this?

DR. RUBIN: Yeah, I know. I'm not sure I can

help you, but DPR generated a map, a plume-dispersion map.

PANEL MEMBER BLANC: Right.

DR. RUBIN: So you have the neighborhoods. You have the fields. And then you have the neighborhoods of varying distances from the fields.

PANEL MEMBER BLANC: Yeah.

DR. RUBIN: And you get -- it sort of looks like weather map, in a way. You get a, you know, like, a bubble --

PANEL MEMBER BLANC: Right.

DR. RUBIN: -- that says, "In this area, based on these wind conditions and so forth that we assume occurred, the modelling concentrations are .1 --.5 to 1 ppm."

But there may have been areas of Zone A that weren't covered by that bubble. That's probably why I used that language -- that they may actually have been exposed to lower concentrations.

PANEL MEMBER BLANC: But the Point 10 in your

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summary --

DR. RUBIN: Point 10?

PANEL MEMBER BLANC: Oh, he has -- I'm sorry.

"Following an incident of agricultural drift over populated areas, residents of Earlimart, California, were exposed to levels of MITC estimated to be in the range of .5 to 1 part per million. Odor complaints were received" -- blah, blah, blah.

"Of 171 exposed individuals, nearly 80 percent experienced symptoms of eye or upper respiratory irritation."

The 171 exposed individuals -- were they all in Zone A, then?

DR. RUBIN: I have the numbers here.

DR. FANNING: No. 136 --

DR. RUBIN: 136.

DR. FANNING: -- in Zone A.

PANEL MEMBER BLANC: And, then, where were the other people from?

DR. RUBIN: Further out. Zone B, C, D.

PANEL MEMBER BLANC: And --

DR. RUBIN: In other words, enough -- I'm interrupting you. Sorry.

PANEL MEMBER BLANC: No. So what's the denominator, then? The 171 is people who had any

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complaints at all? Or were they just people who were surveyed?

DR. RUBIN: These are people who were surveyed.

PANEL MEMBER BLANC: Regardless of whether they had complaints or not? They weren't --

DR. RUBIN: Right. Right.

PANEL MEMBER BLANC: Okay. So there were -- so as they got farther out of Zone A into B and C, there was still an incidence of respiratory and

mucous membrane irritation. And in those areas, the modelling was that the exposure level was lower than .5 to 1?

DR. FANNING: No concentrations are given in this document for those other zones, the other neighborhoods.

CHAIRMAN FROINES: But were there -- the obvious -- this is Pandora's box being opened up because what it seems -- what you seem to be saying is that there were affected people in other zones where the concentrations -- where you were further away and therefore the concentrations would be expected to be lower. Is that the case? Because if that's the case, then we have a problem.

PANEL MEMBER BLANC: Melanie, maybe you could

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help because I'm reading from your memo.

DR. MARTY: We had the same problem. We don't have concentrations. We have -- we have the exact same question that John has.

CHAIRMAN FROINES: So what did we do? We just forgot those people who were symptomatic in those areas? What's going on?

DR. RUBIN: I'll have to get -- I'll have to check back on that data. I'm --

CHAIRMAN FROINES: You realize that this is a fundamental issue because you're suggesting that there may be people exposed to lower concentrations who were symptomatic. And so if you thought that the .5-part-million question was an issue, wait till we get to this one.

Am I correct, Paul?

PANEL MEMBER BLANC: Yeah. Yeah.

DR. FANNING: Yeah.

CHAIRMAN FROINES: But it's their data. We have to use their data. We can't say, "Yes. these models tend to be incorrect." We don't know that. But if they put it forward, then they have to live by it.

DR. FANNING: Yeah. The way -- the only, you know -- since I just had this document to go on, the

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way that I -- the language I suggested for your finding has a sentence that says, "Exposure levels are unknown," and then goes on to describe what I could glean of the modelling results here.

But it does seem that you may need to see the complete modelling results to complete your finding on this.

PANEL MEMBER BLANC: Well, Andy, can we go back? I have three versions, then, of summaries of the data. One is Elinor's various modifications of draft findings. Another is the Health Department's response to the DPR.

DR. RUBIN: OEHHA's.

PANEL MEMBER BLANC: OEHHA's -- I'm sorry -- response to DPR. And the other is DPR's draft. And

somehow what we did this morning was, Andy, we heard your presentation on your document, which responded in part to DPR's input or the memo that we were sent that Melanie was a co-author of.

But we actually haven't heard from OEHHA in terms of their outstanding questions. And so maybe we should take a step back and just give a OEHHA a chance to speak. Maybe they have nothing to say.

But this -- for example, I would have

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thought OEHHA would have been more proactive if they had the same question about the Earlimart data. I hope there aren't other areas that OEHHA's anxious about but not voicing their concerns.

DR. MARTY: Melanie Marty from OEHHA. There are little differences that we had in terms of the approach for the benchmark concentration.

PANEL MEMBER BLANC: Right. That was addressed.

DR. MARTY: Yeah. And that was already addressed. And this particular issue -- this is the first time, actually, that I have -- that this has been brought to my attention -- this issue of "Were all those 171 people within the modelled isopleth from the air-dispersion modelling?"

And I don't know the answer to that. The pesticide group, represented by Michael DiBartolomeis here, also doesn't know the answer to that, as he just indicated to me. So I think it -- we need to figure out where these people were; what was, you know, what was modelled.

At the same time, I have some heartburn using modelled concentrations to indicate personal exposure of these individuals because, within those isopleths, the concentration could, you

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know, be really squirrelly and some people may have gotten considerably more than the range of .5 to 1 and some people considerably less.

So it's really hard to go back and say, "This guy was right here, and therefore his concentration was X." so I have a little bit of heartburn over doing that.

CHAIRMAN FROINES: I think that's fair. And I think the panel would agree with you. But I think it's also true that that --

DR. MARTY: Okay. On Page 25 of the document.

PANEL MEMBER BLANC: Which section?

DR. MARTY: Oh, let's see. On Page 25 of the document, is it --

DR. FANNING: It's Part C.

DR. MARTY: Part C.

DR. FANNING: Page 25 at the top.

DR. MARTY: Page 25, describing the Earlimart incident. Yeah. That's what you guys just looked at that. We don't have what the concentrations were for

these other zones. So that's what -- that's what's missing.
And also my guess is -- and I haven't seen this -- but that the zones -- they're not going to correspond neatly to an isopleth map of
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concentrations.
DR. RUBIN: No. That's the point I just made.
DR. MARTY: I don't know if ARB has any comment on the modelling either. OEHHA -- we don't -- we're not modelers. So I'm sorry. I can't help.
CHAIRMAN FROINES: I don't think anybody is suggesting that we define the REL or the NOEL based on the modelling. But as a matter of public health conservatism, if you find sick people further away, that's something that should be addressed. It can't be just ignored. That's the problem.
DR. MARTY: Yeah.
PANEL MEMBER ATKINSON: On Page 24, it states that the 0.5-to-1-ppm-concentration range extended into Zone B and possibly into C. So it's not just those --
DR. RUBIN: Yeah. That's true.
CHAIRMAN FROINES: Well, look --
PANEL MEMBER BLANC: -- but not into.
CHAIRMAN FROINES: The role of this panel is to function as a kind of quality-assurance, quality-control effort on the science. All we're trying to do is to get clarity on the science. That's what we're trying to do. We're not trying to
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make science here.
But we're certainly trying to understand what you are talking about. And so we need to know the answer to these kinds of questions. And you need to know the answers to your questions. What modelling -- when you do the modelling, what were the concentrations in the area that's you found people with symptoms? It's a very simple question.
PANEL MEMBER BLANC: Well, or another thing that might make the data more interpretable or would lend it to health effects interpretation would be, instead of having solely incidence data presented by Zones A, B, C, and D, which are geographically defined, why not give us incidence data by the isopleth model of concentrations? We need that --
DR. MARTY: Overlaying where the individuals were --
PANEL MEMBER BLANC: Yes.
DR. MARTY: -- with the map that was generated by the dispersion modelling.
CHAIRMAN FROINES: Michael, do you have a comment?
DR. DIBARTOLOMEIS: No. I'm standing here just in case.
DR. MARTY: That presupposes you know where

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the individuals were. And, you know, I don't know if you can get that from -- I don't know how the survey was done --

PANEL MEMBER BLANC: Yeah. But you see the unfortunate thing about the way the data are presented in the document is that it is very easy to, especially in the summarized part of it -- but it's very easy to misunderstand it.

What is being said is not what's being said. What is being said was my original understanding that if, in a population which was -- which had a modelled exposure of .5 to 1, with all the limitations of that modelling, here's what the incidence was.

But that turns out not to be the case. There was a certain incidence within the modelled exposure -- .5 to 1. And then there was an incidence at a lower modelled exposure concentration, although I don't know what that exposure modelling was and I don't know what the incidence was. But it wasn't zero in either -- in either axis.

Shall I go on, then, or --

DR. FANNING: I'm sorry. Could I just get a little bit of summary on that point, then? Do you want to see the document changed with more

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concentrations? Or do you want to take the data as presented in the document and create a finding for the panel that is accurate given what is there?

In other words, we could say, "In people whose exposure was estimated to be between .5 and 1 ppm, the incidence rates were as follows:"

PANEL MEMBER BLANC: We can't say that, based on the data that's provided. What we could say is that "There was a high incidence of symptoms and the highest incidence of symptoms appear to be consistent with an isopleth of .5 to 1. There clearly were people symptomatic at lower levels of exposure, based on available data."

We cannot say what the modelling was and what the incidence was or -- I mean if -- that's if you want to have findings based on what we have currently.

If there was other data presented, I want to make other findings. But I think the finding would have to be that "We cannot determine from the data available what the modelling would suggest was the lower -- was the lower -- lowest effect level."

CHAIRMAN FROINES: I think -- so that I think that we've just added another item for the interim period, which is to get some greater clarity to what

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was indeed found. And then we can write a finding that will be consistent with that, if that's okay.

DR. FANNING: Okay.

CHAIRMAN FROINES: Is that okay, panel? And I

think -- and Roger has been very articulate about the uncertainty in this modelling. And any finding should reflect that, I think.

PANEL MEMBER ATKINSON: Okay.

PANEL MEMBER BLANC: I also think that, whatever else is said, I think that the final sentence which says -- of Point 17, which says, "Exposure to other breakdown products of metam sodium was neither measured nor modelled" -- the point is that it wasn't modelled.

Metam sodium wasn't measured either, nor was MITC. It implies that something was actually measured for anything.

DR. FANNING: Okay.

PANEL MEMBER BLANC: So it's just that it wasn't modelled --

DR. FANNING: Sure.

PANEL MEMBER BLANC: -- I mean. So those were my comments.

CHAIRMAN FROINES: Gary?

PANEL MEMBER FRIEDMAN: I have a more general

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comment. And I'm not suggesting that major work should be done on this, but I have never seen our findings in such great detail. It seems to me we have a couple of pages with the major points, the major data, and our conclusions that it's a toxic air contaminant or here it should be listed.

And I'm just wondering whether -- how -- we're sort of regurgitating the whole report. And I'm not sure that that's our function or that that's -- we're really doing a service by doing that.

And I would suggest that, in the future, we try to narrow down the thing to the major conclusion as we've done in all previous examples that I can remember.

PANEL MEMBER GLANTZ: Yeah. I agree totally.

DR. FANNING: Yeah.

PANEL MEMBER FUCALORO: And I can give you my comment now because it follows up what you said. I'm just curious. Findings 18 through 20 regarding exposure of experimental animals to metam sodium in drinking water -- I'm not sure how that relates to anything we should be doing. And so I -- my comments are yours. So we can kill two birds with one stone.

PANEL MEMBER GLANTZ: Three birds.

CHAIRMAN FROINES: Elinor and I talked at

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great length about this issue because I had the same problems. And we took out -- and you haven't seen what we've taken out yet.

PANEL MEMBER GLANTZ: Good.

DR. FANNING: Well --

CHAIRMAN FROINES: There are two paragraphs -- there are two paragraphs we've taken out. So we -- as you've noticed, if you've -- that what we did was to shorten the document that was originally drafted.

The origin of this, of course, is what everybody's forgotten is that this encyclopedia began with diesel. And we've been writing very long documents ever since.

And this is in a good point to say, "Halt. We don't want to do that." I think what we should do, in fact, is to define what are the important scientific issues. And that's what our findings should reflect.

PANEL MEMBER FUCALORO: In other words, what we base -- what anyone can base their recommendation to designate something a TAC, I mean, based upon, you know, the exposure level -- well, the exposure level and the toxicity -- I mean the major toxicity findings.

CHAIRMAN FROINES: Well, we will certainly --

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we've put a lot of time into this already, as you might expect. And so what I'd like is that, if people will feel that there are sections that should be shortened, we'll be happy to do that. I'm not going to go back and redo it myself. It has to come from them.

PANEL MEMBER FUCALORO: No. I think Gary's idea was that it would be in the future.

PANEL MEMBER GLANTZ: He's saying it's in the future.

PANEL MEMBER FRIEDMAN: Given the time that's elapsed with this and how much work has gone into it, I don't think we should rewrite it from scratch. But I just question this format for our report.

CHAIRMAN FROINES: Yes. I think that's what we're saying. So we should not go redo this, but in the future it should be -- the number of findings should be less than a substantial --

PANEL MEMBER FRIEDMAN: This is actually longer than the executive summary of the original report.

DR. FANNING: Yes. I'll say that it was a great temptation, in taking this up again after a year, to begin over again and make a very simple, streamlined document that highlighted the important

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issues. It did seem, though, perhaps more expedient to just build on what you had already approved at this point and just try to make the relevant changes, move on from here.

And the reason this document was so complex in the beginning had to do with the -- there were a lot of issues that came up around this particular chemical. And there was some feeling a couple, two years back, that some of the detail might be important.

PANEL MEMBER BLANC: I think that the points, your points are very well taken, both in terms of process and in terms of the history of it.

DR. FANNING: Yeah.

PANEL MEMBER BLANC: And I think that one of the questions I would have for the chair is whether or not you wish to enter into an explicit discussion of the panel's consensus that the document and our finding about the document -- it is important that it encompass metam sodium and its breakdown products.

And I think we effectively do that by having the level of detail that we have here. And that's one of the reasons why it is as detailed as it is. But on the other hand, there was no point where we sort of take the bull by the horns except in Point 53 where --

CHAIRMAN FROINES: Where?

PANEL MEMBER BLANC: The very next -- the very last point is really getting at that. And I guess my question -- should that be even more explicit than it is? Or is it good enough as is?

CHAIRMAN FROINES: Well, I -- from my standpoint, it seemed -- that seemed to be a reasonable statement. I think that, to enlarge upon it, it's going to, again, create more discussion and greater time will be lost or gained, however you want to look at it.

But the -- I would like to leave it pretty much as it is unless there's a consensus which can --

PANEL MEMBER GLANTZ: And I don't think Gary is proposing that you not leave this one as it is. I think he's talking about the next one.

CHAIRMAN FROINES: No. No. No. Paul's asking a specific question. Paul's asking -- look at 53. Paul's asking a very precise question.

PANEL MEMBER GLANTZ: Which is?

CHAIRMAN FROINES: Paul's asking is the -- in 53, there's a sentence that says, "In addition, because MITC air levels derive overwhelmingly from applications of metam sodium with a smaller part contributed by dazomet, we recommend that these two pesticides also be listed, along with MIC, as toxic air contaminants."

Paul's asking the question "Do we want to -- do we want to discuss that issue further?"

And I'm saying I thought that was sufficient.

PANEL MEMBER FUCALORO: No. I thought -- I thought we had hammered that out and we had decided, once you apply metam sodium, you're getting MITC that attaches --

PANEL MEMBER BLANC: I didn't mean to discuss it. I meant is that explicit enough? Is that enough text? Not that we need to discuss it, but does this text --

PANEL MEMBER GLANTZ: I think it's pretty clear.

PANEL MEMBER BLANC: Okay. And as a sort of a

part of that, the very -- the last part of which was that MIC is automatically -- which is attached due its status as a hazardous air pollutant -- where does that leave hydrogen sulfide and carbon disulfide? Are they already listed elsewhere?
PANEL MEMBER FUCALORO: I don't know.

CHAIRMAN FROINES: They're HAPs.
PANEL MEMBER FUCALORO: I would guess that they would be.
CHAIRMAN FROINES: So they probably should be included in there.
PANEL MEMBER BLANC: Shouldn't there be a sentence there that says --
DR. FANNING: I know carbon disulfide is, but hydrogen --
DR. MARTY: I'm pretty sure that hydrogen sulfide is not a HAP. I'm pretty sure that George Bush, Sr., blue-pencilled it.
CHAIRMAN FROINES: Who?
DR. MARTY: George Bush, Sr., had it removed.
CHAIRMAN FROINES: Really?
PANEL MEMBER FUCALORO: Why?
DR. MARTY: I'm almost a hundred percent certain of that. We've got the list. It's not on there; correct?
CHAIRMAN FROINES: It's not on there?
UNIDENTIFIED AUDIENCE MEMBER: He took it off.
CHAIRMAN FROINES: Well, that's a very significant --
PANEL MEMBER FUCALORO: It's interesting.
CHAIRMAN FROINES: -- issue especially because

H2S is a primary breakdown product and one could argue that, given that it's in -- in the chemical context, it's in the same category as MITC, that one should have dealt with both of them simultaneously.
PANEL MEMBER FUCALORO: Yeah. I can't --
CHAIRMAN FROINES: So we should have a sentence in there that says, "H2S is a product -- it is not a HAP -- and needs to be considered in the future."
PANEL MEMBER GLANTZ: Well, H2S being a breakdown product -- that's in Paragraph 5.
CHAIRMAN FROINES: But in the conclusion about what's going to be listed and what's not listed.
PANEL MEMBER BLANC: What about carbon disulfide? I'm sorry.
DR. FANNING: Carbon disulfide is listed via the federal list.
PANEL MEMBER BLANC: It is?
DR. FANNING: It's a HAP.
PANEL MEMBER BLANC: So that we should explicitly say that too in that paragraph.
DR. FANNING: Since we're on this, the new version of the document also has identified metam potassium as a new source of MITC.

PANEL MEMBER FUCALORO: Sure.

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DR. FANNING: Does the panel want to add metam potassium --

PANEL MEMBER BLANC: Yes.

DR. FANNING: -- as well? Right now, you have dazomet.

PANEL MEMBER BLANC: I would say the wording should be "For applications of metam sodium and metam potassium -- comma --" with dazomet --

DR. FANNING: And so we recommend that these --

CHAIRMAN FROINES: Where are you at?

DR. FANNING: -- these three pesticides, then --

PANEL MEMBER BLANC: Well, these three pesticides --

PANEL MEMBER FUCALORO: Let me ask a question, a hypothetical question. What if a manufacturer gets mischievous and decides to put -- I don't know -- metam lithium or something, changing the "cation" ion and still producing MITC? I guess he would still -- that manufacturer would still fall under this, wouldn't he?

PANEL MEMBER BYUS: It would help --

CHAIRMAN FROINES: Would he have any market?

PANEL MEMBER FUCALORO: He -- you're saying,

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if the wording is strong enough. I'm suggesting -- originally said, "Yes, I think it is."

But now I'm thinking that perhaps you might want to put some words which say, "All products -- all products which lead -- which primary goal, the consequence of application is to lead to MITC" or something like that, I think, might be what we wish.

CHAIRMAN FROINES: Okay.

PANEL MEMBER FUCALORO: I'm not --

CHAIRMAN FROINES: I don't think lithium is about to emerge on the market.

PANEL MEMBER FUCALORO: I know. I was trying to think of a cheap one. I was thinking of cesium, but that's very expensive.

PANEL MEMBER BLANC: Maybe the pesticide people could comment on this, but isn't there also a formulation which is metam disodium? Does such a thing exist? Because this came up in Poison Control Center case presentation where there were two CAS numbers for metam sodium. And we were confused, and somebody tracked it down. And one was technically the CAS number for metam disodium or something like that.

PANEL MEMBER GLANTZ: Aren't we getting kind

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of faraway from the report?

PANEL MEMBER BLANC: Yeah. Just asking. That's relevant to your question.

DR. FANNING: So maybe a little more on this -- do you want to say something to the effect that of, you know, "All pesticides which produce MITC as the active fumigant should be listed"? Or do you want to specifically identify metam sodium, metam potassium, and dazomet?
CHAIRMAN FROINES: Well, we want to -- I think there was a general agreement on including metam potassium. So we can go with that.
DR. FANNING: Okay.
CHAIRMAN FROINES: And then what Tony is saying is that we may want to add a sentence --
PANEL MEMBER FUCALORO: Right.
CHAIRMAN FROINES: -- that, if other pesticides are identified that break down to MITC, they should be included.
PANEL MEMBER BYUS: Right.
CHAIRMAN FROINES: I think -- is that what you -- I think --
PANEL MEMBER FUCALORO: Yeah. Yeah. That would be my suggestion.
DR. FANNING: Okay.

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PANEL MEMBER FUCALORO: But H2S is still something that we need to discuss, I would think.
CHAIRMAN FROINES: And I think that the other thing for the record is that there is a consensus on this panel that we need briefer, to-the-point findings that identify precise issues and address them. And I think that's Gary's recommendation, but I think everybody agreed with it.
DR. FANNING: Uh-huh.
CHAIRMAN FROINES: Craig?
PANEL MEMBER BYUS: Yeah. I just have a -- Number 36.
CHAIRMAN FROINES: Did you have any more comments, Gary?
PANEL MEMBER FRIEDMAN: No.
PANEL MEMBER BYUS: Number 36. I assume this is the Klimisch study we talked about this morning?
DR. FANNING: Yes.
PANEL MEMBER BYUS: Okay. Well, it says here, "A NOEL was not identified. In pairwise comparison to control animals, the increase in nasal atrophy reaches statistical significance only at the highest exposure of 34 parts per million. Nonetheless, the panel considers the finding of 1.7 parts per million toxicologically relevant."

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Maybe we should put in here about the chi square test for --
CHAIRMAN FROINES: We are.
PANEL MEMBER BYUS: -- trend. Okay.
CHAIRMAN FROINES: I think Paul's talked about that.
PANEL MEMBER BYUS: Did he say that in --
DR. FANNING: Paul didn't actually specify

that. I asked you that at the beginning, saying, "I suspect we need to modify 36 based on that."
PANEL MEMBER BYUS: It makes it sound like we're --
PANEL MEMBER FUCALORO: Arbitrary.
PANEL MEMBER BYUS: "Even though it's not statistically significant, we do think it's significant." I mean I just think, the way it reads, it's like we just decided to do it arbitrarily.
CHAIRMAN FROINES: We need to develop new language.
PANEL MEMBER GLANTZ: Yeah. I mean I think for that what I would -- this is a place where -- and a great example of words -- there's too much information --
PANEL MEMBER BYUS: Too much information. Yes.

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PANEL MEMBER GLANTZ: -- as my daughter says to me. I think you can just say, "A LOEL of 1.7 was identified from a 4-week inhalation study in rats, based on atrophy of the nasal" --
PANEL MEMBER BYUS: There you go.
PANEL MEMBER GLANTZ: -- "epithelium" --
PANEL MEMBER BYUS: And leave it at that.
PANEL MEMBER GLANTZ: -- period.
PANEL MEMBER BYUS: Leave it at that.
CHAIRMAN FROINES: Do you want to --
PANEL MEMBER GLANTZ: And then I'd like on that one to add in a sentence saying that "A benchmark dose approach to the same experiment yields similar results."
PANEL MEMBER BYUS: Very good.
PANEL MEMBER GLANTZ: You need to keep the last -- you need to keep the last sentence, then. "The subchronic NOEL of 100 parts per billion" --
CHAIRMAN FROINES: Do you want to include -- do you want to do what Elinor was suggesting, that a sentence -- sentences be added that talk about the dose-response statistics?
PANEL MEMBER BLANC: No. What he's suggesting is the opposite. From the -- after -- from the -- that you strike everything in there beginning with "A

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NOEL was not identified. In the pairwise comparison -- at the highest concentrations -- nonetheless" --
PANEL MEMBER GLANTZ: Well, I would even strike it before that. I would say, if you go up to the line before that, it says, "A LOAEL of 1.7 ppm MITC was identified from a 4-week inhalation study in rats, based on increased atrophy of the nasal epithelium" -- period.
And I would delete everything down to the last -- and then I would -- the whole rest of that item except for the last sentence.
CHAIRMAN FROINES: So --

PANEL MEMBER GLANTZ: And then I would add a sentence saying, "One reaches similar conclusions based on a benchmark dose analysis of the dose response -- of a dose-response relationship."
CHAIRMAN FROINES: So you don't want to add any discussion of the statistics associated with the --
PANEL MEMBER GLANTZ: No. They can go read the report for that.
CHAIRMAN FROINES: Well, it's not in there.
PANEL MEMBER GLANTZ: Well, it will be. Paul wrote that up. And he circulated -- or not "Paul" --

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Andy, I presume -- where's Andy? Is he still --
CHAIRMAN FROINES: No.
PANEL MEMBER GLANTZ: No. But the point is that Paul --
PANEL MEMBER BLANC: Well, Andy said he would modify it.
CHAIRMAN FROINES: Well, Paul Blanc gave the trend --
PANEL MEMBER GLANTZ: But that'll go in the report.
CHAIRMAN FROINES: -- the numbers from the trend test. And so Andy will then incorporate that.
PANEL MEMBER GLANTZ: Into the report.
CHAIRMAN FROINES: And we won't incorporate that into our document.
PANEL MEMBER BLANC: No.
CHAIRMAN FROINES: I mean Andy's making notes now. So it means it wasn't clear to him that he was going to do that.
PANEL MEMBER GLANTZ: Well, my understanding, just to be doubly clear, was that the changes that were circulated that talked about the power issues and the benchmark dose issues that were circulated to the panel a few days ago or a week ago -- that's going to be added to the report.

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CHAIRMAN FROINES: Right.
PANEL MEMBER GLANTZ: And then the material that Paul talked about will also be added to the report. And I think that deals with that. I don't think we need to --
CHAIRMAN FROINES: Is everybody in agreement with that?
PANEL MEMBER BYUS: Totally.
CHAIRMAN FROINES: Okay. Still with you.
PANEL MEMBER BYUS: I just had one other question on Number 42. You say, "A lesser factor" -- you're cutting out "A lesser factor might be appropriate for extrapolation of systemic effects observed in the subchronic study. However, a factor of 10 was retained to provide additional protection against the possibility that MITC may be onconic."
You just don't need the --
DR. FANNING: First of all, the endpoint on

which the current NOEL is based is not -- is no longer systemic effects.
PANEL MEMBER BYUS: Okay.
DR. FANNING: So that part wasn't appropriate.
PANEL MEMBER BYUS: Okay.
DR. FANNING: And the sort of -- the sort of -- the thinking behind that on so many factors

changed a little bit with the new basis of it. So my sense was that that was no longer necessary.
PANEL MEMBER BYUS: Okay. That's fine. I'm done.
CHAIRMAN FROINES: Tony.
PANEL MEMBER FUCALORO: Well, I had the same comment as Gary did. But I would just point -- in Number 44, to get out this hydrogen sulfide issue again, I still am somewhat surprised that hydrogen sulfide is not a toxic air contaminant.
And I'm wondering -- probably it's not the right time to talk about it -- I'm wondering whether or not we should at some point discuss this or maybe someone can explain to us why it's not.
DR. MARTY: It is a criteria air pollutant.
PANEL MEMBER FUCALORO: And that would -- yeah.
DR. MARTY: But it's an ambient air quality standard. It's based on odor and not health effects. So I think that needs to be noted.
PANEL MEMBER FUCALORO: I see. Okay. All right. Then, that's all.
CHAIRMAN FROINES: There's a national ambient air quality standard for hydrogen sulfide? What?
DR. MARTY: State.

DR. FANNING: State. It's a state value.
CHAIRMAN FROINES: Yeah. I'm glad to hear that because I thought I knew. And it's based on odor?
DR. MARTY: Yes.
PANEL MEMBER BLANC: Well, it has a pretty low odor threshold.
PANEL MEMBER FUCALORO: It does. I can smell it anywhere.
CHAIRMAN FROINES: But there's some interesting neurologic data about its effects at low dose. So it's an interesting compound.
PANEL MEMBER FUCALORO: Yeah. When the concentration gets too high, you can't smell -- you cannot smell it. And that's, of course, deadly. Then you really can't smell it. You can't smell anything.
CHAIRMAN FROINES: Tony?
PANEL MEMBER FUCALORO: I'm finished.
CHAIRMAN FROINES: You don't want us to put that statement in the record. So -- but are you finished?
PANEL MEMBER FUCALORO: I want everything I

said erased.
CHAIRMAN FROINES: Melanie?

DR. MARTY: I'd just like to remind the panel we do have a chronic reference exposure level for hydrogen sulfide because it's not on the Air Toxics Hot Spots list. So we have looked at that. And it's based on health effects, but it's pretty consistent with the odor threshold because it's really stinky stuff.
DR. FANNING: And you don't have an acute REL; is that correct?
DR. MARTY: I think we do. And it's based on the -- yeah. It's the -- yeah. We do have an acute REL for it also. But that's mostly based on the odor and the path of physiologic reaction to odor.
CHAIRMAN FROINES: Well, let's not get into it because that will start raising questions about the relevance of your RELs to the EPR process. So we'll leave that one aside for the moment.
Stan.
PANEL MEMBER GLANTZ: I'm -- I don't have anything else to say. I mean I could tinker with this, but I think we're done. I don't -- I don't have any other points. I would delete a bunch of stuff.
CHAIRMAN FROINES: You can delete it.
PANEL MEMBER GLANTZ: No. It's not worth it.

Well, can I delete something? I'll delete something. If you go to 41 -- and this is an example of too much information -- where it says, "Reference exposure levels for acute, seasonal, and chronic exposures developed by DPR in Table 1" -- I would delete the rest of the findings. But you don't have to.
DR. FANNING: So take out the --
PANEL MEMBER GLANTZ: Yeah. I would just say, "Here they are."
DR. FANNING: -- uncertainty factor --
PANEL MEMBER GLANTZ: That's in the report. People can go read the report. I mean there are several points. The only reason I'm not making a big deal out of it is I mean there's a whole bunch of things in here where you go through all these dividing by 10, divided by a hundred. I think that stuff's all in the report.
And so I would -- and I mean going through this, I think, in 42, 44, or is it 44 or 42?
CHAIRMAN FROINES: Well, I think it's reasonable --
PANEL MEMBER GLANTZ: But if you want to leave it --
CHAIRMAN FROINES: -- if the panel agrees that, if you want to submit deletions, that they will

basically trust you and we'll certainly make them.
PANEL MEMBER GLANTZ: Okay. I mean, just in

the -- going along with what Gary said, I mean I think that, the longer these things get, the more useful -- the less useful they are.

And I think going into all of these computational details -- that's all in the report. So I would take that out. And --

PANEL MEMBER FRIEDMAN: I'd support that. I'd be willing to sort of go through and cross out things if you're open to receiving that. I don't, you know --

CHAIRMAN FROINES: We could have violent disagreements, but we'll --

PANEL MEMBER FUCALORO: You'd need another meeting.

PANEL MEMBER GLANTZ: Yeah. That's the thing. I don't want to delay this anymore is the problem. Well, here, just looking at it, I mean I already gave you the one on 41. Let's see. 42 -- "Because toxicological data on chronic inhalation are lacking" -- "study in rats." Then in the next --

CHAIRMAN FROINES: Take out 42.

PANEL MEMBER GLANTZ: Huh?

CHAIRMAN FROINES: Take out 42.

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PANEL MEMBER GLANTZ: You could delete the whole thing, you think?

CHAIRMAN FROINES: Sure.

PANEL MEMBER GLANTZ: And, then 40 -- well, there is --

PANEL MEMBER BLANC: You know what I think? You're going down it too quickly.

PANEL MEMBER FUCALORO: I think he's right. I think this requires some deliberation. I mean you --

PANEL MEMBER GLANTZ: I think, rather than me doing it, I'd rather get the thing accepted. And we will become -- we will, like, have something that we don't have. But the point is you don't need, as I think Gary or somebody said -- this is longer than the executive summary.

CHAIRMAN FROINES: We all agree. We've talked about it.

PANEL MEMBER FUCALORO: No. No. Gary made his point.

CHAIRMAN FROINES: We've talked about it --

PANEL MEMBER GLANTZ: So don't delete --

CHAIRMAN FROINES: Elinor and I talked about it this week. We agreed. Everybody has said it. Let's not discuss every issue more than three times.

PANEL MEMBER BLANC: But, I do have a process

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question. It sounds like -- well, Roger, you make your comments first. Mine's more global.

PANEL MEMBER ATKINSON: I don't have any.

PANEL MEMBER BLANC: All right. Then here's my process question: It seems as if -- okay -- we have a sentence here, a word there. You've got notes of this. You're going to go back to the word

processor and make those changes.
And within, you know, three days, that could be circulated; and it will be, you know, on that -- at least on that level. And, you know, it will be one of those things where, unless somebody strongly disagrees, this is the minor modifications of the findings that we've approved.
But there's this big hole in the findings that we're discussing. And it has to do with what the acute REL is.
CHAIRMAN FROINES: We already agreed to approve these findings with these minor, minor changes.
PANEL MEMBER BLANC: Right.
CHAIRMAN FROINES: And then during the course -- over the next, say, month, if there is new evidence that suggests we need to change our findings and recommend changes to DPR, then we will then take

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that up at another meeting. But otherwise we'll go with these existing findings.
PANEL MEMBER BLANC: Well, that's what I'm just trying to clarify. So what is the -- so the process would be that we would move to reopen -- I mean that's what it would be? And it would be something that would come from you as chair?
PANEL MEMBER FUCALORO: And we're getting additional information -- right? -- from Andy on the -- on those -- on that study.
CHAIRMAN FROINES: I would send a letter to everyone on the panel saying, "Given the new information, we think it's prudent to reopen the discussion."
PANEL MEMBER GLANTZ: I'd like to propose an alternative procedural thing, which is sort of along the lines of things we've done before.
I'd like to move that we approve the report and the findings subject to this discussion today or tentatively approve them; that the -- well, no -- and that subject to -- the Chair will circulate the final edited cleaned-up findings for everyone to look at, and that before the -- and that, in the meantime, the Chair and Paul will work with DPR on this issue that Paul has raised about -- and then if,

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in those discussions, there does not appear to be any issue that's worthy of further discussion by the panel, John would simply sign the findings and submit them.
If it appears, between now and the next meeting, that there are substantive issues that would result in a change to the findings, then do not sign them and submit them; and we will complete the work at the next meeting. That's what I move we do.
CHAIRMAN FROINES: I think that's what I said but --
PANEL MEMBER GLANTZ: Yeah. But that's a

motion.
PANEL MEMBER FUCALORO: Who can second something that long?
PANEL MEMBER GLANTZ: We have a court reporter. Go ahead and do it -- second that.
PANEL MEMBER FUCALORO: I second it.
CHAIRMAN FROINES: The court reporter did not second the motion.
PANEL MEMBER FUCALORO: No, not -- I understand the gist of what you're saying. And I think that's what everyone understands. And I think that's fine.
CHAIRMAN FROINES: Is there discussion? Paul,

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you're okay with that?
PANEL MEMBER BLANC: Yes. That's fine. You're the one that has to be the most comfortable.
DR. FANNING: I have one question, John. And that's -- sorry to go back to the details -- but you had suggested two deletions to me that the panel has not yet seen. And would you like to identify those? Shall I identify those and ask about them?
CHAIRMAN FROINES: Yeah. I don't have the -- I don't know -- I don't remember what they are.
DR. FANNING: I know what they are.
CHAIRMAN FROINES: Okay.
DR. FANNING: Additionally, Dr. Froines recommended that Findings Number 38 and 40 be entirely deleted. These two findings address the margins of exposure that were reported for the ambient air studies as opposed to the application site studies.
So he considered those to be somewhat superfluous, given the MOEs that resulted from application site air.
CHAIRMAN FROINES: It's also consistent with our recommendations to DPR on the importance of application-site monitoring.
DR. FANNING: Yeah.

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CHAIRMAN FROINES: They have -- there's lots of ambient monitoring discussion in the report. But what I'm interested in, in the long run, is that our findings reflect the basis for decisions.
In other words, you could have 20 studies that you could report, but you used one to make a decision, and that's the one we should comment on. And so it's that kind of thinking that I think is important.
PANEL MEMBER FUCALORO: Right.
PANEL MEMBER BLANC: Well, then, I would propose a friendly amendment to Stan's motion that would take into account these two further deletions of Points 39 and 40.
PANEL MEMBER GLANTZ: 38 and 40.
DR. FANNING: 38 and 40.
PANEL MEMBER BLANC: 38 and 40. I'm sorry.

38 and 40.
PANEL MEMBER FUCALORO: That's not enough, Stan. The person who seconds it also has to accept the friendly --
PANEL MEMBER GLANTZ: All right. Do you accept it?
PANEL MEMBER FUCALORO: All right.
DR. FANNING: And I'm afraid there's one more

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that's pertinent to what the Chair said about that the finding should reflect the basis for the decision, the fundamental point that was used.
Finding Number 51 -- currently it says -- the last sentence reads, "Such is the case for MITC" -- referring to margins of exposure that are within a level indicating risk. "Such is the case for MITC, based on MOEs for acute exposure."
In our current Finding 39, MOEs for seasonal exposures at application sites are also within a range that indicate risk. So we may want to modify this finding to say, "acute and moderate term," or "acute and seasonal exposures" as sort of your basis for your recommendation.
CHAIRMAN FROINES: Okay.
PANEL MEMBER FUCALORO: In the last sentence, you would say, "Such is the case for MITC based on MOEs for acute and seasonal exposure"? That's what you're suggesting? Well, if that's what it says, of course, it should -- we should say what we mean.
DR. FANNING: Yeah. And one final question for the panel would be -- I just want to note that the way the title of your findings currently reads -- first of all, the italicized language that's highlighted obviously would be removed. That's just

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a comment. That's not part of your title for the record.
So the title, as we have it currently, would read "Scientific Panel Review Findings on the Department of Pesticide Regulation's Toxic Air Contaminant Document for Metam Sodium and other pesticidal sources of MITC."
PANEL MEMBER FUCALORO: Yeah. And that gets to what we were discussing in the last one, obviously -- the pesticidal sources of MITC. Does this disadvantage a company that produces metam sodium?
Or should we be more neutral and say, "Toxic air contaminants for pesticidal sources of MITC and MITC itself" or something like that? Does that disadvantage the company that makes metam sodium?
PANEL MEMBER GLANTZ: That is the main source of MITC.
PANEL MEMBER BLANC: Yeah.
PANEL MEMBER FUCALORO: I just asked the question. I don't feel really strongly either way.

PANEL MEMBER BLANC: I don't understand your question or your comment.
DR. FANNING: I just wanted to verify that the
0134
title reads as you want it.
PANEL MEMBER BLANC: But there must be a reason why you want to verify it. What is the implication that we should be aware of?
DR. FANNING: Well, the document that DPR has brought to you is titled "Evaluation of MITC." So I just --
PANEL MEMBER BLANC: Well, then, what I would suggest is that, if the title of our finding was worded as it is up until the very end where it says, you know, "and other pesticidal sources of methyl isothiocyanate -- MITC" -- and then there should be a parentheses -- "the document, which is entitled by DPR as" -- quote -- "'Evaluation of Methyl Isothiocyanate as a Toxic Air Contaminant, Parts A through C'" -- whatever it is, because obviously there could be confusion at a later date.
One could say, based on your findings, there's no corresponding document with the title that corresponds to it.
DR. FANNING: Right. Right.
PANEL MEMBER BLANC: I don't want to back off from what it is that we're doing our findings over. In fact, the question would be whether the title of this should say, "other pesticidal sources of Methyl
0135
Isothiocyanate and related breakdown products." But I don't think that that has to be in the title.
So that would be my suggestion would be that there be a parentheses saying, "the document entitled by DPR as."
DR. FANNING: Okay. I think that improves the accuracy of the title.
CHAIRMAN FROINES: Hearing no --
PANEL MEMBER BLANC: And I suggest that as a friendly amendment to Stan's previous --
PANEL MEMBER GLANTZ: In fact, I would actually go a bit further.
Given the history of this, I would say, "Scientific Review Panel Findings on the Department of Pesticide Regulation's Toxic Air Contaminant Document" -- quote -- "'Evaluation of Methyl Isothiocyanate as a Toxic Air Contaminant'" -- comma -- close quote -- "which is produced by the use of metam sodium and other pesticidal sources" or something like that to make it clear that the primary commenting is on the document on MITC but, in the title, to note where it comes from so there can be no question raised by any pesky people that we're commenting on this document.
PANEL MEMBER BLANC: Elinor, could you follow
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that? It's just a slight -- it's just a reordering.

DR. FANNING: Yeah. Reordering, essentially, the language that you gave. Yeah. I got that. And I'll get the details from the transcript if I wrote it down wrong.

Sorry. There's one more small one.

PANEL MEMBER GLANTZ: Oh, there's yet another one? Okay.

DR. FANNING: It's small.

PANEL MEMBER BLANC: Is this the last?

DR. FANNING: This would be the last of my questions for you.

PANEL MEMBER BLANC: Okay. Okay.

DR. FANNING: That would be in Finding 47. First of all, the current language -- "may exceed benchmark risk levels."

Dr. Froines recommended changing that to "regulatory exposure levels" for clarity. So that was a small wording change.

But the other question on that is the sentence -- "The combined risk of exposure to the mixture of irritants is the most relevant benchmark by which risk management strategies for metam sodium should be measured."

And the question is, given the

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discussions today, would you say that, "combined risks of exposure to mixtures is the most relevant benchmark by which risk management strategies for metam sodium and other pesticidal sources of MITC"?

PANEL MEMBER GLANTZ: Yes.

PANEL MEMBER FUCALORO: That's consistent language.

DR. FANNING: Okay. That's -- it's a small thing. But that's my last question for you.

PANEL MEMBER GLANTZ: So you have to second or you have to agree to that all that.

PANEL MEMBER FUCALORO: I certainly do. My cooperation has never wavered.

CHAIRMAN FROINES: This is undoubtedly the longest motion in the history of this panel.

PANEL MEMBER FUCALORO: I hate to have my name --

PANEL MEMBER GLANTZ: I think actually my whole lead thing was longer than that.

PANEL MEMBER FUCALORO: I hate to have my name associated with this, but --

PANEL MEMBER BLANC: The reason why Item Number 47 is important, by the way, is that it provides our Chair with a rationale by which he can do this follow-up on the data from Earlimart since

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clearly it would be a contradiction to, on the one hand, have a finding such as Number 47 and at the same time downplay the actual data set which addresses this issue.

DR. FANNING: Yeah. I think it's clear that your Finding 47 was designed to address situations

exactly like what you see in the Earlimart data.
CHAIRMAN FROINES: So we should proceed.
PANEL MEMBER BLANC: So do you want to call the motion?
CHAIRMAN FROINES: Yes. All in favor of the motion, which we will not try and restate. We will generate it from the transcript.
(Panel members raise their hands.)
CHAIRMAN FROINES: The vote -- those opposed?
The vote carries unanimously for approval of the findings.
PANEL MEMBER GLANTZ: And the report.
CHAIRMAN FROINES: And the report.
PANEL MEMBER GLANTZ: Subject to the motion.
PANEL MEMBER BLANC: As per the motion.
PANEL MEMBER GLANTZ: Or as per the motion. Yeah. No jokes.
CHAIRMAN FROINES: Okay. We have about an hour -- 1:15, 2:15 -- an hour and a half. Let's take
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a 5-minute break. And then let's go to the prioritization document. But before the prioritization document, Melanie, can we do carbon disulfide?
DR. MARTY: Yes.
CHAIRMAN FROINES: No. We're going to take a break. Then we'll do carbon disulfide. Then we'll do the prioritization document.
(Break.)
CHAIRMAN FROINES: Melanie, I think all we need to do is raise the issue because we've already -- on carbon disulfide, we have already had a discussion, and we did not take a vote on it. So we are simply -- this is almost an administrative matter rather than a technical one.
DR. MARTY: Right. The panel reviewed carbon disulfide and came to agreement with what we had done for developing the chronic REL.
But the problem was procedural. It had not been properly noticed before the last meeting that we were going to discuss carbon disulfide. So legally we couldn't vote -- you guys couldn't vote to approve the REL.
So now that we've properly noticed --
PANEL MEMBER FUCALORO: Oh, I see.
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DR. MARTY: -- it, you can take your discussion and move forward to approve or disapprove the REL.
CHAIRMAN FROINES: Well, do you want to make a brief showing of an overhead?
DR. MARTY: We can -- we have two slides which basically will remind you of what we did to generate the REL.
CHAIRMAN FROINES: Yeah. I hate to vote without 'cause there has been a gap and --
DR. MARTY: Okay.

DR. SALMON: Do you want to see those slides?
DR. MARTY: Yeah.
CHAIRMAN FROINES: Go ahead.
DR. MARTY: Yeah.
DR. SALMON: Okay. Coming up.
PANEL MEMBER GLANTZ: And Dr. Blanc will find something to reject.
DR. MARTY: Basically we looked at -- we looked at people exposed in the viscose rayon industry and looked at nerve conduction velocity as the critical effect. The NOAEL was not observed in this study.

We did a benchmark concentration calculation and adjusted for exposure continuity from

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the occupational exposures to equivalent residential 24-hour exposure. And the exposure duration was --
PANEL MEMBER FUCALORO: Can you go back? I'm sorry. Go on.
DR. MARTY: Sure. It was a chronic -- these folks were exposed chronically.
PANEL MEMBER FUCALORO: Thank you.
DR. SALMON: Okay.
DR. MARTY: Okay. So the human equivalent concentration turns out to be 2 1/2 ppm for the benchmark concentration at 5 percent response rate. We didn't feel that we needed a subchronic uncertainty factor because there was a duration of exposure.

And, of course, there's no interspecies extrapolation. We applied an intraspecies uncertainty factor of 10. So the total cumulative uncertainty factor is 10. This results in a chronic REL of basically 800 micrograms per cubic meter or 300 ppb.

We also, if you'll recall, had added in a paragraph about potential for differential impacts on children's health. And we looked at, basically, animal studies on developmental toxicity. And the data are messy.

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Some studies show effects. Some studies, using higher concentrations, show no effects. So we conclude that the two studies that did show effects, which were Russian studies, were not very consistent with the data base as a whole. And further research might clarify some of these potential behavioral "tox" impacts.

But no adverse effects were reported at concentrations below the proposed REL. And that summarizes what we had discussed previously.
PANEL MEMBER FUCALORO: So what is our require -- what is required of us? That we approve this REL --
CHAIRMAN FROINES: Yes.
PANEL MEMBER FUCALORO: -- for CS2?
DR. MARTY: Right.

CHAIRMAN FROINES: No. We approve the methodology and the analysis that led to the development of this REL.

PANEL MEMBER FUCALORO: So is there a motion to that effect that --

CHAIRMAN FROINES: Somebody should make --

PANEL MEMBER FUCALORO: I'll make a motion to that effect.

CHAIRMAN FROINES: Do we have a second?

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PANEL MEMBER FUCALORO: I think Stan should do that.

PANEL MEMBER GLANTZ: Well, actually this was discussed at the last meeting, which I missed. So I --

PANEL MEMBER BLANC: I'll second it.

CHAIRMAN FROINES: Does that mean that you are refusing to second it because --

PANEL MEMBER GLANTZ: I think I should abstain since I didn't hear the discussion.

CHAIRMAN FROINES: Oh, okay.

PANEL MEMBER GLANTZ: That's a problem.

CHAIRMAN FROINES: Okay. Is there further discussion on this matter? No?

Hearing none, shall we -- all those in favor of approval?

("Ayes.")

CHAIRMAN FROINES: Opposed?

(No audible response.)

CHAIRMAN FROINES: Abstentions?

(No audible response.)

CHAIRMAN FROINES: One abstention. So the vote carries. 1, 2, 3, 4, 5, 6 --

PANEL MEMBER GLANTZ: Just for the record, the abstention is only because I missed the last meeting

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and did not hear the discussion. It is not a statement about the --

CHAIRMAN FROINES: So the vote is 6 in favor and 1 abstention.

Thanks, Melanie. Thanks, Andy.

Paul Blanc gave a great seminar on carbon disulfide yesterday. So some of us are really well prepared for this discussion. Hearing Paul, we think the REL should be about 1 part per billion. That wasn't said seriously.

CHAIRMAN FROINES: Randy, do you want to introduce yourself for the --

DR. SEGAWA: Sure. I'm Randy Segawa, Environmental Scientist with the Department of Pesticide Regulation. And I'll be talking about DPR's updated prioritization for monitoring as well as risk evaluation of toxic air contaminants.

The draft prioritization that was sent to you earlier is an update of DPR's 1996 report. To give you a little bit of background, within California, there's approximately 900 different

pesticides currently registered for use in the state. Approximately 40 of those are currently listed as toxic air contaminants, which leave, of course, a balance of approximately 860 that we need to evaluate as potential candidates.

The prioritization that we're looking at now applies both to monitoring for candidate toxic air contaminants as well as their risk evaluation. If you recall, the previous prioritization -- 1996 -- was focussed mainly on monitoring. This update now deals both with monitoring as well as risk evaluation.

900 pesticides, of course, or 860 pesticides are quite a bit to evaluate. So we whittled that group down. And the current prioritization includes two groups of pesticides.

The first 200 pesticides that DPR's looking at under the Birth Defect Prevention Act, SB 950, as well as all pesticides listed under Proposition 65.

To do this prioritization, we set up a number of criteria to rank the pesticides. These criteria fall into three main categories -- toxicity, volatility, and use or sales.

Specifically in "toxicity," we're ranked according to acute toxicity, whether it's listed as a carcinogen under Prop 65, whether it's listed as a reproductive toxin under Prop 65, and the no-observed-effect level for chronic and subchronic exposure.

PANEL MEMBER FUCALORO: Just a second. Can I interrupt you?

DR. SEGAWA: Yes.

PANEL MEMBER FUCALORO: The criteria for prioritization -- you have Toxicity, Vapor Pressure, and Uses and Sale. Now, I can understand -- Use and Sale. Is "vapor pressure" a surrogate for "exposure"? It's not. What is exposure -- where is "exposure" in this?

DR. SEGAWA: In this case, it is a surrogate for "exposure." Yes.

PANEL MEMBER ATKINSON: Is it a very good surrogate? I mean, in reality, the partitioning between air and water, for example, is really the vapor pressure divided by the aqueous solubility or the Henry's law constant.

And if you're talking about air to soil or sediment, then it would be the Henry's law constant divided by the octanol/water partition coefficient. So it looks to me as though vapor pressure alone is a little bit too simplistic.

I mean, for something which is highly water soluble, it's not the only thing that's going to affect things getting into the atmosphere.

DR. SEGAWA: You're absolutely correct. By incorporating other physical or chemical processes, though, we do make the criteria much more complex and more difficult to score.

For example, not all pesticides are applied or mixed with water. Not all are applied to soil. There are -- a great many pesticides are applied to foliage, for example. And so --

PANEL MEMBER ATKINSON: Yeah. But if it's foliage, it would presumably be either onto water layers that are present on foliage or to the leaf itself, which would be octanol/water, again.

I mean there's plenty of stuff in equilibrium or in multimedia modelling that would allow you to go one step beyond just the vapor pressure.

DR. SEGAWA: Yes.

PANEL MEMBER ATKINSON: And it's either going to be either Henry's law constant or octanol/water or a mixture of those two.

DR. SEGAWA: So do you have a specific suggestion or recommendation, then?

PANEL MEMBER ATKINSON: I think -- no. That's the problem.

DR. SEGAWA: Yeah.

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PANEL MEMBER ATKINSON: I mean it --

PANEL MEMBER FUCALORO: Yeah.

PANEL MEMBER ATKINSON: -- depends whether it goes onto soil or onto water.

PANEL MEMBER FUCALORO: And it also depends on what's known of the particular compound.

PANEL MEMBER ATKINSON: Yeah. That's right.

PANEL MEMBER FUCALORO: That's one of the problems.

DR. SEGAWA: Okay.

PANEL MEMBER ATKINSON: I mean I would have thought that the Henry's law constant would have probably been a bit better than just vapor pressure alone.

PANEL MEMBER BLANC: When you use the vapor pressure -- just to clarify -- it's broken down into an ordinal scale -- sort of "not very much," "a lot," "a little." Or you're not using the absolute vapor pressure as a number?

PANEL MEMBER ATKINSON: Yeah. It's used --

DR. SEGAWA: Yes. Actually --

PANEL MEMBER ATKINSON: -- 2 further on -- 2 or 3 further on.

DR. SEGAWA: Yeah. We'll get into more details on each of these criteria. I'm just going to

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general characteristics right now.

PANEL MEMBER BLANC: Maybe we can come back to this question --

DR. SEGAWA: Yes. Exactly. Yes.

PANEL MEMBER FUCALORO: It is ordinal, though.

It has to be ordinal, though. You'll see.
DR. SEGAWA: Okay. These criteria are changed somewhat from the criteria we used in 1996. For acute toxicity, that criteria is unchanged, both in the criteria we use as well as the scoring.
The Prop 65 carcinogen criteria is an addition. It replaces the EPA and NTP carcinogen categories that we had used previously. The Prop 65 reproductive toxin category is added. We did not have a category for a reproductive toxin previously.
The criteria for chronic and subchronic NOEL -- that's unchanged from 1996.
For vapor pressure, we have changed the scoring, which I'll get to in more detail on a couple more slides. Same with use and sales. We've changed the scoring on how that's ranked.
And as Dr. Atkinson pointed out previously, we have included a Henry's law constant. We're proposing to drop that at this point. But we can discuss that further once we get to the

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volatility scoring.
The exact category in scoring for acute toxicity -- again, this is unchanged from the 1996 prioritization. It's based on US EPA's toxicity categories which, in turn, is based on a 4-hour inhalation LC50.
Under the EPA's categorization, we assigned 4 points if the LC50 is less than .05 milligrams per liter and 3 points if it's between .05 and .5. And you can see that it's one point decrease for every tenfold factor -- or every increase in tenfold factor from there.
CHAIRMAN FROINES: I have a question about this.
DR. SEGAWA: Yes.
CHAIRMAN FROINES: But Melanie's gone, and so's Andy. Oh, there's Andy. Sorry. Oh, I -- sorry. I just didn't see you.
PANEL MEMBER FUCALORO: What's in your glass?
PANEL MEMBER GLANTZ: Stand up, Melanie. Melanie will stand up.
CHAIRMAN FROINES: I formally apologize. I want it on the record. Okay. Melanie or Andy or both -- of the acute RELs that the panel has approved, how many pesticides would you say are

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included in that list?
DR. MARTY: Very few. I'm guessing it's less than 5 -- 5 or less. The ones I can think of are acrolein, methyl bromide, chlorine --
PANEL MEMBER BLANC: Chloropicrin.
DR. MARTY: -- chloropicrin --
DR. SALMON: Well, phosphine, if you count that.
DR. MARTY: Phosphine. I don't think we have an acute REL for phosphine.

CHAIRMAN FROINES: I'm asking the question, in part, because it seems to me that a 4-hour inhalation LC50 is a very, very simple estimate of acute toxicity and that it seems to me to be an inadequate estimate of acute toxicity.

Given the -- given what we did, for example, in the acute RELs, which is a very -- was a relatively complex process, the question is: Is this the best we can do in terms of acute toxicity?

And I think it's an important issue because, for example, MITC acute toxicity is after -- at the end, what was the basis for the recommendations. And so the question is, by using an LC50 as the only way of evaluating acute toxicity, are we getting the best bang for the buck, so to

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speak?

PANEL MEMBER BLANC: In a relative phasing --

ASSISTANT DIRECTOR JONES: John, Randy can correct me if I'm wrong, but I believe -- and I don't know formally if that was the same value that was used -- but that is the most readily available data that we have at DPR.

So I think, for purposes of prioritization, trying to use readily available data as one of several factors for attempting to rank compounds, both for monitoring and for risk assessment, was the effort here.

CHAIRMAN FROINES: Well, it's an important question because an LC50 is such a simple endpoint. And I don't know where, if one looked at MITC or any of the other acute RELs, where, how --

In other words, if you take the RELs that we have that Melanie's developed and you look at the LC50s and then you take a look and see if -- what you get when you compare them, that would be an interesting exercise to see if, in fact, we're getting a sufficiently complex evaluation of acute toxicity.

ASSISTANT DIRECTOR JONES: I don't think the effort of the exercise was to do an evaluation of the

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acute toxicity. It was to -- and I think, on the advice of the panel -- try to simplify the array of materials which we used to establish some kind of prioritized list.

CHAIRMAN FROINES: I do not agree with you. In your prioritization document, the notion of simplification -- one has to be careful. The panel really did not go through the prioritization categories in detail at the January meeting.

And I think that you may have misunderstood our intent. But we'll come to that as we go through.

ASSISTANT DIRECTOR JONES: Okay. And I guess we didn't get feedback to that effect.

CHAIRMAN FROINES: Go ahead.

PANEL MEMBER BLANC: Well, I was going to say that, on a pragmatic basis, one of the things that one would be looking for in any of these schemes would be "Do you get a spread of results with it?"
PANEL MEMBER FUCALORO: That's what I was going to ask. In other words --
PANEL MEMBER BLANC: So, at a certain point after we go through this, is a sort of screening question you would ask yourself: "If I categorize these individual chemicals, based on this axis of the
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scoring system, does everybody come out as a 1? Or does everybody come out as a 4?"
If there's a spread, where 25 percent of them are 1 and 25 percent of them are 2 and 25 percent are 3 and 25 percent are 4, then I would say you are getting a spread that's telling you something relative. It seems to be differentiating in some way.
You're asking a question about "Is that the very best way to differentiate?" but, typically, for this kind of crude scoring, if you've got different domains that you're evaluating and it doesn't -- on the face of it, it would be seem to be serving its purpose.
And I think that's a practical way of looking at that kind of single axis. So we really -- it will depend a bit on how it performs, I think.
PANEL MEMBER FUCALORO: Why don't you just ask the question? I mean do you have the chemicals listed? And you're looking for the -- you said a spread, a distribution among these 4 categories. Did you have a distribution?
DR. SEGAWA: Yes. If you look at the report, the table near the back -- at least -- it lists all the individual scores in the individual categories.
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PANEL MEMBER FUCALORO: Now, this is a report dated when?
DR. SEGAWA: January, 2002.
PANEL MEMBER FUCALORO: Right.
CHAIRMAN FROINES: One may have a very different dose-response slope or general relationship between irritation or immunologic changes or any other acute endpoint you might measure relative to mortality. And so to use mortality as a measure of acute toxicity is a little troubling, I think. But --
PANEL MEMBER BYUS: How do you want to do this, John? Do you want us to jump in at any appropriate time? Or do you want us to --
CHAIRMAN FROINES: Well, I think that, as we go through, I think that we should -- as Randy goes through each of his categories, I think we should get comments during that particular category. I don't think we should then try and go back later. One, we'll run out of time. And, then, secondly, it won't

be as focussed.
PANEL MEMBER BYUS: So I have a comment about the LC50s as well. I mean I teach in pharmacology. In addition to what you've just said about toxicity versus lethality for drugs, it being much more
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important what the toxic dose -- 50 percent -- is rather than the lethal dose 'cause it's the dose limiting toxicity.
But also I teach that it's also much more relevant to do something like the LC5 as opposed to the LC50. I mean what you really don't want -- you really want to know what is happening on the fringes of your population rather than the 50 percent mark.
So if you were going to use a lethal dose for comparative purposes, you wouldn't pick the 50 percent-point dose. You would pick the 5 percent dose because that would affect the slopes, the distribution much more. It would be much more relevant.
DR. SEGAWA: Yes.
PANEL MEMBER BYUS: And I assume that that information is available. If they've calculated LV and LC50s, they've also calculated -- you'd need -- the 5 percent dosage --
DR. SEGAWA: Right.
PANEL MEMBER BYUS: -- calculation would be there as well.
DR. SEGAWA: Right.
PANEL MEMBER BYUS: So I would suggest you
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might consider that.
DR. SEGAWA: And actually that was going to be one point I was going to make. We are trying to prioritize over 200 pesticides here. And whatever criteria we choose, we have to be able to score it for all 200-plus pesticides.
And, for instance, I don't know that we have RELs for all 200 plus or so. And I -- same with the LC5. We may or may not have them.
PANEL MEMBER BYUS: But if somebody's calculated -- I can guarantee you, if someone has calculated the LD50 or the LC50, they must have the LC5 data. It is available. It's there in the curve. I think you have to -- it would be there, by definition.
DR. SEGAWA: Yeah.
PANEL MEMBER BYUS: And you could get it; and it would be much better than using the 50 percent point, I mean if you're going to use lethality.
DR. SEGAWA: Yes. Any other questions or comments on the acute toxicity? Okay.
Then, moving on to the Prop 65 categories, we're using both the carcinogen listing as well as the reproductive toxic listing. 2 points if the pesticide is on the cancer list; zero points

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if it's not.
Same with the reproductive list -- 2 points if it's listed and zero points if it's not.
PANEL MEMBER BYUS: I have a question. Why is it that it only gets 2 points if it's a carcinogen? I mean it just seems to me what you're doing is vastly -- given your -- we can come back to this at the end if there's a global discussion of how you decide how many points are in each category.
And this is a very important consideration because it affects the overall number --
DR. SEGAWA: Yes.
PANEL MEMBER BYUS: -- and the overall priorities. But, in any case, giving -- only giving, if it's a carcinogen, 2 points, I think vastly underestimates the significance of the toxicity of a carcinogen in this overall scheme.
DR. SEGAWA: Right. The scoring --
PANEL MEMBER BYUS: So it would be 2 points out of how many total?
DR. SEGAWA: Out of 28 possible. Yeah. You may very well be correct. The points assigned and the way we scored it is definitely subjective. There's really no objective way to determine the
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scoring, and so it is a subjective measure.
One of the things that we tried to revise from the 1996 prioritization was your recommendation that we want to try and balance the weighting of the toxicity use and volatility.
And so, before, the toxicity categories were highly weighted, in the previous prioritization. And so now we're trying to weight them less in comparison to the other two categories. So that's what led to the lower scoring in this case.
PANEL MEMBER BYUS: Okay. I still don't -- I mean I think we should come back to this --
DR. SEGAWA: Yes. I agree.
PANEL MEMBER BYUS: -- discussion at the end relative -- how you -- how many points you -- whether the categories you've come up with are correct and then how many points you give each category --
DR. SEGAWA: Yeah. I agree. We should come back to --
PANEL MEMBER BYUS: -- and how you weigh each category.
DR. SEGAWA: Yeah. When we look at the overall score and how that comes out.
PANEL MEMBER FUCALORO: Yeah. We've been through this before.
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DR. SEGAWA: Yeah.
PANEL MEMBER BYUS: We did.
PANEL MEMBER FUCALORO: Okay. And I brought up some issues. I guess I'll just mention them here.

First of all, there are two types of categories here. One is exposure, availability in the environment. The other is toxicity. It's not clear whether you should be adding them all together or not or multiplying one by the other, you know. I don't know.

The other thing is I still think that it's unwise mathematically to have these -- make sure these add up to 4. You can do a 1-to-10 scale on everything and then put coefficients in front of each category, which you can then adjust to make -- to say, "Well, I want to weight cancer, a carcinogenicity, a little more." You turn up that coefficient a little more, you know.

So that's my suggestion. I'll make it. I'm not going to veto anything. I don't think I have the authority to veto. But I'm not going to vote against it if you don't. But I think you ought to be thinking about those sorts of things.

I think there's probably some research on how one makes priority lists. And I don't know if

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you're familiar with it. I'm not. But I can just imagine, though, some of these issues coming up. It's just common sense. That's all.

CHAIRMAN FROINES: I -- as a philosophical construct, I agree with what Tony was saying. I might have said it a little differently. I think that the one subject area is human health. Is the material toxic? That seems to me to be the primary criteria that we're concerned with.

If there is exposure, is there a potential for human beings to be adversely affected by the pesticide? That's where -- that's it. That's the centerpiece of everything we do. And that has to have -- it seems to me -- has to be the primary evaluation that we carry out.

Then we say, "Now, having said that, having said that X chemical has significant toxicity or potential toxicity, then are people exposed to it?" And that becomes a weighting of the toxicity.

So the algorithm of using simply an additive idea doesn't reflect the sort of underlying objectives that one has to, I think, establish. To me, the primary objective is to determine toxicity and then to determine whether people are exposed to it or not.

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PANEL MEMBER FUCALORO: Exactly.

CHAIRMAN FROINES: And that means that your exposure rating could be multiplicative, for example. It doesn't have to be additive because you end up with propargite and sulfuryl fluoride having the same numbers.

Those are very different compounds in terms of toxicity. And so it -- in a sense, both of them having 18 ends up meaning that one has made a

mistake. One's made a mistake in terms of prioritization, I think.

And so it seems to me that, once you say, "Toxicity is the centerpiece and the modifying factor is the exposure," you've defined the way to look at the problem. And so it seems to me that -- I would argue that "zero to 2" is ridiculous. It's not even in my window of discussion.

So we should come back to it. But I think that this is certainly an inadequate estimation of a major biological endpoint. And so I'll leave it for the time being.

But I think that one has to think about it in ways that are having -- one has to define what are the objectives and what are the key parameters that we want to be concerned with and then

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decide what the algorithm looks like.

DR. SEGAWA: Okay.

PANEL MEMBER GLANTZ: Well, you know, I mean I think they tried to do -- first of all, whether you score it the way they do or whether you score everything on a zero-to-10 basis and then put a weight on it, you get the same results. So that doesn't really matter.

And the other problem that's inherent in this whole process -- and there's absolutely nothing you can do about it -- is, when you've got a multidimensional system, which you have, there is no unique order. I mean you can prove that in number theory.

So I mean I think that they're doing the best they can. And I think that one of the changes between -- which we haven't gotten to yet -- but one of the changes they made from the last prioritization document was to weight the amount of the stuff that's used much more heavily than they did before.

So I think that, effectively, they've been trying to do exactly what you're saying. I mean, if you wanted to change the scale so that you took the toxicity score and multiplied it by the

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amount that's used or the exposure -- I mean that's not a bad idea.

But I don't think that those kinds of adjustments are going to get around the sort of fundamental theoretical problem you have that you can't uniquely rank a multidimensional system.

And, you know, if you look at the prioritizations that we've done for the ARB TACs -- I mean what was done then was that we came up with this rough order, based on these kinds of scales, which actually struck me as pretty reasonable, and then looked at them and applied some judgment and moved things around.

And I really think that will get you

further than, you know, tinkering with the scale.
I mean my concern about this was I noticed the dates, you know. This law passed in -- what? -- 1983, which is not quite 20 years ago. And how many pesticides have we made it through the process? 3? Does that count MITC? So 3 and a half.
So that's an average of about 1 every 7 years or something. That's kind of -- that, to me, is a much bigger problem. And if DPR could move these things a little more expeditiously, the details of exactly where you ended up in the list would be

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less important.
CHAIRMAN FROINES: Well --
PANEL MEMBER FUCALORO: I don't agree.
PANEL MEMBER GLANTZ: Well, that's okay.
PANEL MEMBER FUCALORO: I don't agree. I think -- I think it's important to have a good priority list. But I do agree with you. There's the concern that the pesticides have not come before us in very frequently -- at a high frequency rate. I think I agree with that.
But I do believe that any priority list should be broken down into two parts -- again, the exposure potential and the toxicity. Just like one has a slope factor, for example, in some sort of, say, carcinogenicity and then the exposure. You put them together in a multiplicative fashion.
You know what you're talking about. You're talking about exposure. You're talking about toxicity.
PANEL MEMBER GLANTZ: Oh, no. And I'm not disagreeing that DPR wanted to adjust their methodology. That's probably slightly better. But my guess is it's not going to change the list radically.
PANEL MEMBER BLANC: I think actually it could

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change it incredibly --
PANEL MEMBER GLANTZ: Oh, you do?
PANEL MEMBER BLANC: -- because of the arithmetic --
PANEL MEMBER GLANTZ: Okay.
PANEL MEMBER BLANC: I mean, what you say is true in principle. But I think that there are some aspects of this which are so ill-founded in terms of, on a practical basis, how your weights come out.
I mean I know you haven't finished going through your slide by slide. But since we have a handout, I'm going to speak, based on the handouts since we've, you know --
PANEL MEMBER FUCALORO: Just let us know which one.
PANEL MEMBER BLANC: Yeah. So, you know, you have two slides on -- that are related to the axis that Tony refers to of use. One is vapor pressure, and one is use and sales.

Now, we can debate about whether or not there should be something dealing with nonvolatile, highly water-soluble materials that would tend to, you know, be out there in droplets very quickly.

But leaving that aside, even if you

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took this at face value, what one would get is 16 points with a volatile, widely used agricultural chemical. And the most you could get from the very most toxic chemical, based on your scoring system, is -- I'd have to do the arithmetic, but I think it's --

PANEL MEMBER FUCALORO: 12.

PANEL MEMBER BLANC: -- is it 12?

So there's something screwy about that if you're using it, as you propose, as an additive scale because what it means is that something with a low toxicity of 1 that's, you know, volatile and highly used is going to be your highest priority material, which we would fundamentally and completely disagree with.

So what happens is that -- Stan, I think they've carried it to such an extreme, that it doesn't mean that it can't -- once you come to sort of fundamental decisions about "What are the domains in which you think toxicity matters? And what are the domains in which you think exposure matters?" then it can very quickly -- you can tinker with the scales so you're getting reasonable separation and weight.

So I think that the first step is to

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make sure there's sort of consensus on what your domains are. And I think you've gone -- what you've done here has sort of conceptually muddied the waters.

PANEL MEMBER FUCALORO: I mean a same way of saying that is that -- I mean some of -- in the category where the total is 16, water falls into that category in this scale.

PANEL MEMBER BLANC: Yeah. The word and chemical. So --

PANEL MEMBER FUCALORO: Its use is all over the place and its vapor pressure is very high, I mean but, you know, that's --

PANEL MEMBER FRIEDMAN: For the record, you know, apparently in the 1996 categorization, you spent a lot -- you devoted a lot more interest in toxicity and since then have added some of these exposure variables. And I was just wondering -- was that in response to this panel's recommendation?

DR. SEGAWA: Correct.

CHAIRMAN FROINES: No.

PANEL MEMBER FRIEDMAN: So I think we should make that --

CHAIRMAN FROINES: No.

PANEL MEMBER FRIEDMAN: You may have carried
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it to a greater extreme --
CHAIRMAN FROINES: We have read the transcript from the January meeting. And there is nothing in the transcript that reflects that statement. That's not correct, and I won't accept it -- that this panel said that there should be a lower weighting of toxicity. This panel never said that.
PANEL MEMBER FRIEDMAN: No. But we suggested that they add more exposure; is that correct?
CHAIRMAN FROINES: Yes.
PANEL MEMBER FRIEDMAN: So maybe they've carried it to an extreme that we would not agree with. But I think it's fair -- in all fairness, we should recognize that they're trying to respond to something that we suggested. Maybe we don't agree with the way they've done it, but I think that they're being responsive.
CHAIRMAN FROINES: I think -- having gone over the January transcript, I think that that may be true. But that was never the intent of this panel -- to create a situation that we now have here with this document.
PANEL MEMBER BLANC: So I think there's a couple of easy solutions, really quick fixes. And they would just require a sort of a meeting of the
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minds.
One is that I couldn't support more strongly Dr. Fucaloro's comments that the use scoring, however it is determined mathematically, should be used as a multiplicative factor and not as an additive factor.
And that will generate two scores -- unweighted scores and use-weighted scores, which will be important. And we'll need to see which chemicals have moved because of the use-weighting.
The nuance of how you create the use-weighting score and how big it is -- I would just suggest it as a magnitude -- if you're using it as a multiplicative factor, it doesn't need to be such a huge numeric value.
But I think the point was very well taken about water solubility and what that would mean functionally for exposure. And that needs to be in there somehow.
In terms of the toxicity score and how the weightings are for toxicity, I think that it would be the consensus, I would imagine, of this group that carcinogenicity and reproductive toxicity aren't are a kind of trade-off; that, if those are domains in which toxicity are important, those
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domains need to be weighted as strongly as whatever measure of acute toxicity -- generic or acute toxicity you use.

And then you have a sort of a generic measure of chronic toxicity which you're basing on DPR or US EPA chronic and subchronic no-observed-effect levels. That's your sort of generic chronic toxicity.

Then you have a reproductive axis. And then you have a carcinogenicity axis. And then you have an acute toxicity domain. And I think what the panel is telling you is those all need to be weighted equally, at least.

Nobody's saying, "Make the carcinogenicity the driving point." But it shouldn't be underweighted, half underweighted, which is what you're doing.

One question I would have, from a technical nature, is since you seem to have NOELs for chronic or subacute toxicity for every chemical, since that was one of your pragmatic ways of choosing a rubric, do you not also have NOELs for acute toxicity that parallel? Or do those not exist?

DR. SEGAWA: I presume that we do, but I don't know for sure.

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PANEL MEMBER BLANC: Well, wouldn't using the acute toxicity NOELs as the acute toxicity marker, rather than the LC50, address the comments that have been made by the other panel members?

And wouldn't it also be more symmetric or more parallel with your other toxicity measures, assuming that they're available?

ASSISTANT DIRECTOR JONES: In all cases, we do not have acute NOELs. Oftentimes for the kinds of assessments -- and Andy can tell me if I'm wrong -- for the kind of assessments that you heard today or have been discussing with MITC, the acute NOEL is extracted out of another study.

And part of the reason that I said we use the LC50 inhalation study is we receive a battery of acute toxicity studies that don't look at NOELs. They look at LC50, LD50, and that kind of thing.

And I think, as Dr. Byus indicated, from some of these data, we may be able extract lower values; but we don't -- we can't readily, you know, sort of --

PANEL MEMBER BLANC: Whereas --

ASSISTANT DIRECTOR JONES: -- readily, readily get the acute NOELs.

PANEL MEMBER BLANC: Whereas -- whereas for

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the chronic and subacute, the EPA has already extracted or calculated the NOEL?

ASSISTANT DIRECTOR JONES: And we have too. We have too. All I'm saying is those values come more readily out of specific studies that are designed to ask questions about chronic and subchronic toxicity.

We don't -- there is not necessarily a

comparable parallel for acute toxicity where you can get an explicit NOEL out of that. And we oftentimes find ourselves extracting an acute NOEL out of a repro study, a carcinogenicity study.

PANEL MEMBER BLANC: Would you have an estimate of the list of the agricultural chemicals in question -- for how many of them, for some other reason, you already have an acute NOEL for one reason or another?

ASSISTANT DIRECTOR JONES: I'd have to go back and look at the number of risk assessments we've completed.

PANEL MEMBER BLANC: Because, you know, if it's 60 or 70 percent already -- if you think it's that high --

ASSISTANT DIRECTOR JONES: Oh --

PANEL MEMBER BLANC: -- then it would probably

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be worth doing the other 30 percent.

ASSISTANT DIRECTOR JONES: We probably don't. We probably haven't done that -- and these are risk assessments we've done under the Birth Defect Prevention Act.

The numbers are probably not that high because we, you know -- on an annual basis, we probably complete probably six to eight active ingredients per year.

PANEL MEMBER BLANC: In terms of NOELs, acute NOELs?

ASSISTANT DIRECTOR JONES: No. In terms of completed risk characterization documents that parallel the documents that you look at under the 1807 act.

PANEL MEMBER BLANC: And the EPA doesn't do that -- federal EPA doesn't do that either, on terms of acute?

ASSISTANT DIRECTOR JONES: They're going through their re-registration process. And under their risk assessments contained in their registration-eligibility documents, they may include those data. But they do -- they basically are looking at the same data set that we are. And so they do not have explicit studies that are designed

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to determine an acute NOEL.

PANEL MEMBER BLANC: Well, one solution to this problem in the domain of acute toxicity would be, assuming you had a 4-point ordinal ranking, is that -- in cases where you have acute data, you use that and only in cases -- in cases where you have an acute NOEL data already or it's very easily obtainable, you use that.

And the default is that you use -- your fallback is the LC50, if you don't.

ASSISTANT DIRECTOR JONES: Okay.

PANEL MEMBER BLANC: 'Cause we obviously don't want you to delay having a priority scheme for 20

years doing another series of studies. That's not the point here. So I'm sensitive to the efficiency of the data. But it seems to me you could have an algorithm that would allow you to have your cake and eat it too.

CHAIRMAN FROINES: Well, I think that, going back to Stan's point, that up to now this panel has approved -- what? -- 3 pesticides, 4 pesticides in 20 years. Dealing with the vast numbers of pesticides isn't necessarily our problem.

I mean I think that, if you look at the listing, if you have between 6 points and 8

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points, you go between 800,000 and greater than 12 million pounds, one could argue that below 800,000 doesn't mean that there aren't some important pesticides.

But one could look at the Categories 6, 7, and 8 in terms of use in California -- that would be ranging with for greater than 800,000 pounds -- and try and do a more -- look at acute NOELs or look at carcinogenicity or look at reproductive toxicity or the chronic, and it seems like we might come up with a fairly interesting list that -- for which there were potential, anyway, of humans being affected in California, adversely.

And so it may be that, rather than just say, "We need to use the LC50," that we may want to slice and dice it a slightly different way to try and get at -- quote -- "the problems" rather than --

The priority score is an attempt to identify problems. And so that's what its endpoint should be. And so, to the degree that we don't -- we end up with systems that don't help us answer that question or artificially answer it, then it seems to me the prioritization doesn't -- isn't as effective as one might hope.

PANEL MEMBER BLANC: Well, John, would you

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agree with Tony's fundamental suggestion --

CHAIRMAN FROINES: Absolutely. That's my starting point.

PANEL MEMBER BLANC: Would the other panel members agree with that?

So, in other words, whatever version you come back to us should have two sets of scores -- use-unweighted and use-weighted. And I suggest that the use-weighting be by a multiplicative process.

ASSISTANT DIRECTOR JONES: I think one observation that we made, in taking a couple of different scoring mechanisms and playing it through, is that the pesticide active ingredients about which we have most concern, which tend to be the fumigants, show up at the top of the list.

And we have devoted considerable effort to evaluating the risk of those, both in this setting and independently. And so, from the

standpoint of how we are looking at human exposure, our simplified prioritization scheme reflects the kinds of concerns that we are dealing with.

PANEL MEMBER ATKINSON: I'd like to come back to the volatility side of things. I mean I notice that, in 1996, you were using both vapor pressure and Henry's law constant. And that, to a certain extent,

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was really double-counting. So I agree with you certainly going towards one measure of volatility.

And I can certainly see that vapor pressure's the obvious, simple one. But I have do have a couple of comments on, even if you used vapor pressure, I really don't see that you need to split it up into zero to 8 points because really, once something's got any reasonable vapor pressure greater than, say, 1 torr, it's going to presumably partition somewhat into the atmosphere.

The other one is that if it's got vapor pressure less than 10-to-the-minus-6 torr, it's never going to exist in the gas phase in the atmosphere.

Admittedly, you might use that, if it's going to be sprayed and exist as a particle or as an aerosol droplet -- lower volatility compounds. But, otherwise, I think you could split it up into, at the most, two or three categories.

And I would urge you to look into using either Henry's law constant and the octanol/water partition coefficient or vapor pressure divided by the octanol/water partition coefficient for things that are sprayed or end up in the soil and the Henry's law constant for things that are -- that end

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up in the water.

I think you would get -- it would help a little if the data's available and you can do it.

DR. SEGAWA: I was going to say, one of the problems we had with Henry's constant was that we didn't have reliable data for a lot of the chemicals.

PANEL MEMBER ATKINSON: For the -- I mean it's essentially the vapor pressure divided by the aqueous solubility. So --

DR. SEGAWA: Right. But in many cases, we didn't have data that was done at the same temperature. And so we had to make mathematical adjustments and things like that. And, like I said, we didn't have a whole lot of faith in some of the data.

PANEL MEMBER FUCALORO: But it's -- well, I'd guess you need to know the enthalpy.

PANEL MEMBER ATKINSON: I don't know. Just the aqueous solubility -- I mean vapor pressure -- the one that really changes is the vapor pressure. The aqueous solubility isn't going to change much, not dramatically between 20 C and 30 C or whatever.

CHAIRMAN FROINES: I have a -- I want to

change the subject a little bit. I have one question that is troubling a little bit. And that is that the problem -- there is problem, it seems to me, with the notion of administrative listing, namely, that there's a list of chemicals here for which there are some highly toxic chemicals that are administratively listed because they were HAPs, defined by the Clean Air Act amendments in 1990.

But the HAPs -- because they become administratively listed, what worries me about them is that they fall into a black hole and are then not addressed in the future because it seems to me that, at some level, a compound that's identified administratively still requires a risk assessment because a HAPs doesn't give you a risk assessment and that the risk assessment then becomes used in the actual control process for risk management purposes.

And so one of the questions that I have, in terms of priority, is the compounds that are listed here as administratively designated -- where do they fit in terms of this prioritization? Because what worries me about lists of chemicals is that the list becomes an end -- and it has the danger of becoming an end in itself.

And but obviously we list things so that then is there a subsequent process to control it -- control public exposure.

PANEL MEMBER FUCALORO: Mitigation.

CHAIRMAN FROINES: So the question then becomes, of the compounds that are administratively listed, when do we see the risk assessments for them and control strategies developed accordingly?

PANEL MEMBER FUCALORO: You do have control strategies for those listed. I mean that's --

DR. SEGAWA: For some, we do. But getting back to your original question, this panel, at least in the past, has only looked at candidate toxic air contaminants, not pesticides that are already listed.

CHAIRMAN FROINES: No. That's not true. We have spent months and months and months on Melanie's chronic RELs and acute RELs. We've approved hundreds of chemicals that were already identified as toxic air contaminants. So it's not true that we've only dealt with candidate compounds.

DR. SEGAWA: With DPR, that's the case.

PANEL MEMBER GLANTZ: Yeah. But we've only done 3.

PANEL MEMBER FUCALORO: You can't cover too many.

ASSISTANT DIRECTOR JONES: I guess, in answer to another part of your question, methyl bromide is an example of a HAP. We haven't brought that before the committee. That does not mean we haven't moved ahead with a risk assessment and moved ahead with

risk-mitigation measures.
CHAIRMAN FROINES: The what?
ASSISTANT DIRECTOR JONES: That we haven't moved ahead with risk-mitigation measures. So we have used other authority than listing it as a TAC or listing it as a TAC administratively, because it is a HAP, to move ahead with mitigation.
CHAIRMAN FROINES: So you're suggesting that you wouldn't bring those compounds before this panel to review the risk assessment?
ASSISTANT DIRECTOR JONES: We wouldn't necessarily. I mean I think that had been a direction in the past. And I think that's what Andy -- Randy is referring to.
So in the case of methyl bromide, we had the National Academy of Science review our risk assessment and -- but moved ahead with mitigation based on that risk assessment.
CHAIRMAN FROINES: But I -- actually methyl bromide is a case in point because this panel has the intent of taking up methyl bromide, that Paul Goslin and I discussed two years ago about our taking methyl bromide -- bringing, after the National Academy

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review occurred -- was to bring it before the panel for a review.
So that compound actually -- we have an expectation of seeing it at some point.
ASSISTANT DIRECTOR JONES: Well, I guess he didn't inform us of that. I mean I didn't think going through another review with this panel would necessarily serve the purpose because I mean we still have three documents that we would like to move ahead with on, with the panel.
CHAIRMAN FROINES: Well, okay. Let's put this aside and -- but it's an issue of -- the issue of administratively listed compounds is an issue, I think, that deserves attention. So let's go back to the prioritization issue.
PANEL MEMBER FUCALORO: Well, what are we to do now? I mean are you going to go back and review this document and your method? Or --
ASSISTANT DIRECTOR JONES: Well, you know, I mean it's April of 2002. We've presented what kind of areas we were working on, based on your comments to us in January of 2001. We will take your comments that you provided at this meeting and rerun it and identify additional data that we conclude.
And I would say we will probably have

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continued discussions with the committee. And we'll provide you drafts as we have done and would appreciate your comments on how we take what you have described to us today in transforming the data.
PANEL MEMBER GLANTZ: Yeah. I would hope, given the very slow pace that this has gone and the fact that you have most of the information you need

here -- and I'm sort of beaten into submission on the multiplication. I agree now.

To generate another draft of this, done along the lines that the panel's talking about, should not be a huge undertaking. So I would hope that you could bring back another draft before we meet -- don't we have a meeting next month? So it's in two months?

So I would hope that we could have a draft of this come back then and that everyone would sort of nod and say, "Oh, yes. This looks very good," 'cause I think the panel's given you pretty specific guidance.

And as they say, I think you have the data you need. It's just a matter of kind of recasting the calculations.

And, you know, the other thing that you might want to do -- and I actually was, a long

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time ago, was the lead person with the ARB prioritization.

You might try a couple of alternative reasonable weighting schemes and just see how much difference it makes too because, you know, if you get results that are reasonably independent of the specific numbers you pick for the weighting, that's always encouraging.

And I think, if you take the suggestions that have been made, that, in some sense, collapses some of the categories. And then the other thing is with the weighting by multiplying by exposure -- that will probably simplify things some.

PANEL MEMBER FUCALORO: Yeah. And I think that's a good idea to see that the list seems be to relatively independent of the algorithm used to develop the list is what he's suggesting.

But also there's a lot of experience in your department. If someone looks at a list and says, "This is cockamamy. This should not be so high. This one should not be so low," that then you know you have a problem with your algorithm and you have to rethink it.

ASSISTANT DIRECTOR JONES: Well, I think that, given the experts at the table and toxicologists that

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I work with to take your previous suggestions and rework the list, the list came out kind of like we thought it would, based on --

PANEL MEMBER FUCALORO: I didn't hear what you said. I'm sorry.

ASSISTANT DIRECTOR JONES: Yeah. Because the list came out --

PANEL MEMBER FUCALORO: "Because the list came out"? I didn't hear you.

ASSISTANT DIRECTOR JONES: Because the list came out like we would expect.

PANEL MEMBER FUCALORO: I see.

ASSISTANT DIRECTOR JONES: And it had, you know, compounds that we have the most concern about that have the highest use and --
PANEL MEMBER GLANTZ: Oh, maybe it's --
ASSISTANT DIRECTOR JONES: And I think, you know, recalculating some of the numbers, based on your suggestions, would be very instructive. But I guess, Tony, we've sort of, you know -- the experts looked at it --
PANEL MEMBER FUCALORO: Sure. Yeah.
ASSISTANT DIRECTOR JONES: -- and were not dissatisfied with our simplified approach --
PANEL MEMBER FUCALORO: Right.

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ASSISTANT DIRECTOR JONES: -- in prioritizing those compounds that we have concerns about.
PANEL MEMBER GLANTZ: Well, you know, and it may be that, when you do it with a panel suggesting, it won't change things wildly, although Paul seems to think it will.
PANEL MEMBER BLANC: Well --
PANEL MEMBER GLANTZ: But that's something -- I mean that's an exercise that you can do that should be pretty straightforward. And that's why I suggest that we bring it back at the next meeting. My concern, to just not bang a dead horse, though, is that we're doing one every 7 years. And that, to me, is the bigger problem that you've got this huge list of chemicals.
And at the rate we're going, you know, we're all going to be dead before you get down to Number 10.
ASSISTANT DIRECTOR JONES: Well, we'd like to get --
CHAIRMAN FROINES: Can I -- can I --
ASSISTANT DIRECTOR JONES: We'd like to get 3 --
PANEL MEMBER FUCALORO: Did you say you'd like to see that happen?

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(Laughter.)
ASSISTANT DIRECTOR JONES: We would like to see the 3 documents that we have prepared to come before the panel deliberated on and move on.
CHAIRMAN FROINES: But what are those three documents that are ready to --
ASSISTANT DIRECTOR JONES: Azinphos-methyl, molinate. And chlorpyrifos has not yet had leads assigned, but that's the third document.
CHAIRMAN FROINES: Which? Are they all 3 ready to come to the panel?
ASSISTANT DIRECTOR JONES: Azinphos -- we need feedback from you and whoever the other lead is. Molinate. Molinate is ready to come back to the panel. And chlorpyrifos still needs leads assigned to work with us and complete that. But all three have had deliberations and public meetings.

CHAIRMAN FROINES: I think the azinphos-methyl compound needs to be reviewed, in a sense, because I don't want a compound coming before us that -- I don't want compounds coming before us that we're not going to recommend them as toxic air contaminants.

In other words, we don't want -- if you have a compound that doesn't meet criteria for coming as a TAC, I wouldn't bring it before the panel

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if we're going to then say, "We don't think this is a TAC."

So azinphos-methyl is a problem substance, I think, at this point, given the letter that I got yesterday.

PANEL MEMBER BLANC: I wasn't sure. So the answer was you thought it would be feasible to, at our July meeting, to bring this modified document back?

ASSISTANT DIRECTOR JONES: Yes.

PANEL MEMBER FUCALORO: I can see it.

ASSISTANT DIRECTOR JONES: And, John, I think, given your interest in discussing Paul Goslin's letter to you, Randy and I'll go back and talk to Paul about your wanting to discuss that at the next meeting because your question is salient. Randy and I had a further discussion with Paul about that very issue.

CHAIRMAN FROINES: About which issue? I'm sorry.

ASSISTANT DIRECTOR JONES: About whether or not we bring TACs before the panel that may not meet the criteria of being listed under DPR's regulations.

And I think one of the things that perhaps is not clear in Paul's letter but which he

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articulated us is that he greatly values the peer review of this panel of RELs for pesticides because those have utility outside of their being listed as a toxic air contaminant. But I will tell Paul that --

CHAIRMAN FROINES: Well, no. I think that --

ASSISTANT DIRECTOR JONES: -- that in terms of discussing his letter, I think that will be an important --

CHAIRMAN FROINES: Don't misunderstand. Don't misunderstand. This panel took up, reviewed MTBE for the Air Resources Board. And so we have a history of taking up things that are not necessarily in this -- you know. And that's fine. I think that's quite reasonable.

My only concern is that, some years ago, for example, we took up ethylene dibromide, which was at that point not being used to -- not in the from the DPR, but from the standpoint of as a TAC with ARB.

And so there was -- the panel at that time was -- got frustrated because they felt as though they were spending a lot of time on something

for which there was virtually no human exposure and there was a frustration about that. So it's those kinds of considerations.

I think it's -- in the end, it is your decision which chemicals come to this panel. It is not the panel's decision. It's your decision. So I would say unequivocally that we would defer to your judgment on those matters. It's not up to us to define your priorities. It's up to you. And we'll then -- we'll respond accordingly.

PANEL MEMBER GLANTZ: But, at the same time -- I mean the role we've played historically has been to assist them and also ARB in coming up with a scientifically defendable rational set of priorities.

Because I remember -- the whole thing that got the priority -- I mean, again, I was the one who started this longer ago than I want to admit -- and it was when, at one point, ARB was talking about coke oven emissions to this panel. And there are no coke ovens in California. Remember?

So we said, "Why are we bothering?" It's, like, bad if you live next to a coke oven in Pennsylvania.

ASSISTANT DIRECTOR JONES: No. And we would agree. We would agree wholeheartedly. We would like your review of pesticides that have concerns to the State.

CHAIRMAN FROINES: I think that the -- at this point, just to summarize -- one point, of course, that's been mentioned a number of times, which is looking at a multiplicative approach. The second comment was "Can we identify different approaches to acute toxicity over the LC50?"

The third point is I think that the panel would not be very happy about a priority scheme that list -- that had reproductive and carcinogenicity as a 0-to-2 ranking. That seems to underestimate the importance of the ranking. And, in fact, I think the panel would, in general, argue that toxicity is a major -- should be a major defining feature.

And so, clearly, carcinogenicity and reproductive toxicity are elements of significance. There is Roger Atkinson's point about the Henry's law constant versus vapor pressure. And obviously the issue of use is a key one in terms of looking at compounds relative to whether or not there are large amounts being used.

So I think those are the -- those are the issues that are -- that we've talked about. Am I missing something here?

DR. SEGAWA: Just to make sure I understand the panel's wish is that you would like to see that

the individual toxicity categories -- that is, acute,

NOEL, repro, and carcinogen -- all weighted equally; is that correct?

And that use would be used as a multiplicative factor to toxicity. It was unclear to me whether the volatility, vapor pressure, or whatever we choose is a multiplicative --

PANEL MEMBER BLANC: Exposure. Exposure --

PANEL MEMBER FUCALORO: Exposure.

DR. SEGAWA: Oh, exposure. Okay. I'm sorry.

PANEL MEMBER BLANC: -- which includes both use and volatility.

DR. SEGAWA: Oh, I see.

CHAIRMAN FROINES: The toxicity is the centerpiece, and the coefficient or multiplication is the exposure.

PANEL MEMBER BLANC: And that there be two sets of rankings -- exposure-unweighted and an exposure-weighted ranking.

DR. SEGAWA: Okay.

PANEL MEMBER BYUS: And I would just like to add -- I think, with the HAPs and about the other compounds we have already listed as TACs -- you should run all of them, or at least as many of them as you can, through your prioritization scheme, as

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well, especially things we know a lot about because at least we can see where they fall.

PANEL MEMBER FUCALORO: They'll be markers -- markers in the list.

PANEL MEMBER BYUS: They'll be markers. Or any other ones that you --

DR. SEGAWA: We actually went through that exercise. They just didn't appear since they weren't candidates.

PANEL MEMBER BYUS: Okay.

DR. SEGAWA: That was one of the reasons why we felt that the proposed prioritization was actually pretty good because things like methyl bromide, dichloropropene, formaldehyde -- they all came up high in those rankings.

PANEL MEMBER BYUS: But I want to ask you why --

CHAIRMAN FROINES: Well, there are some ones back here that I sure as hell wouldn't put up high. But I think you mean the ones that really we do know are problems.

DR. SEGAWA: Right.

CHAIRMAN FROINES: Right. So I think that the January meeting -- I think we may have given you a false impression. And I apologize if that was the

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case. But I don't want to say that -- think that you didn't follow our direction. But there clearly was some level of misunderstanding. And to the degree that we are part of that, we apologize.

But I think that is a clear statement -- a clear statement of, I think, where we

are, would be at this point.
I don't think -- I agree with Stan. I actually think that we are not far from coming to a place where we need to come up -- where the final document would be acceptable. I don't think it's a major effort at this point. It's easy for me to say, you know.
PANEL MEMBER GLANTZ: I agree.
CHAIRMAN FROINES: I think we're tinkering.
PANEL MEMBER BYUS: It remains to be seen how much we're tinkering.
CHAIRMAN FROINES: No. I mean --
PANEL MEMBER BYUS: Well, we spent -- for example, Dr. Fucaloro, Dr. Atkinson, and I spent -- what? -- 2 hours, 2-and-a-half hours with OEHHA on their prioritization scheme. I mean it was a very intense discussion. I think we made -- we basically said the same things that were said here today. I mean it's --

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PANEL MEMBER GLANTZ: Now, you might actually want to take a look -- you know, we went through this --
PANEL MEMBER BYUS: I remember the -- I brought up Stan's old prioritization document and how much time you had spent on it.
PANEL MEMBER GLANTZ: You know, you might want to look at that. And the other one that we -- where we just went through this about a year ago was the thing on the -- I forget the bill, but the kids' exposure where we had all these same problems and ended up, I thought, with a pretty good document.
So you just might want to take a look at that too. I'm sure Melanie will be happy to give you a copy -- 50 copies.
CHAIRMAN FROINES: I think that the one point I didn't mention in my review was the comment somebody made about two 8's adding up to 16 gave too much emphasis on exposure. But we've talked about that much more terms in of the multiplicative point. So I think it was, in a sense, covered under that.
So at this point I think we're going to quit for the day.
PANEL MEMBER BLANC: Do you have any discussion about the date, the July date, Peter?

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PANEL MEMBER FUCALORO: Do you have any responses?
CHAIRMAN FROINES: July 22, 23, or 26.
PANEL MEMBER FUCALORO: Yes.
PANEL MEMBER GLANTZ: Let's decide.
CHAIRMAN FROINES: Riverside or Ontario.
PANEL MEMBER FUCALORO: I don't care. Riverside.
CHAIRMAN FROINES: I don't think we should --
PANEL MEMBER GLANTZ: The only -- the only -- with all due respect to our colleagues in

Riverside --
PANEL MEMBER BYUS: Riverside in July is delightful for the Air Resources Board.
PANEL MEMBER GLANTZ: I was just in Riverside on Monday for a meeting. And the problem is there's no flights out of San Francisco.
PANEL MEMBER BYUS: I know.
CHAIRMAN FROINES: You San Francisco people -- we have bent --
PANEL MEMBER GLANTZ: Okay. All right.
CHAIRMAN FROINES: -- over backwards so many times to accommodate your --
PANEL MEMBER GLANTZ: All right. This is a nice place. Don't you want to come to L.A.?

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PANEL MEMBER BYUS: No.
PANEL MEMBER FUCALORO: I really don't care. I mean, if it's Southern California, any of those dates are fine with me.
CHAIRMAN FROINES: Well, for people -- having it a little west of Riverside obviously is a benefit for those of us who come from this side of town but --
PANEL MEMBER BYUS: So Ontario is better.
CHAIRMAN FROINES: So if we can have it near the airport, that would seem --
PANEL MEMBER BYUS: In the airport.
PANEL MEMBER BLANC: What were the dates again?
CHAIRMAN FROINES: July 23, 22, and 26.
PANEL MEMBER GLANTZ: How about Burbank?
PANEL MEMBER FUCALORO: The problem with Ontario, I think, is that there are no direct flights from San Francisco.
PANEL MEMBER BLANC: That is correct. No. That is correct. There are none.
CHAIRMAN FROINES: Well, what about having it near Burbank? But that's, again, we --
PANEL MEMBER BYUS: Burbank.
PANEL MEMBER GLANTZ: Is that the worst of all

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possible worlds?
PANEL MEMBER BLANC: No. Burbank is fine.
PANEL MEMBER GLANTZ: There are flights from San Francisco.
PANEL MEMBER FUCALORO: I'd rather be out here. You gotta be careful of Blanc because he has friends. He likes to go and visit all these places.
PANEL MEMBER BLANC: I would say that Monday or Fridays are, in general, you know, better than the middle of the week, from my point of view.
CHAIRMAN FROINES: Peter, did you give me this, expecting people were going to give a yea, nay on the date?
PANEL MEMBER GLANTZ: Well, why don't we? Let's do it. We're almost all here.
CHAIRMAN FROINES: Can we do it?

PANEL MEMBER FUCALORO: I have no preference on that. I'm not going to vote.
CHAIRMAN FROINES: Can we do it?
PANEL MEMBER ATKINSON: Yeah.
CHAIRMAN FROINES: Gary?
PANEL MEMBER FRIEDMAN: (No audible response.)
PANEL MEMBER BYUS: I don't know. Any of the dates are fine with me right now.
PANEL MEMBER FUCALORO: I don't work. I'm a professor.
CHAIRMAN FROINES: Paul?
PANEL MEMBER BYUS: You don't want that on the record.
PANEL MEMBER BLANC: How about the 26th?
PANEL MEMBER GLANTZ: Is that Friday?
PANEL MEMBER BLANC: Yeah.
PANEL MEMBER GLANTZ: Okay. That's fine with me.
CHAIRMAN FROINES: 26th is okay with you. 26th is okay with you. 26th is okay with you and you.
PANEL MEMBER FUCALORO: 7-26? Where?
PANEL MEMBER BYUS: We don't know yet.
CHAIRMAN FROINES: We don't know yet.
PANEL MEMBER FUCALORO: Place to be determined.
PANEL MEMBER GLANTZ: If you could make it someplace that, you know, there are flights out of San Francisco.
PANEL MEMBER BLANC: There are flights to Cabo that are nonstop.
PANEL MEMBER GLANTZ: Huh?
PANEL MEMBER BLANC: There are flights to Cabo that are nonstop.
PANEL MEMBER GLANTZ: To where?
PANEL MEMBER BLANC: Cabo San Lucas.
PANEL MEMBER FUCALORO: Forget this guy.
PANEL MEMBER GLANTZ: How about the Owani hotel? That would be good.
CHAIRMAN FROINES: I would like to have a meeting in Monterey sometime.
PANEL MEMBER FUCALORO: And I would like to have a meeting in Palermo. I mean who cares where you'd like to have a meeting?
PANEL MEMBER BYUS: I always voted for Lake Tahoe.
CHAIRMAN FROINES: Tahoe's good too.
PANEL MEMBER BYUS: In the winter.
PANEL MEMBER GLANTZ: Hawaii?
PANEL MEMBER BLANC: That would be --
CHAIRMAN FROINES: Can we -- sit down. May I have a motion to adjourn?
PANEL MEMBER GLANTZ: So moved.
PANEL MEMBER BLANC: Second.
CHAIRMAN FROINES: All in favor?

("Ayes.")
CHAIRMAN FROINES: It's unanimous.
(Proceedings concluded at 2:41 P.M.)

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STATE OF CALIFORNIA)
) ss.
COUNTY OF LOS ANGELES)

I, NEALY KENDRICK, CSR No. 11265, do hereby certify:

That the foregoing transcript of proceedings was taken before me at the time and place therein set forth and thereafter transcribed by computer under my direction and supervision, and I hereby certify the foregoing transcript of proceedings is a full, true, and correct transcript of the proceedings.

I further certify that I am neither counsel for nor related to any party to said action nor in anywise interested in the outcome thereof.

IN WITNESS WHEREOF, I have hereunto subscribed my name this 5th day of May, 2002.

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NEALY KENDRICK, CSR NO. 11265