FIFRA SCIENTIFIC ADVISORY PANEL (SAP)

OPEN MEETING

SEPTEMBER 9 - 10, 2004

FUMIGANT BYSTANDER EXPOSURE MODEL REVIEW:
SOIL FUMIGANT EXPOSURE ASSESSMENT SYSTEM (SOFEA)
USING TELONE AS A CASE STUDY

FRIDAY, SEPTEMBER 10, 2004

VOLUME II OF II

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DR. HEERINGA: Good morning, everyone. And welcome to the second day of our two day FIFRA Scientific Advisory Panel meeting on the topic of Fumigant Bystander Exposure Models. This is a two-day meeting focusing on the Soil Fumigant Exposure Assessment System, acronym, SOFEA, using telone as a case study.

I'm Steve Heeringa. I'm the chair for this two-day meeting of the FIFRA SAP. I'm a biostatistician with the University of Michigan's Institute for Social Research. My specialty is in the design of research for population based studies.

I have fortunately on this panel substantially more expertise on the specific topic of interest and on exposure modeling. And I would like these individuals to begin to introduce themselves.

I will begin with Dr. Handwerger.

DR. HANDWERGER: My name is Stuart Handwerger. I'm not one of those with much expertise on this subject. I'm a molecular and developmental endocrinologist in the department of pediatrics and cell biology at the University of Cincinnati and at the

Childrens Hospital in Cincinnati.

I'm primarily interested in the molecular mechanisms involved in human fetal growth.

DR. ARYA: I'm Pal Arya. I'm a professor of meteorology at North Carolina State University in Raleigh. And my areas of interest are micro meteorology, atmospheric boundary layer, air pollution meteorology and short range dispersion.

DR. SPICER: My name is Tom Spicer. I'm professor and head of chemical engineering at the University of Arkansas. My research interests are in short term atmospheric dispersion.

DR. HANNA: I'm Adel Hanna. I am a research professor at the University of North Carolina, Chapel Hill. My area is air quality and meteorological modeling and analysis.

DR. MACDONALD: Peter Macdonald, professor of mathematics and statistics at McMaster University in Canada with general expertise in applied statistics.

DR. SHOKES: Fred Shokes, Director of the Tidewater Agricultural Research and Extension Center. I work for Virginia Tech, and I'm a plant pathologist by trade.

DR. BARTLETT: Paul Bartlett, Queens College, City University of New York. I work in the area of air transport environmental fate and deposition of semivolatiles, regional and long range.

DR. GOUVEIA: Frank Gouveia from Lawrence Livermore National Laboratory. I'm a meteorologist involved with micro monitoring, regulatory monitoring, ISC modeling and other types of modeling.

DR. COHEN: Mark Cohen from the NOAA Air Resources Laboratory in Silver Spring, Maryland. I'm an atmospheric scientist specializing in modeling of atmospheric toxics.

DR. POTTER: Tom Potter, USDA/ARS Southeast Watershed Laboratory in Tifton, Georgia. I'm a research chemist conducting work investigating pesticide fate and transport and exposure assessment.

DR. WINEGAR: I'm Eric Winegar, Principal at Applied Measurement Science. I do monitoring and measurements, analytical chemistry and exposure assessments.

DR. OU: Li Ou. I'm a soil microbiologist with University of Florida. My major interest is the biodegradation of organic chemicals.

DR. MAJEWSKI: I am Mike Majewski, I'm a research chemist with the U.S. Geological Survey. I study the atmospheric transport and fate of organic chemicals.

DR. YATES: I'm Scott Yates. I'm a research soil physicist with USDA Agricultural Research Service in Riverside, California. The area of my interest is fate and transport of pesticides and soils, modeling and volatilization into the atmosphere.

DR. MAXWELL: Good morning. I am Dave Maxwell, air quality meteorologist with the National Park Service in Denver, Colorado. My interest areas are air quality permitting, air dispersion modeling, and air quality modeling.

DR. HEERINGA: Thank you, panel members. Again, I want to express my appreciation to the EPA for pulling together such a diverse and highly qualified group of individuals to address this particular topic.

At this point in time I would like to turn the mic over to Mr. Joseph Bailey, who is the Designated Federal Official for this two-day meeting of the FIFRA SAP.

MR. BAILEY: Thank you, Dr. Heeringa. I'm Joe Bailey with the EPA's Office of Science Coordination and Policy. I'm the Designated Federal Official.

I just wanted to just make a very brief announcement, a reminder of a couple of things. The meeting -- this is a public meeting. It is being recorded.

We do have a public docket available that will contain all of the background materials presented at the meeting and materials that were presented to the panel in preparation for the meeting.

The docket will also contain the report meeting minutes, which we expect to have completed in about eight weeks after this meeting concludes.

And I do want to thank everybody for being here again today, and welcome. Thank you.

DR. HEERINGA: Thank you, Joe. At this point in time I think I would like to begin the morning program with Mr. Jeffrey Dawson of the Office of Pesticide Programs.

Jeff, I don't know if you have any follow up from yesterday's session or any other comments. If you could also introduce your colleagues with you.

MR. DAWSON: Good morning, everyone. And thank you for an excellent discussion yesterday. We really appreciate the effort that you all are making.

On my left is Dr. Bruce Johnson from the California Department of Pesticide Regulation. He has been intimately involved for several years with their scientific and regulatory processes related to soil fumigants, especially in the areas of volatilization and modeling.

On my right is Mr. Michael Metzger, who is a Branch Chief in the Health Effects Division of the Office of Pesticides. Basically, my boss.

And then the three individuals here are from Dow Agrosciences, the developer of SOFEA. And Dr. Steve Cryer, Dr. Ian Van Wesenbeeck and Mr. Bruise Houtman.

DR. HEERINGA: Thank you very much.

MR. DAWSON: We did have just one kind of clarifying question I think we walked away with yesterday and we were kind of thinking about it over the evening.

And basically, it has to do with the methodologies used to calculate flux rates.

We heard a lot of discussion yesterday about the different methodologies, the back calculation method and the aerodynamic flux method and the direct monitoring method.

And we were wondering if the panel could potentially clarify in the context of whether or not they have an inherent over or underestimate of flux enhanced exposures for each of the methods, or is there a preference as to which one of those should more routinely be utilized.

DR. HEERINGA: Thank you. We'll turn that question to the panel. It is early, first thing. Is there somebody who would like to address that at this point?

Yes, Dr. Arya.

DR. ARYA: I think among the methods mentioned, I think that aerodynamic method is the best practical method available.

Certainly, there is a direct method, eddy correlation, eddy covariance method. But I doubt we have any instrument or probe measuring -- which can measure rapid or high frequency fluctuations of concentrations.

So in the absence of that, I think the aerodynamic is the best available method. And I don't think there is -- one can say that it underestimates or overestimates. I know there may be some errors associated with it, maybe 20 percent or so.

DR. HEERINGA: Yes, Dr. Yates.

DR. YATES: I don't have any personal experience with the back calculation method. But with the other -- direct methods for estimating flux that would include aerodynamic, integrated horizontal flux, theoretical profile shape, flux chambers, in terms of bias, one way to -- one indication of bias would be if they give different cumulative fluxes.

And from my experience, when an experiment works out well and you don't have some kind of experimental problem, the cumulative flux from all the different methods are basically the same which indicates to me that the methods aren't bias.

In terms of period flux, there is a wide range -- even with using the same data set in the different methods to estimate the flux, you get a wide range in the period flux.

At the last panel meeting we had, I showed a slide where from one experiment we obtained some data and used aerodynamic theoretical profile shape integrated horizontal flux and flux chambers. And the range at one particular period in the experiment was tremendous.

In terms of bias for the period flux, that's something I really can't say anything about. But it would seem that if the cumulative fluxes are the same for all the different methods, you wouldn't expect that there would be a bias.

But the period flux is, for at least acute exposure, would be more important. And that question, I think, is still unanswered.

DR. HEERINGA: Dr. Yates, I wonder if we could include that figure in the proceedings from this two-day session as well.

DR. YATES: Certainly.

DR. HEERINGA: Yes, Mr. Gouveia.

DR. GOUVEIA: There was some talk yesterday about the aerodynamic method and maybe some inaccuracies at night, the low flux time.

I noticed that you used naturally aspirated shields for the temperature sensors. Is that right?

DR. VAN WESENBEECK: They were thermal couples.

DR. GOUVEIA: Thermal couple sensors. But the shields themselves, the housing for the shields, were they naturally aspirated or forced aspirated with fans?

DR. VAN WESENBEECK: They are all naturally aspirated.

DR. GOUVEIA: Yes, that's typical for a field experiment at night with the low wind conditions and, I presume, clear skies, which is typical for Kern County. It was taking place in Kern County. Right?

DR. VAN WESENBEECK: Salinas.

DR. GOUVEIA: Salinas. Clear skies. Those naturally aspirated shields are notoriously inaccurate. Not the sensor itself, but the shield becomes too cold. It could bias the profile.

And Dr. Arya might expound on that. But there might be some problem, inaccuracies at the low end during the nighttime measurements.

DR. HEERINGA: And the alternative is?

DR. GOUVEIA: The alternative would be what I suggested yesterday, what Dr. Arya suggested, eddy correlation methods, direct measurements of the eddy correlation of momentum.

Isn't that right, Dr. Arya?

DR. ARYA: One can use that method for momentum flux and heat flux. But I'm not sure that if you can do that for the flux of 1,3-D, for example.

I'm not aware that they have any fast response instrument to measure the concentration fluctuations higher than one hertz.

DR. GOUVEIA: Would the eddy correlation measurement of 3D anemometer, thermal coupled instrument help with the calculation of a Richardson number?

DR. ARYA: Well, the Richardson number, of course, we can calculate just from the gradient measurements, you know, mean wind, mean temperature. You don't need eddy correlation measurements for Richardson number.

But eddy correlation measurements are made to measure flux directly, flux momentum, heat and mass of chemical.

DR. HEERINGA: Dr. Winegar.

DR. WINEGAR: I have seen reference to a relaxed eddy correlation technique, which, as I recall, obviates the requirement for a fast response sensor.

I really don't know much about that other than that short description. Can you comment whether that would be an alternative to the difficulties of the eddy correlation method?

DR. ARYA: Well, there are some methods like eddy accumulation or relaxed eddy correlation methods. But you still need instruments which can measure the concentration fluctuations. You cannot depend on something sample collected and analyzed in the laboratory.

DR. HEERINGA: Dr. Majewski and then Dr. Cohen.

DR. MAJEWSKI: With the eddy accumulation method or any single height measurement, if you lose a sample, then the data for that period is gone. What I like about the aerodynamic gradient method is that you have five or six data points with height.

And if you have one bizarre number, something happens to one sample, you can interpolate from the other samples and get the best fit curve there.

Eddy accumulation is nice, but you still need the fast response sensors. And it is an electronic problem and a mechanical problem getting the switches turning or turning on and off that becomes a problem.

DR. ARYA: Yes, I agree with that.

And certainly, even in the gradient method, I think one should make use of all the measurements. If you have measurements at four or five different levels, you don't have to use just the gradient base on the highest two levels.

You should use all the height levels.

DR. MAJEWSKI: In terms of the temperature gradient, one thing that we did early on was used, and this is going back, I have to get rid of some cobwebs here, but it is -- like a thermal pile or something. It is aspirated and it is measuring temperature difference between the two heights.

Measures that directly instead of taking the temperature gradient and measures the temperature difference directly.

DR. ROBERTS: Dr. Cohen and then Dr. Yates.

DR. COHEN: Some of my colleagues at the NOAA Air Resources Laboratory have worked on the relaxed eddy accumulation method. In that method you don't need the fast response sensor.

You are just switching the airflow to one filter or another depending on which way the turbulent eddies are going. So you don't need to actually measure the eddies at that time.

You are just collecting all the downward moving eddies on a filter and all the upward moving eddies on a filter. So you can -- it is relaxed, meaning you could do it like over two hours or something.

They have had some success with some compounds with ammonia, I think they have worked with. But it is I guess perhaps more of a research type of technique at this point.

But it could be used potentially for these compounds where there isn't any possibility of getting a second on -- a measurement on the order of hertz frequencies, which you need.

DR. HEERINGA: Dr. Yates.

DR. YATES: I would like a little bit of clarification with respect to the thermal couple.

You had a thermal couple inside of a shield? And was it a fine wire thermal couple or just a --

DR. VAN WESENBEECK: I believe it was a copper constant (ph) thermal couple.

DR. YATES: Because I know Campbell Scientific sells a fine wire thermal couple that they have done some studies and they actually find that you don't really want to put it in anything under a shield. The wire is so thin that, you know, sun hitting it, it doesn't really change the temperature of the thermal couple.

They have a way where you can wire it that you get a direct measurement of the gradient. So you don't have any problem like with offsets or, you know, -- if you have two measurements, there could be a little bit of a bias between the two.

This gets rid of that. You can make very accurate measurements of the gradient with these things.

So what we have done in our experiments we will have three replicates of the thermal couples. We just leave them out in the air stream and then we don't have to worry about aspiration and what the shield might be doing to the sensor.

DR. HEERINGA: Are there any other questions or comments, excuse me, from the panel on this subject?

Dr. Arya and then Dr. Shokes.

DR. ARYA: Regarding the aerodynamic method, I think one important thing I would like to emphasize is that the gradient measure, gradient of concentration should also be measured on average over a period of an hour or so rather than very long periods, six hours or 12 hours.

Because the equations they are using for gradient method, you know, those so-called flux gradient relations (ph), they are based on kind of hourly average measurements.

DR. HEERINGA: This was a point raised yesterday also. Thanks. Dr. Shokes.

DR. SHOKES: I have a question that's a little different from the actual sampling. It goes back to fundamental consideration.

We're looking at a model here that is designed to measure chronic exposure. We looked at two other models a few weeks ago, looked at acute or the high end. And I would like to just address this really to the agency and anyone else that might be qualified to speak to the health effects of that, the importance of this type of model versus the other, the chronic versus the acute exposure and do you need two models?

Do you need to look at both of these aspects with a given fumigant and under what conditions would that be necessary? This is kind of a fundamental question, but

I think as we look at this, it is very different from the other models.

And I think that's a very important question to clarify. And I need to get it clear in my mind as we evaluate this.

DR. HEERINGA: Mr. Dawson, are you able to address that?

MR. DAWSON: Yes. I can address that.

Let's focus on this model first. It was our understanding that -- Dr. Shokes is exactly correct, that historically this is a model or a methodology, whatever you want to call it, that has been developed with the focus on longer duration exposures.

Because for the case study chemical, 1,3-D, historically, that's been the durations of exposures that have been of concern from the regulatory perspective.

Recently, we have had a number of discussions with the developers as has other entities. And they incorporated the capabilities to -- whatever they are at this point, to address the shorter term exposure.

So we're interested in your evaluation of both elements of this. Because I guess we view this as potentially a viable choice for all durations of exposure.

As far as the health effects component, the way we routinely do our risk assessment processes, we evaluate all types of durations of exposure.

So, for example, when we do our toxicological evaluations for various chemicals, we're going to evaluate it based on studies, on toxicology studies that range the gamut from those that could be used to represent acute exposures, all the way through chronic exposures with all sorts of subchronic durations in between.

So that's basically how we define the durations of concern.

For the fumigants, most people think about the shorter term exposures. In a way, that's been our focus as we go through the risk assessment process for many of them.

But for others, we are concerned about the longer term exposures. Even the ones where you think that there are -- you know, the shorter term issues are the ones that are going to drive it. But we're still looking at all those other durations of exposure in our assessment.

That's routinely how we approach all the different kinds of cases that we look at.

DR. HEERINGA: Dr. Shokes, are you --

DR. SHOKES: That's fine.

DR. HEERINGA: Any additional comments from the panel on the measurement of flux or establishment of the flux profile?

I think there is a strong consensus on this panel that the aerodynamic method is among the better options among those that we have considered at least as you have employed it. And there clearly are some issues around technical measurement and calibration that are involved.

Mr. Dawson, do you think that response was satisfactory?

MR. DAWSON: Yes. Thank you.

DR. HEERINGA: Any additional questions from material that we covered yesterday or responses to questions 1 through 3?

MR. DAWSON: No, I think that was the one major clarification.

DR. HEERINGA: We'll have a chance to take up Dr. Shokes' issue later on in one of the other questions, too, I think.

At this point then I guess I would like to continue on with the charge questions, question Number 4. And maybe ask, Jeff, if you would be willing to read this into the record, please.

MR. DAWSON: Question 4. The integration of meteorological data into ISCST 3 is one of the key components that separates the SOFEA methodology from that being employed by the agency in its current assessment.

This information coupled with GIS or Geographical Information Systems data such as the amount of ag capable land cover, elevation and population densities are optional inputs for SOFEA.

Sub part A. Can the panel comment on the value of adding this information for conducting spatially realistic simulations.

Sub part B. There are several potential sources of meteorological and GIS data, for example, in National Weather Service or CIMIS or the California Irrigation Management Information System. Please comment on the methods used to select these data including locations for meteorological stations.

Sub part C. What criteria should be used to identify airsheds for analysis and how should data be selected to address each airshed? Please comment on the manner in which these data are processed.

Sub part D. Data quality and uncertainty associated with these data vary with the source. Does the panel agree with the approaches used to characterize these factors.

Sub part E. Anemometer sampling height has been identified as a concern by the agency in preparation for this meeting. What are the potential impacts of using data collected with different anemometer heights in an analysis of this nature?

Sub part F. Does SOFEA treat meteorological stability class inputs appropriately?

And finally, sub part G. Does SOFEA appropriately calculate bounding air concentration estimates.

DR. MAXWELL: This is Dave Maxwell. What do you mean by bounding air?

MR. DAWSON: We're interested in ascertaining whether or not the way that the meteorological data are used is appropriate when we're looking at exposure concentrations in the high percentiles of exposure.

DR. HEERINGA: So these would be probability bounds on the flux distributions, concentration distributions.

MR. DAWSON: Right.

DR. HEERINGA: I would like to turn to our lead discussant for this question, which is Dr. Arya.

DR. ARYA: Thank you. I would like to certainly try to make some comments and answer to some extent. The question is very long, of course. And I think -- going through it, I realized that many of the points raised probably have been discussed quite adequately in connection with other questions too.

But I think this question has to do mostly with the meteorological information, that's how I took it and how -- value of adding this information for conducting spatially realistic simulation.

In my opinion, the meteorological data, meteorological information provided in SOFEA is really what is required to run the ISCST.

And that is vital information, certainly. It is certainly actual meteorological data, hour to hour data, they are important. Especially, if they are available for some station within the domain or nearby station, meteorological station.

It is much more important to have that information, hourly data, rather than using the EPA's current approach of sort of using, you know, 24 hour same wind speed, same wind direction, same stability.

Certainly, I think that is not really appropriate or consistent with the ISCST. Those stability categories, they are used to define the dispersion coefficients.

And the dispersion coefficients used in the

ISCST are based on Pascal Gifford (ph) curves. They were developed based on the experimental diffusion data that's short range and also kind of short term average data.

Also, so they are applicable to original data where really three, 10 minute averages. But they have been routinely used for one hour averages.

But one should not use them for more than one hour averages. Certainly, not 24 hour.

If you are interested in 24 hour average, you calculate the concentration for each hour, then make the 24 hour average calculation from those.

So I think this meteorological information, hour to hour information on wind speed, wind direction and also stability is a very important compound.

Coming to B, mentioned there are several important sources, of course, of meteorological data like National Weather Service. You mentioned this California Irrigation Management Information Service.

You know, those states you may have similar data like North Carolina. There is a state climate office. They maintain a number of stations across the state where they collect the weather data.

So one can use those kinds of -- if one of those stations happen to be in the region of interest or near the region of interest, one can use those. Otherwise, the nearest National Weather Service Station is appropriate.

I'm not sure of the comment on the method used to select these data including locations. I believe that in SOFEA, you know, you probably -- many try to use the nearest available service station that I know whether it is from National Weather Service or from this CIMIS network. I think that's the main criteria to be used.

If there are more than one station available and which are appropriate for the area of interest where you are making the model calculations, then one can use maybe the average of the two stations.

Because the model certainly is not designed to accept more than one set of information, actually.

C, what criteria should be used to identify airsheds. I think we had this for analysis and how should the data be selected. We had lengthy discussion on the airshed, the concept of airshed yesterday also.

In my opinion, the criteria should be based on the receptors of interest. Probably the model domain I mentioned to be used should depend on the receptors of interest.

If you are considering the exposure in a certain town, then one should consider, you know, the model domain, where the town is located in the center and all the fields treated are surrounding.

And certainly, the airshed, again, how large it should be. Probably the criteria should also be based on the limitations of the model you are using.

I emphasized that point yesterday too, that in no case one should actually have a model domain too large so that you have to calculate the concentrations more than 100 kilometers downwind of any source.

So I think the maximum distance within the source and receptor should not be more than 100 kilometers.

In fact, the dispersion curves that are given, they don't extend beyond 100 kilometers. And actual data on which they were based, those experimental data did not extend beyond 40 or 50 kilometers.

Even now they are kind of used in at least twice as large as the original data were, diffusion data were available at that time.

In D part, data quality and uncertainty associated with these data vary with the source. Of course, we know the meteorological data certainly there are uncertainties associated with the mean wind speed, mean wind direction.

Now do we agree with the approach used to characterize these factors. I don't know if the model actually does not consider those uncertainties.

You know, they simply take the mean values, wind speed, wind direction, and stability based on other measurements.

So even though we know that there are -- and there may be other variables too like the flux certainly are uncertain. But certainly in the meteorological data I don't know of any way -- the model doesn't consider the uncertainties involved in those measurements.

So I don't know what this question implies. Do we agree with the approach used to characterize these factors? I don't think SOFEA is using an approach to characterize those uncertainties, the meteorological data.

E part has to do with anemometer sampling height. This has been identified as a concern by the agency, what are the potential impacts of using data collected with different anemometer heights.

Of course, in the use of ISCST for calculating stabilities, you need to have information of wind speed at 10 meter height. So even though the data may be available at different heights like some station may have two meter height, there are ways of extrapolating to 10 meter based on some sort of power law profile, wind profile.

So one can I think -- SOFEA -- or ISCST, they do have probably those relations. If you have two meter data, you know, how to calculate the 10 meter wind speed and then stability.

In the calculation of concentration themselves, I still think that for surface sources like the ones we are dealing with here, one should use the kind of standard height of 10 meter, wind speed at 10 meter rather than two meter.

If you use a two meter wind speeds, it's likely to overestimate the concentrations.

In effective wind speed in concentration calculation and any Gaussian model actually is the average wind speed or the height of the plume, you know, the material get mixed.

But in routine use of these regulator models, actually it is not done that way. So they recommend that you use the wind speed at 10 meter height for surface sources and wind speed at the height of the release for elevated sources.

So in this application, if the wind speeds other than 10 meters is measured, then it is appropriate to estimate the wind speed at 10 meter using a power law profile and so on.

And that wind speed should be used for the calculation of concentrations also.

Next, does SOFEA treat meteorological stability classes inputs appropriately? I think it treats appropriately in the way that ISCST model takes those input, because mainly stability classes are used to define what dispersion parameters to use.

And certainly, these stability classes are determined based on measurements of wind speed, cloudiness during the daytime and nighttime, also the intensity of soil insulation (ph) and there are some objective methods used to determine that, whether it is strong or moderate or slight.

So those are actually specified in the model ISCST.

Does SOFEA appropriately calculate the bounding air concentration estimate? That's a loaded question and I don't know whether SOFEA does calculate the bounding -- the upper bound of the concentrations.

That depends on the length of the meteorological data. If you have meteorological data, you use the kind of limited data, say, one year and you don't encounter the worst case conditions leading to those highest concentrations during that year, then you are not likely to get those bounding concentrations.

The longer the meteorological data available, 10, 20 years, you know, more likely you are likely to get those worst case conditions.

Also, depends on the location of the receptors. I think it was pointed out yesterday, you know, if you have receptors at close grid and maybe they don't -- many of them may not be close to the source and may not give very high concentrations.

So to avoid that might be -- you may have kind of two sets of receptors, you know. I would say that one kind of regularly based, grid based receptors and other you can say that near source receptors.

So any treated fields you can have receptors just beyond the zone you define, maybe 30 meters or 50 meters, whatever you specify.

So near surface or near source receptors are certainly likely to give these highest concentrations.

So I think if you locate your receptors, then that way that the closest to the source that is permissible that you can calculate those concentrations. Certainly, you don't want to go very close within few meters, because the model is not really applicable that close.

But if buffer, like if you got a 300 feet buffer, you know, 100 meter, that's within that distance. You can do that.

So I think that will be my comments. Certainly, my colleagues here can add more to this.

DR. HEERINGA: Dr. Spicer, if you are willing to offer your comments in response to this particular question.

DR. SPICER: With regard to -- I'm not necessarily going to follow the A, B, C arrangement, but with regard to the first two questions, is the data useful and important and that sort of thing, I think that, and this has been talked about already, but I think it is important to recognize that even the previous discussion about whether you are talking about acute or chronic exposures even influences how you think about the met data from my point of view.

The reason being that if you are interested in the acute exposures, then you are talking about shorter distances. And it may be possible that the present treatment of the met data gives you a reasonable way of addressing that sort of question.

But it, of course, even ignores the sorts of local conditions that you can have such as drainage flows and that sort of thing.

But once you get into the chronic exposure, and even the level of interest associated with the chronic exposure, whether you are talking about milligrams, micrograms, picograms, that can be important in terms of distances and that sort of thing.

Because, obviously, the longer the distance you have, then the more the meteorological conditions that you have got for a particular station will not be expected to apply to others.

The more you will have terrain effects being important as far as the determination of the concentrations and those sorts of things.

So this acute versus chronic question I believe even gets into the selection of the appropriateness of the met data.

And that even impacts on question C as far as that is concerned with regard to the airsheds. As I pointed yesterday, the township arrangement is understandably a way, an approach, that we started with. But ultimately, though, as far as an airshed is concerned, what you are looking at is impact.

And once again, it is important impact on the people involved. So the levels that you are talking about are extremely important again.

And so the idea of airsheds seems to be kind of in the background at this point and not being directly addressed. There was the sensitivity study done that indicated you can increase the number of townships and early on you start to capture the 50th percentile, later on you start to capture the 95th percentile.

But that still doesn't have to do with things such as larger terrain effects and those sorts of things that can actually channel flows and have those sorts of issues.

They are simply not addressed in this methodology.

I'll skip over question D as I think that there are other people more appropriate to answer that, and even answers and discussion from the first panel may be important to include there.

As far as the sampling height is concerned, I don't believe that that is a particular problem as long as you are talking about measurements below 10 meters. There are standard approaches to make those adjustments and they are accepted. Whether they are 100 percent accurate or not is a separate issue because they are accepted.

As far as question F is concerned, of course the reason for SOFEA to treat the meteorological stability class inputs appropriately is because they are ultimately what is going to be used to make the dispersion calculations.

And even, too, a part of the determination of the fluxes and that sort of thing, because you are really looking at measures of stability and even in the aerodynamic method as far as the flux calculations are concerned.

I guess the point is that hopefully there will be a time when the future models, maybe AERMOD, for example, will not have this sort of issue. And so at present we're stuck with this issue because of the use of the ISC group of models and that standard sort of characterization of the dispersion coefficients.

With regard to the last question, the bounding air concentration estimates, of course it has been previously discussed that ISC has these inherent problems at long term distances. The dispersion coefficients do not apply, this idea of the fact that if you have a change in wind direction, then ISC assumes that you calculate a steady state plume in the wind direction that you are talking about.

The other aspect that has not been mentioned yet is the fact that for longer term distances you have boundary layer depth coming into play.

I don't remember that being discussed. That's an ISC input. And I don't know how that's treated as far as the present case study is done.

As far as the met conditions, I think that with regard to estimating the bounding concentrations, again, things like drainage flows can be important as far as the acute exposures are concerned. Possibly even as far as that's concerned the longer term chronic exposures, for example, if you have a situation where a field would have a directed flow as a consequence of terrain that might not be accounted for associated with the wind direction in another met station.

The other aspect here is in terms of underestimating the exposure, and I think that's what you are trying to get at, the idea that I don't believe is addressed in the model is this situation that you can have at sunset where you go from neutral to stable transitions where the soil can still be warm increasing the evolution rate.

And the net result is that you can essentially build up concentrations over the source that then can be advected around. Of course, that gets into the issue associated with actually capturing that sort of phenomenon with the flux measurements because your concentration averaging times are so long they won't capture that sort of phenomenon.

So once again, it seems that this approach can be underestimating and, therefore, not getting the bounding air concentration because of met considerations.

The other aspect that's been discussed with regard to Gaussian models is the fact that, although the Gaussian models are generally considered to predict the maximum concentration correctly, they are not necessarily considered to predict the location of the maximum concentration very accurately.

So the maximum value is better predicted than the actual location. And so that gets into an issue that is sort of met related in the sense of this idea of using a regular grid to look at the chronic exposures.

And of course, it is ultimately the reason why you chose the finer grid to do the acute exposures. And so that sort of gridding issue does infringe on the met aspects of this bounding concentration idea.

One thing that, and you may have done this and I simply may have overlooked it, is I don't know whether you have done a sensitivity study, and that actually could be part of question eight, as to refining the grid and looking at whether the average values predicted to 95th percentiles are predicted and those sorts of things.

Just for the record, I will include the other two issues that were mentioned as far as the flux and treating it as a stochastic variable and that consequently not reflecting the upper tails of distribution and this idea of using a single source that's supposed to be a worst case set of conditions may -- there may have been competing effects associated with degradation due to temperature that Dr. Ou was pointing out yesterday that may change the flux characteristics at a different location that's unaccounted for at this point.

DR. HEERINGA: Thank you very much. The next discussant is David Maxwell.

DR. MAXWELL: I'm Dave Maxwell. I'm coming at this question from the perspective of an air pollution meteorologist. And under item A, I think, basically, the more applications you have in the model, the better. And I think you have gone through a lot of applications in this model. As long as it is documented and fairly easy to assess and not too cumbersome for a beginning user, I think that's very useful what you have proposed.

Under item B of selecting the meteorology data, the question I have is how are onsite meteorology data compared with long term National Weather Service data. Perhaps that could be explained a little more thoroughly.

How does that role play in when you run the model. Especially, the short term version of it.

Under C, the criteria used for identifying data for the airshed, the major source appropriately for your meteorology data is National Weather Service data.

I know the other fumigant models have proposed using Weather Service data, which has generally been determined to be the best source for many reasons, including it takes data usually at 10 meters elevation.

And also the CIMIS data in California. That's very useful also.

Definitely, the closest quality assured data source should be used for model input. That may not be the Weather Service data or CIMIS data. It could be industrial emissions data and at a 10 meter station there or some other local state or city weather station.

So I just bring that up because that is

sometimes a very useful source if the data are quality assured and may even be better than Weather Service data.

Now, the question with that is do they have at least five years worth of good data too. But for worst case analyses, it may be a good source or a comparison source. And all data sources should be documented.

Under D, data quality and uncertainty associated with meteorology data, evaluating calms, and this would be brought up I believe in the next question, but I thought I would just mention it here, you did discuss that. But my question was how was the idea of applying, I believe it was, the meteorology from a previous year's date and hour derived?

Was that how you replace calms from the same date and hour of the previous year or previous years?

DR. VAN WESENBEECK: I believe that was a description of how we replaced missing data. Not how we dealt with calms.

In the CIMIS data there were intervals on the order of a few hours. Sometimes an entire day that were missing. And then we agreed on an averaging procedure based on the other four years of data within the data set.

DR. MAXWELL: Thank you for clarifying that. I have a follow-up question on that.

Would persistence be -- has that been addressed or would that be an option too where you look at perhaps the previous hour that was a valid hour and use similar meteorology data or the following hour after the missing period.

DR. VAN WESENBEECK: No, we didn't use that approach. But that is another way that I have seen also that people have filled in missing data.

Where I did sort of apply that was just in the stability class to ensure there weren't any abrupt jumps in stability, that there was a smooth transition.

DR. MAXWELL: All right.

Now for E, the impacts of collecting data at different anemometer heights, my colleagues have adequately addressed this.

Definitely, I think there is agreement that 10 meters is the best height and perhaps you do need power laws or some sort of adjustment for going from two meters to 10 meters.

F, about SOFEA treating stability class appropriately, once again, my colleagues, I think, have adequately examined this question. I personally think that stability adjustment equations that you presented were very thorough and addressed that issue.

My question is I believe -- in your presentations yesterday you adjusted stability classes through QA checks. I just wanted to follow up how do you do that? I know during the daytime it's B, C, or D. But how do you go back and say, well, that stability class apparently is not right. How do you change it?

DR. VAN WESENBEECK: Just for clarification, are you referring to the aerodynamic method or a QC of the met data? In terms of the CIMIS weather files, I never changed any stability classes there other than when there was missing data. And we entered it.

DR. MAXWELL: I just thought during your presentation you mentioned that there was some potential adjustment stability classes through QA checks.

If that's not correct, fine.

DR. HEERINGA: Was that in the calibration of the aerodynamic method possibly?

DR. VAN WESENBEECK: The only place where stability -- where there were stability correction factors were in the aerodynamic method.

DR. MAXWELL: Were they based upon going from B to C or C to D or would you jump from B to D, for example, or D to B?

DR. VAN WESENBEECK: No. Those were just based on the temperature and wind gradients measured at the site in order to adjust the Richardsons coefficient.

DR. MAXWELL: Okay, thank you.

And under G, appropriately calculating the bounding air calculation estimates. This may be addressed also in a future question. But I was inquiring about the worst case emission flux, how that's assured with using five years of meteorology data.

DR. VAN WESENBEECK: Well, I think it is a combination of not just having five years of meteorology. Obviously, having 10 or 20 or 100 years would be better.

But we're also varying the application rate, application depth and a lot of other parameters. So that's going to add to some sort of worst case scenario there in our opinion as far as coming up with a bounding estimate.

You know, we'll put in as much good weather data as we can in the future. Dr. Arya mentioned even using two weather stations. A question back for him on that, I assume you don't mean averaging weather, but just putting in two, appending them onto each other and then using that in a simulation?

DR. ARYA: Yes. You can use both data and calculate concentrations using both meteorological stations and then take the average.

DR. VAN WESENBEECK: So average the ultimate concentration distributions, not the weather.

DR. MAXWELL: That's it for me.

DR. HEERINGA: Thank you very much, Dr. Maxwell. At this point the final scheduled discussant is Frank Gouveia.

DR. GOUVEIA: There were a few issues.

Incorporation of ISC should be done with many caveats. I imagine you have already thought of a lot of these. It is, of course, a straight line model like many people have said here. So the actual location of that maximum concentration is in doubt with the straight line model, especially at great distances of several kilometers.

I think you have fixed that problem or accounted for that problem by using a regular space grid and not trying to locate specific receptors at specific locations, XY locations relative to the sources.

And so using that approach, I think in a probabilistic sense, you are capturing the concentrations far downwind.

I would, and I think other panel members have said also, the use of GIS to identify specific locations of sources and specific locations of receptors and population centers might not be appropriate if ISC was used also.

We have also talked about using other models like AERMOD, Cal Puff and even mass consistent models to get a better idea for the wind field than provided by just a straight line model.

Using the straight line model is appropriate if the meteorological data is taken in the same location. I think other panel members have commented on this.

In the Bay area, our regulatory agency suggests a three mile distance. If you don't have a meteorological station within three miles of your source, you should have your own station, an industrial station like Mr. Maxwell suggested.

That might be a little too stringent for agricultural use because the land's relatively flat. But I would imagine a regulatory agent, a regulatory agency, a local ag dispersion regulatory agency might have a suggestion for the distance needed to bring in appropriate meteorological data. Maybe it is tens of kilometers away.

I think question B has been answered completely or to my satisfaction and as well as question C.

Data quality, of course, question D, is very important. And a good thing about going fourth on the panel is a lot of these issues have already been brought up.

Anemometer height as well. And of course, anemometer height -- the appropriate anemometer height depends on the local terrain. I don't know if that was discussed. But in such flat terrain, 10 meters -- agricultural terrain, 10 meters seems more than adequate, and extrapolation of two meter data to 10 meters is appropriate in the flat terrain of agricultural use.

The use of stability class by SOFEA has been really driven by the application of ISC. ISC uses stability classes, Pascal Gifford stability classes. So you are pretty much in that line.

And I imagine if another model was used in SOFEA, AERMOD, for instance, that doesn't necessarily use stability class, uses a more continuous, I believe more continuous measurements of stability, SOFEA could adapt to that new model as well.

Except for the issues that have already been brought up, I think that's it for me. Thank you.

DR. HEERINGA: Thank you very much. At this point I would like to open it up for additional comments from other members of the panel. We'll begin with Dr.

Winegar.

DR. WINEGAR: I just want to make one comment in regards to the inclusion of GIS type of regional data in the model. I welcome that development.

I recall, and I think Dr. Johnson might remember of this instance, too, when we were involved in some methylbromide monitoring a few years back, we were trying to figure out basically regional distributions of that fumigant to the general population.

And it seems like the use of actual emissions data coupled with local met data to predict what a regional distribution might be for ambient air concentrations of a fumigant, it seems like it would be a useful model.

This first step of looking at buffer zones, for example, probably I assume will evolve into looking at regional subchronic type of exposure and such.

So I'm not sure -- to me, it seems like it is an applicable tool here. But at any rate, and don't know if you agree with that, but it seems like it could be a useful tool for those types of assessments.

So I think that -- I applaud the inclusion of this type of data and the generalization to a broader geographical region.

DR. HEERINGA: A question of clarification to the panel, having sat through the presentation or the panels on three of the models.

In a prior panel, there was discussion of the CIMIS data and potentially I think -- I recall the recommendation there was to come down fairly heavily in support of the National Weather Service data, primarily because of -- did CIMIS suffer from lack of measurements at 10 meter heights?

Is that part of the issue or am I confusing that with the Florida data? Does anyone who was here previously recall? Dr. Majewski.

DR. MAJEWSKI: I think CIMIS takes their measurements at two and/or six meters.

DR. HEERINGA: Thank you. I didn't want to confuse the issue. But I just -- ultimately, there will have to be some consistency on some of the factual information across these reports. We'll be sure that as we work together to prepare these meetings, if there are inconsistencies of fact, those will be noted.

Dr. Shokes.

DR. SHOKES: I have a question. We talk about 10 meter data. If we looked at the chronic exposure in a large area, how realistic is that and what happens to what you are measuring at 10 meters. As it gets out further, does it mix in and come back down to lower altitudes? How are people affected by it?

DR. HEERINGA: Dr. Arya.

DR. ARYA: Well, I think as you go further down, say, beyond 20 kilometers or so, essentially, the material is mixed through the whole depth of the boundary layer.

And there then, as Dr. Spicer mentioned, it is a question of whether you have the right mixing height. The model does have -- of course, it will emit the mixing to the mixing height. And I think the mixing height in these models is based on upper air sounding data. Those upper air stations are not that closely spaced as our surface stations are.

Sometimes upper air sounding information may be far away. Several hundred kilometers away. And may not give you the right mixing height. Especially in the coastal area.

In the coastal area, the mixing height varies very strongly. Mixing heights are generally smaller near the coast and increases inward toward the land.

Certainly, the appropriateness of the 10 meter information certainly becomes -- 10 meter wind speed may not be the effective transport velocity at large distances where the factor transfer velocity will be somewhat the average velocity in the mixing layer, really, above (ph) the surface layer.

DR. HEERINGA: Mr. Gouveia.

DR. GOUVEIA: This issue about mixing height is moot almost because since many regulators suggest a constant mixing height when you use ISC. In the Bay area, and it may be appropriate in Monterey County and Kern County as well, they suggest a mixing height I think of 500 or 600 meters.

So the chronic long range concentrations are the only ones affected by the mixing height parameter anyway.

Concentrations five, 10 kilometers away are the only ones affected by changing the mixing height.

I was also pleased to see in some of this documentation that mixing height was measured to be exactly some like 262 meters. It is really good to see such precision in the measurement of mixing height.

But a constant value is appropriate. It doesn't really need to be measured, especially for surface releases and surface receptors, constant --

To be conservative, the regulator might suggest 300 meters just to be a little on the conservative side. Of course, mixing height, as Dr. Arya said, would change with stability and other conditions, day and nighttime.

But pretty much a constant value is appropriate.

DR. VAN WESENBEECK: Just for clarification, we use 320 meters as a constant mixing height based on input from California.

DR. HEERINGA: Dr. Arya.

DR. ARYA: Regarding the spatial and temporal variation of mixing height, of course for the nighttime stable condition, the model does not include the mixing height. The stable conditions, the vertical diffusion is kind of limited by the stability anyway, so the plume does not become very thick.

But during the daytime, the mixing height can vary over a very wide range. 300 meter constant mixing height may not be appropriate really because even in California some of the climatological studies on mixing height indicate that you have very large gradient near the coast.

But there is also very large variability with season. If you consider the mixing height in January as opposed to in July and August, you can have almost four or five times large mixing heights in August compared to January.

So it is strongly dependent on the season also. Depends on the heating, surface heating.

DR. HEERINGA: At this point I would like to turn to Mr. Dawson and ask whether he feels that the subpoints in this question have been addressed or were there any points where he would like or the agency would like to seek clarification on the responses.

MR. DAWSON: I think we're going to need some clarification. Particularly, on the mixing height issue, because we're still basically confused.

I'll try to summarize our understanding. In previous discussions, Dr. Heeringa, you had mentioned this earlier, going back to the other meetings where we had this discussion where a lot of it today is very similar to what we talked about in the previous meetings where there were -- what it boiled down to I think was that we had suggested that we would develop more or less a selection criteria for identifying appropriate data that we would use for the regulatory modeling that we're going to do based on the key factors that have been talked about today.

And that's the location to the areas that we're interested in modeling. So where are those stations in relation to the areas that we're interested in modeling, whether the data quality issues associated with that, for example, CIMIS missing a day's worth of data or the quality control associated with the actual instrumentation, whatever it happened to be, and then also the anemometer height issue, which I think at least we're still somewhat unclear, especially considering the previous meetings' discussions where it seemed like to us that there was some implication, particularly in proximity that we would be looking at potentially for making regulatory decisions, let's say within a kilometer, because we're looking at this in the context of what is viable regulatory stance for us with regard to agriculture.

So if you start talking about, you know, buffer zones, kilometers type of distance, it's just not going to work as far agriculture goes.

So let's say within a kilometer distance from a treated field, it was our understanding that potentially the data from a 10 meter height, at least based on the discussions that I heard previously, could potentially underestimate exposures for people in the breathing zone within that kind of a close region versus let's say use of a two meter height.

And the reason we're asking about those particular heights is those are the data sources that we know about at this point.

So it could be various other sources where it could be, I think somebody mentioned, six meters.

So in the context of the kind of closer in areas that we would be truly, I would say, considering in the regulatory process, I guess could more clarification be made about that.

DR. HEERINGA: So for proximal gradient of concentrations using the ISC model to generate those concentrations, weather input data, if I can paraphrase that what you are asking the panel is should we still be focusing on 10 meter data or --

MR. DAWSON: Or considering all the other factors, is it really going to contribute much to the over or underestimation of exposure.

And I guess one other thought that came to mind was if we use two meters, does the panel also recommend that we adjust it to the 10 meter height using I guess what is called log wind speed scaling approach.

And if we do that, are we adding additional uncertainties compared to just using the two meter data straight and what are the implications?

DR. HEERINGA: Dr. Hanna.

DR. HANNA: Concerning the two meter, 10 meter heights, it is the practice in ISCST model that it really extrapolates. Even the input -- I'm sorry. Even the input at two meter it puts it at 10 meter.

So that's the way the ISCST operates. So the log, the power log formula is used to extrapolate from any height as an input to the ISCST to the 10 meters. In a way the 10 meters is the ISCST starting height whether if it's less than that puts it at 10 meter.

So that's one thing. The other thing is about the -- for example, the mixing height that we have been discussing. Usually, in most of the application -- and it's mentioned in the report to use the closest upper air station to estimate the meteorological station, which usually operates on two times a day at zero zero and 12 GMT times.

So you get this info which can at least give you the kind of changes in the mixing height from of course day-to-day and also during day and night.

And this becomes more important as we really need further distances from the source as, for example, in an application that reaches 50 kilometers or for the ISCST model or is it 40 or 20 or five.

You certainly need -- be more accurate to have the mixing height calculated from the nearest upper air station.

DR. HEERINGA: Mr. Dawson, did that response address your question about the two versus 10? It looks like there is a little puzzlement remaining.

MR. DAWSON: I think we're still thinking about it.

DR. HEERINGA: Dr. Spicer.

DR. SPICER: I think that part of what has been suggested is that the ISC models indeed do model the situation the same way regardless of whichever set of information is given.

I think that what I have tried to suggest is that especially during situations where you can have transitions in stability such as at sunrise or sunset that measurements at two meters, especially if you are talking about flux calculations, and even in terms of estimating exposures, that the two meter wind speed may be more important because it is actually what is happening near ground level and near breathing height as opposed to the 10 meter value.

And so the net result is that if you take the two meter value and extrapolate it to 10 meters then that's going to give you a much lower wind speed than you may actually observe at 10 meters during those transition times.

That's where the importance comes in in my thinking.

DR. HEERINGA: Based on that, Dr. Spicer, would you recommend if you had the preference to use the wind speed data from 10 meters or wind speed data collected at two meters.

DR. SPICER: For estimates of exposure and flux, I would suggest two meters. Now, that's not necessarily consistent with trying to compare to other data sets and those sort of things. Because there are general considerations in looking at 10 meter wind speeds when you are trying to compare data sets.

You are trying to answer a different question, basically.

MR. DAWSON: Again, I think what it's going to boil down to for us is that in a regulatory sense is some kind of selection criteria based on all this discussion is what we're going to have to look at.

DR. HEERINGA: And I think on behalf of SAP and this panel, too, we will make sure that in the minutes, to the extent things have been covered in these open discussions, to try to clarify that as best we can in terms of our responses, written responses.

DR. COHEN: Can I just ask one question of clarification, and this is probably a naive question. I wish I was in the earlier panels.

My understanding of the height of the wind, generally, ISC is applied in an industrial setting where you have a stack and you then either try to get the wind speed at the stack height, which can be very high above the ground, 100 meters or something, and you might even -- you generally consider a plume rise, so you actually try to get the wind speed at the height of the center line of the plume when it is starting out.

And so these extrapolations with we using the power law from whatever reference height that you have measured the wind speed at is extrapolated up to the stack height or the plume rised stack height.

Now, in this situation with fumigant releases, we're talking about the height of release of zero. It is right at the ground.

Just as a point of clarification, are we saying that as a convention you use the 10 meter wind height as the input to the ISC model as if the stack was at 10 meters high? Is this the convention that's being used?

MR. DAWSON: I think it is a convention driven by the nature of the way that the data are collected. So for example, we have looked at the CIMIS data where it is two meters. We're using it at that point. But the National Weather Service data is 10 meters. So this is inherent in the data that we're using.

DR. COHEN: Because from a strict application of the ISC model, I'm not sure that you would say that you should use a 10 meter height wind to characterize this ground release source, unless I'm -- maybe somebody else can speak to this.

DR. HEERINGA: Dr. Arya.

DR. ARYA: 10 meter information, 10 meter wind is needed to characterize the stability if we're using ISC model. To determine whether class is A, B, C, those are based on 10 meter wind speed.

Another place where the wind speed goes in the model of course in the calculation of concentration. And the concentration is inversely proportioned with the wind speed.

And there, the ISC model usually specifies that you need to have the wind speed at the height of the source for elevated sources.

So that becomes the stack height or the effective stack height. You calculate including the plume rise for elevated sources.

But for the surface source, the Gaussian model, because the wind speed of the surface is zero, cannot use zero wind speed. In reality, the wind speed at the source height will not be appropriate because effective transport velocity is really the average wind speed across the depth of the plume.

That depth increases with increasing distance from the source. So in a way, the effective velocity that one should use really increases with the distance from the source.

But for simplicity, that's not included in these regulatory models. What is done is they use 10 meter wind speed considering that 10 meter wind speed will be kind of effective velocity or considerable depth of the plume from a surface source.

But I think if you have the resources for future use, that you may use other model than the ISC, the best way will be actually to measure wind speed at two meter, 10 meter, and, of course, having the temperature you can calculate the resurgence (ph) number, bulk resurgence number so you have a continuous measure of stability.

That's a much better of stability than the stability classes. And that stability can be used in estimating or having a correction factor for these aerodynamic approach and adjusting the fluxes too.

DR. HEERINGA: In the interest of time today, I would like to move on to the next question. But if the panel members think about this particular issue between now and the end of our meeting, we will have a session for wrap up, and if we have additional thoughts on this particular wind speed, wind speed measurement height, wind speed measure simulation height issue, we'll return to it.

So at this point I would like to move on to question Number 5, if we could.

MR. DAWSON: Question 5, the agency model ISCST3 is a critical component of the SOFEA approach. This model has been peer reviewed and is commonly used for regulatory purposes by the agency. SOFEA also uses other agency systems such as PCRAMMET and PRZM3 as well as the USDA model CHAIN-2D.

Sub part A, please recommend any parameters that should be altered to optimize the manner that they are used in SOFEA.

Sub part B, ISCST 3, as integrated into SOFEA, was run in regulatory mode, which includes the use of the calms processing routine. Does the panel concur with this approach? If not, please suggest a suitable alternative.

DR. HEERINGA: Dr. Cohen is our lead discussant on this question.

DR. COHEN: In terms of the parameters for the various models, I think there has been a fair amount of discussion throughout the meeting about various choices that can be made.

And as I'm not intimate familiar with these models, I just know them in a general way, I'm going to defer to some of my colleagues on the panel if they have any particular suggestions about parameters. But I have just a few general comments to make.

One parameter that's used is the height of the receptor. And I note that you are using 1.5 meters. I wonder -- clearly, not everybody that is being exposed is an adult, you have children that are lower to the ground. And as the plume gets further and further down wind, the difference between these heights of receptor won't matter that much.

But close in, especially since we have a ground level release within 100 meters or 200 meters, it will be interesting to see what would happen if you put in .5 meters, you know, for a child to see if that would increase. I think it would increase the concentrations a bit. And perhaps, that could be considered in regulatory consideration as well.

In terms of sort of the parameters for the ISC model, I'm actually mainly an expert in the specification of dry deposition, wet deposition and chemical transformation types of parameters.

I don't think any of those processes are included in this model. So the types of regulatory default settings include things like stack tipped downwash and buoyancy induced dispersion, things like that, which seem to be more applicable to this sort of stack type of application when you are applying the ISC model to a smoke stack.

There is -- in going to the next question of part B on the calms, actually, we would like to ask a clarifying question. As I was preparing for this, I was trying to determine exactly what the calms processing routine was. And I found many references. And some of them were contradictory.

My understanding, and then maybe you can correct me if I'm wrong, is that when a calm hour is identified in the regulatory mode of application of the model, it is recommended to set the concentrations to zero for that hour. I guess let me stop there. Is that correct and that's the way it was run?

DR. VAN WESENBEECK: I don't believe that's the way it was run here. I believe the wind speed was set to one meter per second.

DR. COHEN: I know that in PCRAMMET there is a setting where if -- there is two situations. One where you have a low wind speed, but it is measurable or specified in the file in the met file like .2 meters per second.

And in that case, PCRAMMET I believe sets it to one meter per second. And that's also in the Federal Register what they recommend.

If you have on-site measurements and it is less than one meter per second but still measurable, then they say set up to one meter per second.

But that's different I think than a situation that's identified as a calm, which either can be -- calm hours defined in these met data sets that are not, you know, .2 or .5 that would be elevated to one meter per second.

So at least in the Federal Register and at least in the ISC documentation that I have seen, the calm hours are to be treated as zero concentration.

DR. VAN WESENBEECK: I would have to check on that. I'm not sure offhand. If that's the default, then that's probably how it was run.

DR. COHEN: And I guess this sort of, you know, raises this question of we all know that as the wind speed decreases the concentrations can increase.

And so potentially the largest exposures can be at these low wind speeds. And this unfortunately is the situation where the ISC model has the most difficulties.

So when they have low wind speeds lower than one meter per second they say let's just put it up at one meter per second because we don't feel confident at how it handles these .2, .3, .4 meters per second. And when it is calm, then let's not even calculate the concentration at all.

I agree that it is difficult. And I don't know if scientifically we know exactly how to handle those situations. In fact, that seems to me to call for some field measurements that are made, you know, in the near field region within 50 meters, within 100 meters of the field where you try to make the measurements under calm conditions and try to see what happens.

I don't know if anybody has done that or if you have seen some calm conditions in your work. Perhaps you could comment on that.

There was one report that I found in researching this. It was an -- I mentioned it yesterday briefly, it is a comparison of Cal Puff with ISC3. It is EPA report, December 1998, EPA report Number 454-R-98-020 by Thomas Colter (ph) and Peter Ekoff (ph).

In this study, they tried to use to compare Cal Puff, which is a puff model, similar to many of the other sorts of three dimensional models that can be used, to this Gaussian plume model, the ISC3 model. They actually picked some cases where there were low wind speeds and some calms.

They were trying to see what sorts of differences would be found. And indeed, when you sort of try to treat the calms at least -- at least letting the puff stay where it is and then let it move on maybe in the next time step, then it turns out you get much higher concentrations.

And in the near field results, they found dramatically higher concentrations with Cal Puff relative to ISC3.

It is not clear if the Cal Puff results are correct. And I think there is a scientific uncertainty here as to what the correct answer is.

But I don't think it is correct to say it would be zero. And I don't necessarily think it is correct to take a low wind speed and automatically just bring it up to one either.

So this is an area of uncertainty in the model. And unfortunately, it seems to be an area that's going to be underpredicting your exposure. So when these situations are happening, we have our highest potential exposures and our most uncertainty.

So this definitely calls out for some field studies to try to get a better handle on this. Because maybe there would be an empirical -- a lot of the parameters and inputs to this model as well as other models are ultimately based on empirical studies.

It could be that you need to do a series of studies in calm conditions to get some idea of what concentrations to use and perhaps we can do better than just assuming they are just zero.

And I don't know what else to say. I think I will defer to my colleagues for other comments here.

DR. HEERINGA: The next scheduled discussant is Dr. Hanna.

DR. HANNA: Concerning the part A, I think we talked before about the possible improvement in the model SOFEA, which in including the temporal or diurnal variation for the flux rather than using constant value for a certain time span, six hours or 12 hours. So that I think we can improve a little bit the results.

We talked about also from a parameter point of view the adequacy of concentration for distances less than 100 meters. As we noted on a different formula used for the sigma Y, sigma Z in the ISCST model to deal with, it is applicable to distances, more applicable for dispersion more than 100 meter distances.

So that also might be addressed. Yesterday, we have seen a reference relating to different formula that could be more precise at the shorter distances.

And having talked about the ISCST 3 and the SOFEA, of course the SOFEA depends on the ISCST. But a lot of the parameters that we discussed, especially like for the wind and the anemometer height and the availability in certain case studies of the wind at two or six or 10 meters, some of these really will -- in the AERMOD model has been utilized in a more applicable form.

Because you can use these winds at different heights to really get a better measure of the turbulence and consequently a better measure of the stability, of the stability class.

So that could be improved. But that's still another model and still model is being evaluated I believe right now. But I think that -- and by the way, AERMOD depends a lot on the -- have many features of the ISCST model.

But it is in the more improved or more improved way. So it could be the language of the future. But again, that can alleviate many of the concerns here.

For part B, as Dr. Cohen mentioned, the calms, and it could be -- have different kind of meaning is of the wind itself is calm or the concentration -- related to the concentration.

And I know in ICST the calm winds or zero winds are pumped to one meter per second.

But the question again comes to really what is being measured. Because what is being measured even in the report if it is calm -- I think in some of the weather reports they consider winds less than certain they are sure to be calms.

The calm might not be really calm as reported. It might be closer to the one meter per second or at least there is certain value but which we don't know. So the approximation itself in ISCST might still not be bad for this kind of stuff.

I guess that's all what I have.

DR. HEERINGA: Dr. Spicer.

DR. SPICER: To follow up, I believe that the one meter per second was essentially half the lower detectable limit for common instruments associated with velocity measurements. I think that was roughly where that came from.

I would like to simply concur with what Dr. Cohen has said already with regard to the calms. They are a concern to me because I believe that they have the potential especially during these transition periods where you can actually have a higher flux and then add that larger concentrations downwind than you would otherwise expect at subsequent time periods when the wind does pick up.

DR. HEERINGA: Dr. Winegar.

DR. WINEGAR: I wanted to address part A on a couple of things. First of all, mention was made of the

PRZM3 and the CHAIN-2D models in the preliminary part of question five.

I raised some question about the PRZM3 model yesterday in one of the other questions we were addressing. And over dinner talking with some other people who are more familiar with this, I came away with even less feeling of confidence in some of those models based on their comments. Hopefully they will speak up and can shed a little bit more light than I can here.

But basically, what I'm understanding is that the PRZM3 model is just a one D model type of thing and it looks at water vapor transport or water transport in terms of buckets that are basically gravity fed. And you fill up one and then the bucket tips over to the next one.

What I'm hearing from everybody again is that the CHAIN-2D model is probably the more sophisticated and better way of dealing with it. So I have some general concerns about that.

So hopefully some others with more expertise can fill in here.

In terms of ISC input, a lot of good comments I agree with have been made in the past about some of the concerns about different aspects of the inputs.

One of the things that have been mentioned previously in terms of some of the other models was, some of the other fumigation models in past meetings, was the input about the vertical sigma Z dispersion coefficient as an alternative instead of just using the general stability classes.

I found a paper that did a site specific determination of sigma Z using open path FTIR and the use of tracer releases at two different distances. This application was looking at emissions from a waste water treatment plant. But they were basically doing the same type of thing.

Do downward measurements, do a back calculation and try to figure out the source strength, et cetera. They did these -- used a kind of a modification of a Turner Method to determine the site specific sigma Z.

Basically, they compared the difference between a traditional treatment and the site specific treatment. And it decreases it. Again, this in -- they did tracer releases at 22 and at 46 meters from the source.

So again, this is part in that near distance regime that seems to be kind of questionable here. What they found was basically a decrease by a factor of two of these from the traditional treatment from using the site specific tracer methods.

DR. COHEN: A decrease in the mixing or a decrease in the concentration?

DR. WINEGAR: A decrease in the sigma Z. And they show an emission rate reduction after going through all the calculation of a decrease on the order of 50 percent.

So I'm not clear exactly how, what the implications are in terms of the overall incorporation of this into the model. This is something I offer and can put this paper into the record so that everybody can see it.

It might be something to consider in terms of alternative input to try and address some of these short distance questions.

I still keep going back to these plots and looking at the difference between the aerodynamic and the flux chamber and the --

Granted, it does look good in terms of the overall integrated agreement between the different methods and the mass balance there, but I have some concern about these shorter term time periods.

And this discrepancy is troublesome to me between these two methods. And perhaps some of the applications of these type of measurements and these type of refinements as an input into the model could help to understand, could help to elucidate what is going on in some of these shorter term things.

I believe that that's the endpoint of much of the risk calculations that are going to be going on. Not just a chronic mode. But in terms of shorter terms. So I think we need to understand a little bit better what is going on in the shorter time frame.

In terms of the calms routine, I agree with past comments and they've basically reiterated my thoughts in a much more eloquent manner. So I'll leave it at that. Thank you.

DR. HEERINGA: Dr. Winegar, would you be willing just to -- if you have that citation, could you read that?

DR. WINEGAR: Yes. I got it off the web site of the company that did the work, Minnich and Scotto. But it was presented at the Air and Waste Management Annual Meeting in Baltimore on June 23rd to 22nd, 2002.

It is available on line at www.MSIair.net.

DR. HEERINGA: Thank you very much. A copy of that paper will go into the docket as well. But if you didn't get that citation, you can get it from Dr. Winegar afterwards. But we have it in the record now.

Our final formal discussant is Paul Bartlett. Paul if you want to --

DR. BARTLETT: As far as question 5-A goes, I believe there is some overlap with the other questions with the parameters. I'm not sure what is remaining here as far as what the agency is concerned, except possibly the discussion of PRZM and CHAIN-2D and other ways to approach the question of emissions, which was discussed earlier.

And the thing that, I guess, what needs to be reiterated is that from the research and the modeling work that has been done in the past, that it is well-known that there are factors of soil type carbon partitioning, soil moisture and a lot of other effects, a lot of other characteristics that coincide with meteorological conditions and different regions that affect emissions.

And in this case, we're looking -- the fields case study was meant to be representative, and that was used. And not an extreme case, which we had seen in previous studies.

So to understand extreme case, you have to extrapolate, which is much harder to do. And part of this is that I believe that the studies were done in the winter.

So the scaling factors, which is also addressed in the other questions to some extent comes into question here in how to do this.

And so I think the references to PRZM3 and CHAIN-2D is other approaches of generalizing and applying the emissions to other situations.

And in this case I think the numerical models that other people in the panel here that aren't listed as associate discussants should address on how they feel that should be dealt with.

So the mission profile is very different. We did mention the problems of hourly, also, in previous discussions, on inversions, sunrise, sunset, different conditions.

And this all, of course, applies more to short term and acute exposure, which we know that this model wasn't developed for per se, but we're evaluating at this time.

And also, again, what we had mentioned in previous questions is the problem of underestimation using a Gaussian method for regional analysis and the time step. So it probably needs to be mentioned here again.

And as far as the calms processing routine goes, I think that was adequately discussed and the potential for underestimating concentration.

DR. HEERINGA: Thank you very much, Paul.

At this point, are there any other members of the panel that would like to contribute? Dr. Arya.

DR. ARYA: Paul Arya. I have a comment on the treatment of the calm. Certainly, any Gaussian model like ISC is not applicable for calm conditions because of zero wind speed. It will give concentration in finite. So it certainly is not applicable.

So to go around that, and it is not generally applicable even in low, real low wind speed below one meter per second. So even if you can measure, the instrument is good enough to measure wind speed less than one meter per second, they still recommend that you use minimum of one meter per second rather than less than that.

Even wind speeds of one or less than two meter per second you have problems at nighttime. In ISC, there is dispersion coefficient. They use the stability category. The most stable is category is F. And that is defined also for wind speed more than two meter per second.

In fact, they don't have any dispersion, any way of specifying dispersion coefficient for wind speed less than two meter per second in nighttime.

The problem there is the wind direction becomes highly variable. So sigma Z is not necessarily small. Smaller than for the F category. It can become larger because of the variability of wind direction. Sigma Y also can become larger than typical F category.

Sigma Z is considered to be smaller. But sigma Y is the most unreliable at nighttime and weak wind conditions.

So always for weak wind dispersion, better models, some models have been offered. Cal Puff probably will work better. But there are some other K theory (ph) based models where you can use kind of exact solution of the diffusion equation, which is applicable right down to zero wind, you know.

But there you have to specify diffusivities. And there are also some uncertainties about those. Thank you.

DR. HEERINGA: Dr. Yates.

DR. YATES: I have two comments. The first gets back to what Dr. Cohen was saying about collecting data for calm conditions.

It would seem that with the data sets that have been collected where they have direct flux measurements and then also have the information that they can obtain an indirect flux measurement, that maybe the data from those studies could be used to look at what is happening during calm conditions.

Just by -- you know, you have concentrations above the field for the aerodynamic mass. So you would have a profile there. And then you would also have receptor points around the field that might be able to look at what is happening for those conditions.

So I'm not sure if it would really be necessary -- well, before starting new field studies, you might want to look at existing data.

The second comment has to do with PRZM and CHAIN-2D. And while it is true that CHAIN-2D is much more sophisticated, probably -- well, it definitely handles processes in soils more accurately, more rigorously.

I don't see these models as really being a component of SOFEA. I think they are more like a tool that's used to develop or to obtain the input parameters.

And so it really depends on what is the intent of the study. For example, if say that the flux was going to be determined in some kind of stochastic way and you were going to run 1,000 simulations, CHAIN-2D probably isn't going to be a very useful program, because running 1,000 simulations where you couple the atmospheric processes to soil processes would probably take 1,000 times, four days of computer time.

Unless you have some kind of super computer, it is not going to be very feasible, in which case you might have to go and use something like PRZM.

So I guess to me it seems like a person who is going to use SOFEA has to look at all available tools and then pick the appropriate one based on constraints of computer availability, you know, what are the objectives of the study, whether the particular program still handles the, say, volatilization closely enough that you can get the reasonable kind of results.

So I would hesitate to say that no one should use PRZM. But if you want accuracy, then CHAIN-2D would be a better choice.

DR. HEERINGA: Thank you, Dr. Yates.

At this point in time, Mr. Dawson, if you feel that the panel has addressed this, are there any points of clarification you would like to seek at this point?

MR. DAWSON: No. We have no points for clarification.

DR. HEERINGA: What I would like to do at this point, since we are just shy of 10:30, I would like to call for a break for 15 minutes at this point, and if we could reconvene at 10:45 or 15 minutes until 11. Thank you very much.

(Thereupon, a recess was taken.)

DR. HEERINGA: Welcome back to the second half of our morning session.

If I could ask Mr. Dawson to read question 6, please.

MR. DAWSON: Question 6. Soil fumigants can be used in different regions of the country under different conditions and they can be applied with a variety of equipment.

Sub part A. Please comment on to what extent the methodologies in SOFEA can be applied generically in order to assess a wide variety of fumigant uses. What considerations with regard to data needs and model input should be considered for such an effort.

DR. HEERINGA: Thank you. Dr. Potter is the lead discussant on this question.

DR. POTTER: I thought in answering this question it might be useful to at least briefly review at least what I think I have heard and know about SOFEA at this point.

This is in the context of generic applications. So first and foremost, SOFEA assesses bystander fumigant exposures due to volatility losses from treated fields on a regional basis.

Its strengths include the ability to simultaneously assess impacts of multiple sources within a region and, I believe, the use of a readily available spread sheet program, Excel, for input and output. Something that most of us are familiar with and use in a daily basis. In that sense, it is a very versatile tool.

In the form it was presented, SOFEA estimates fumigant off-gassing at different points in time and space using a combination of land use and agronomic practice data in a generic fumigant flux profile.

A well-established air dispersion model, ISC3, is used to derive directionally average fumigant concentrations at defined receptor locations.

Like all models, it has limitations, and we heard many of them today, although, again, it is a widely accepted model and one that it appears to have a lot of value in regulatory settings.

In the case study that we looked at for telone in the Central Valley of California, there was an order of magnitude agreement between predicted and measured concentrations at a or multiple receptor locations. I'm a little fuzzy on that.

That was one study. Obviously, we don't have more to look at. So we're kind of looking at one data comparison here. One of the caveats on that study is that it appeared that the model may underpredict chronic and peak exposures at the high ends of exposure distributions, at least that was what was presented. And certainly this would be of a concern.

It is unknown at this point whether that's a characteristic feature of the model. Obviously, additional study could be implemented to provide some insight into that area, and many areas of possible investigation have been suggested at this meeting.

One that I think would perhaps have greatest benefit would be including hourly emission rates in flux input terms.

And again, I think that's been dealt with by several commenters.

Whatever the outcome and whether or not additional efforts are made and notwithstanding all the limitations that I think we have talked about, I believe and I think probably most in the room would agree that the model is a new invaluable tool.

One of the things that it does is to extend the principle of aggregate exposure assessment to fumigants. This is a fundamental principle in FQPA in terms of conducting exposure assessments.

We need to look at all possible routes or relevant routes of exposure for an active ingredient in order to make an appropriate determination of potential exposure.

I know of no other model. I wasn't on the other panels, but I know of no other model that makes an attempt to do this. I believe this is a real strength of SOFEA, and it represents in that sense a significant step forward for risk assessment of fumigants.

With that said, I believe there is opportunity for generic application of SOFEA to both looking at the fumigant in question in the case study telone at other regions in the country and/or looking at other fumigants.

This is in part -- I believe my confidence in this is in part to some sense because of the relative simplicity of SOFEA. What we're looking at is an engine to generate some inputs and directing those into again a fairly well established regulatory model in terms of dispersing those inputs and ultimately generating some output data which can be then routed into a risk assessment model.

So dealing with part B, what are the constraints for broader application of this particular model. While there aren't or do not appear to be any major methodological problems, again from my perspective, successful applications for other reasons and possibly even for the case study that was described here today are hindered by the lack of data or the need for better data or for data that we have a higher degree of confidence in.

So what I would like to do is kind of outline what those data types are. First on my list is the product use data. I note in the California study the registrant hired a contractor to mine the 1,3-D use data from the California PUR database to get the critical information necessary to run SOFEA in the form that it was used.

It included things like application locations, application date, rate, depth, field size, crop type and total pounds of fumigant used.

Now, we have heard some misgivings expressed about the quality of the PUR data. But with that said, from my perspective, it is the gold standard. I know of no other comparable data gathering effort of this type in the country.

In much of my work in Georgia and Florida, we are trying to look at pesticide movement at watershed scales. And we're trying to make estimates of pesticide loading on a watershed basis so we can draw some conclusions about what we see at outlets.

We find that to be a difficult and challenging task. We're faced with using best available information which would include things like farm gate reports, for example, percent acres in production in a given county. That data might be two years old. It might be five years old.

We need to combine that with things like the USDA NAS crop profiles and kind of multiply that together to get some rough estimate of pesticide loading in a particular watershed.

Given the dynamic nature and diversity of agriculture in the region that I'm working in, again, in Florida and Georgia, it is really hard to say what those estimates I'm talking about mean, especially in terms of their uncertainty or their timeliness.

So I would say that one of the, you know, the major problems in using SOFEA generically in any region is the need for this highly detailed data at least if there is going to be an effort again as described to use -- actually use data as opposed to some estimate.

Now, an alternative would be to simply follow the same model that EPA has done in looking at potential drinking water exposures under FQPA and using the model PRZM. And that is to use crop use scenarios. And then theoretically apply chemicals at label rates.

This is a well-established approach. I think stakeholders and the regulators have reached some comfort level with this. And so it certainly seems reasonable that some set of scenarios could be created which would allow the use of SOFEA in other regions in other settings and get around the problem. Because I think that problem will persist of the need for this highly detailed crop use application rate data, et cetera.

One of the other key inputs into the front end of SOFEA is the flux estimate. And we have obviously said a lot about the approach that was used in the case study. I believe this kind of unanimity within the panel that the single profile that was used even for the California setting may not provide accurate flux estimates were certainly lacking estimates of uncertainty that allow flux to be treated in a stochastically at least in any rigorous way.

This is not to say, again, this is my opinion, that the approach is without merit for regulatory purposes provided agreement can be reached on what constitutes an appropriately conservative profile.

Again, perhaps there could be some dialogue on that that would allow us to reach some consensus about what a profile should look like in terms of some kind of building in some conservatism into a risk assessment.

So I think there is a possible path forward there if, in fact, that type of approach would be taken. Of course, an alternative is to again applying the model in other settings is for a whole lot more field work. Of course, as a field oriented scientist, that sounds exciting. I would love to be engaged in that.

I'm not sure that the agency or the registrant would be ready to commit to it at this point. If, in fact, experimental efforts went forward, certainly some application of the aerodynamic method to calibrate and calculate flux would appear to be appropriate.

The registrant appears to be, in the case of 1,3-D, seems to have a headstart on this in the sense of having conducted studies in other parts of the country.

One thing that might be useful is from a summary perspective is to compile and compare data from those studies and/or other studies that are out there that are published and/or unpublished that may allow us to get a much better handle upon what flux profiles should look like under a given set of agronomic and weather conditions.

Make note of the one feature of the model as it was applied under the California setting was that the flux loss was scaled by time of year.

In California, this was done by, from what I understand just applying one -- there were two factors that were developed. Hard to say whether those factors would in any way approach reality for other settings. So certainly that would need to be examined in some detail.

I would expect that looking at other kinds of metrics such as soil temperature on application dates might be an effective way of developing a predictive tool.

Again, mining all available data and conducting regression analyses of various types might prove useful in identifying relationships where there would be a path forward in that.

Again, back to the region where I conduct most of my research in the humid Southeast, we have 50 to 70 inches of rain a year. It is wet and it is wet a lot. So in looking at flux, some consideration should also be given to the impact of precipitation events on flux.

I believe in general that it would tend to dampen flux at least temporally and that could certainly have a major impact on the shape of emission curves and ultimately exposures that are derived as that data is propagated to the SOFEA model.

If precipitation is not taken into account, it would likely tend to make the model more conservative. Perhaps that would then be, you know, rational and logical from the agency's perspective.

An alternative beyond the consensus approach, as I called it, coming up with what we think as a scientific community is a good profile or more experimental work is to use some kind of modeling effort.

There was some description of some effort to use PRZM3. From my general experience with PRZM, I'm not sure it is the most appropriate model to be evaluating contaminant flux from soils. There are better tools.

And I think that's an area that, you know, considerably more effort could be put into in terms of trying to find a model that would generate input profiles for the -- for SOFEA that are perhaps a little bit more rigorous than PRZM.

And finally, I will say with regard to the weather, again, we have heard a lot said about the weather earlier today. There are some serious limitations in terms of the availability of data that is in close proximity to the study site. That's a reality that almost everybody deals with in almost any form of modeling, environmental modeling.

So some key questions always have to be asked about whether or not the data record is appropriate in terms of both proximity and from my perspective length of record.

I think length of record is a very important consideration particularly with regard to concern for including extreme events or extreme weather years relative to exposure.

Again, like all of the above, an alternative could be to choose an appropriately conservative worst case set of conditions to be used in simulations.

I'll end there.

DR. HEERINGA: Thank you, Dr. Potter. Dr. Yates.

DR. YATES: My comments will be pretty brief since 95 percent of what I was going to say was covered by Dr. Potter. I agree with everything he said pretty much point by point.

So I'll just say a couple things more for emphasis so there won't be anything different.

I guess in terms of using SOFEA generically for a variety of fumigant uses, to me, the components in SOFEA are all pretty well documented. I think it is really the input parameters, especially the flux that determines whether it can be used generically or not.

So I think the key is really whether appropriate input information can be obtained for the particular assessment that is being considered, whether that's an acute assessment or a chronic assessment.

To be able to use it, for example, if it was going to be used for a buffer zone more of an acute type of approach, then, of course, the emission data should be something that characterizes the behavior over a region, the region of interest and should have some, you know, measure of uncertainty with it as well.

And how that flux information is obtained, it is clear it can be done through measurements, it can be done through modeling a variety of different models.

Like I alluded to before, I think that depends a bit on how -- whether, for example, if uncertainty is going to be included, that might limit some models because of computational requirements. But anyway, the key is really that that information be appropriate in terms of average behavior and uncertainty.

The met data we have already talked about as well. That has to be appropriate for the site or the region in order for the assessment to have any meaning.

In terms of part B, the only thing I could add I guess is that there may be -- information is needed, I guess, for ways like, say, improved fumigation practices that might reduce emissions.

I know in California VOC emissions is becoming a problem. Not so much from toxicology, but from ozone issues. So this model might be able to be used in that kind of a context as well. And so information about emission reduction, which could be obtained through modeling exercises or through experimentation is needed as well.

A variety of things that have been proposed, use of films, water sealing, virtually impermeable films, some kind of surface compaction, being able to simulate what happens when those kind of techniques are adopted is something that I think SOFEA can do. But how you obtain the input parameters, that's going to be the key. Some work in that area would be helpful.

DR. HEERINGA: Thank you very much. Dr. Shokes.

DR. SHOKES: A whole lot of things I was going to say have already been covered. But I will say some of them again anyway just to reemphasize.

As I understand it, the model does look at the kind of long term exposure chronic exposure. And there has been a lot of discussion about that.

I have seen some of the good things that I saw in the model that I really liked that I think if it works well in different areas that it can take into account terrain elevations and things like that, which could be a very meaningful thing, and look at the exposures and dispersion of atmospheric material that gets out of the soil.

It does allow some input considering whether the people live in areas of highest fumigation or whether they are mobile and moving into and out of the area. That could be a plus for it.

It takes a very different approach from FEMS and PERFUM models, and it is not just trying to look at the acute exposures within a specific area or determine buffer zones. It is very different in that regard.

It does seem -- from my understanding of it, it uses typical flux profiles to determine exposure at set buffer zones. And as such, it probably might potentially miss the high end short term exposures, but it could give some other very valuable information. It is not clear to me really whether SOFEA will work well with other fumigants and locations.

However, it appears likely that it could and should for the types of things that it measures be able to do this, if all of the appropriate data inputs are available.

And as I earlier pointed out, there are a significant number of flux studies, for example, available for some fumigants such as methylbromide. I would suppose that more of them are becoming available for other proposed substitutes to methylbromide.

With the appropriate weather data and terrain data and other inputs, it might be able to calculate chronic and subchronic exposures for other fumigants for a given region.

The model does use a typical, as I understand it, flux profile and calculates exposures over meteorological data.

There is some question that has been raised here about the accuracy of that since weather conditions actually could change that flux profile. And, therefore, if you are given a weather data set with an inputted flux profile, that profile that was used could be wrong. So it could cause some inaccuracy there.

So for a given weather data set, that profile could be wrong. But it would be good to know what the effects of different real weather conditions are on the model.

A question has been raised here about things like rainfall. I have some questions here about other conditions. Particularly, I look at the fundamental aspects of fumigants.

And that is what are the soil conditions when you are putting that fumigant into the soil, because the purpose of that fumigant is to work within that soil to reach toxic levels for nematodes or whatever the pathogens, weeds, whatever they are. In this particular case with 1,3-D, it was with nematodes.

The efficacy within that soil, and as it is stated in there, the dispersion of that material in that soil is going to be affected by things like soil moisture, things like soil temperature and bulk density, the organic matter, characteristics like that. I think those need to be taken into consideration. And certainly all those are going to affect the off-gassing rates that occur.

And it is quite evident from the differences in the off-gassing that occurs with different soils and climatic conditions of the four studies that were shown on page 25 of the presentation that the acute and chronic exposure could very greatly be somewhat dependent on the various soil and climatic factors in different locations. So those things need to be taken into consideration.

Apparently, this model does accept a lot of different kinds of inputs. And is apparently able to handle those. I'm not the one to speak to how correctly they are handled in PRZM or CHAIN-2D or any of those others. But apparently, it can handle a lot of different kinds of inputs.

But again, I think that the input data as close to the real situation and a region for which the model is being used, that data that is as close to the real situation should be used for that output to be meaningful.

DR. HEERINGA: Thank you, Dr. Shokes. Dr. Ou.

DR. OU: I only have two points to add.

First, if a site just had been repeatedly applied 1,3-D for a number of years, I think it is a good idea to include the enhanced biodegradation rate to (inaudible) cis and trans 1,3-D.

The other is a rare event, but it has happened. Like a hurricane. After hurricane, I believe all fumigant in air would be wiped up for quite a while until start to apply the fumigant. So you are taking into account certain event.

DR. HEERINGA: Thank you very much, Dr. Ou.

Do any of the other members of the panel wish to -- Dr. Bartlett, Paul.

DR. BARTLETT: One area that we mentioned a lot in the previous models that we really haven't brought up in this model, except on the discussion of air to mean or airshed model to mean, but what is relevant here to applying to different regions is when topography and terrain have significant effects.

Especially for the regional modeling situation of multiple sources. And in this sense as far as inputs goes, it would apply to weather data when you may need something like rams or MM5 to produce the micro meteorological data that is consistent with the topographical effects like mountains, valleys, different situations where this model might be applied, because when the nearby weather station isn't available to provide that data.

The other element in the model is that they do have the land cover, which would provide information that would be important for deposition which may have some effect in a regional analysis.

Forest may and vegetation material may clean out some of the air concentration. And then, of course, there is the roughness effects on turbulence that these introduce.

As far as I know, ISC can't handle this at this time. The AERMOD can to some extent. So this may be more for future development of the application of the model.

DR. HEERINGA: Thank you very much. Additional comments on this particular question. Thank you, Dr. Potter, for leading off I think with a very nice summary.

Dr. Dawson, are there any points of clarification you would like to seek on the response to this question?

MR. DAWSON: No. I believe we're fine. Thank you.

DR. HEERINGA: With that, then I would like to move right on to question number 7.

MR. DAWSON: Question 7, part A. Please comment on whether SOFEA adequately identifies and quantifies airborne concentrations of soil fumigants that have migrated from treated fields to sensitive receptors.

B, the agency is particularly concerned about air concentrations in the upper ends of the distribution. Are these results presented in a clear and concise manner that would allow for appropriate characterization of exposures that could occur at such levels?

Part C, please comment on SOFEA's approach for calculating and presenting probability distributions of moving average concentrations for differing durations of exposure.

Part D, please comment on the types of monitoring data that would be required to define the accuracy of simulations made with SOFEA for differing durations of exposure.

DR. HEERINGA: Our lead discussant on this is Dr. Arya.

DR. ARYA: I have a few comments. I'm sure my colleagues will fill in additional comments on this.

Regarding quantifying the airborne concentrations that have migrated from treated fields, I take it as if this is asking for -- well, it is somewhat dependent on SOFEA because it is using ISC. It can account for -- it's basically considered hour to hour. It can account for only the material that has travelled to receptor during one hour. So that depends on the wind speed, really.

So if the winds are weak, say, one meter per second, it can account for only the upstream fields which are about 3.6 kilometers away from receptor. Winds at 10 meters per second, it can go up to 36 kilometer.

So again, I think it has been pointed out that SOFEA really does not treat what happens to the material after it has been transported and dispersed for one hour.

The next hour simply takes the new emission and deals with the material really being transported and dispersed from the sources during that hour.

So it certainly cannot account for material coming from far fields. It is a short range dispersion model, straight line assuming constant wind speed, constant wind direction during the hour.

So it is really applicable to short range. Well, the fields, which are a few tenths of kilometers upwind of the receptors. So even though in the application it is mentioned that can treat some very large regions, really, ISC is not designed to really handle the material over those large time scales in that sense.

Again, it treats the next hour as a kind of new hour, emissions and transports. And forgets about what happened to the material during the previous hour. It does not bring back. If the wind direction changes, it does not bring back the material to those receptors.

Going to 7B, I think this has been discussed already enough, the upper end of the distribution. The way SOFEA calculates these is based on the meteorological data, multi year basis. It is assumed that worst case conditions have occurred during those years.

So certainly, again, it also depends on the exposure, you know, how close the receptors are, the placement of receptor to the treatment fields to catch these concentrations in the upper percentile. So it has been pointed out again by comparison that with the observations that some of the upper and percentile concentrations are underpredicted in the model right now.

Going to 7C, I think that SOFEA's approach for calculating and presenting these probability distributions seems to be adequate so far as I can understand. Essentially, running the model for longer periods and then coming up with these distributions over different durations of exposure.

Maybe somebody else may have a more -- again, I'm not familiar with the details of how SOFEA programs are treating these distributions.

The type of monitoring data that would be required to define the accuracy of simulations, I think certainly, especially for different durations of exposure, any model certainly needs to be validated against observations.

And I'm not an experimentalist in the sense that I can suggest an idealized monitoring network for this.

But certainly, it will be good to have a number of monitoring stations, you know, where you certainly want to monitor these concentrations extending from hourly averages to long term averages.

So they have to be operated over longer periods, certainly, to get those and then compare against the model results.

I think I will stop at this and ask for my fellow colleagues to fill in some of the other things.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: Thank you. Before I begin, I just had another question, a clarification for the model developers.

When you ran the model to produce the results that you created for this study, is it correct to my understanding to say that you used real data on the application, that the usage per township, so you used real data on that, but then you stochastically varied where it went and when it went. Okay. If this was going to be used in another application or perhaps for a regulatory purpose, what sorts of usage assumptions would be made?

Would you just assume the full township allocation would be used in each township, or I guess it might depend then what question you are asking.

Because, essentially, part of my comment is we're not necessarily just considering what the exposure is at the current levels of usage. But I guess you are hoping that it is going to be used more broadly.

And if so, if it was used more broadly, then the concentrations are going to be much higher. And so the results that are obtained from this model depend greatly on the usage rates.

I'm just not sure if we're always going to be able to define those accurately. I guess we have to be very careful when we define those to make sure we're asking the right question for the answer that we're getting.

Throughout this meeting, I think you have heard us say, me and others say that you may not be getting the high ends of the distribution. And it seems like you are probably doing a very good job of getting the average and even getting sort of the spread around the average, at least near the average.

But it is not clear that by just stochastically varying the parameters that you are varying you could account for sort of these worst case scenarios, which actually might happen.

It is a bit of a question of how you want to do the risk assessment. But I would argue that we're not just trying to protect the average people or the, you know, even 80 or 90 percent of the people that are kind of around the average.

We're really in a risk assessment looking at the most vulnerable people, the people that happen to be really unlucky that happen to live or work, you know, right near an area of high emissions.

And also by varying things like the weather stochastically, again, you can have situations where the strong emissions are occurring and the wind is blowing right toward the receptor at a slow rate and we're getting a very high exposure. But you might not capture that in your modeling.

I guess you would have to do it maybe for a longer period of time to make sure that you captured those extreme events.

In terms of the probability distributions, this was a question I had when I was going through this model earlier. And I learned through your explanation that you based your probability distribution functions on real data.

But in cases where this was going to be transferred to other areas, it would be useful, I think, in the model to try to provide some guidance to the user on what sort of the acceptable ranges to be varied on.

It's one thing to say that you are varying something stochastically, but, clearly, you know, the shape of the distribution and the ranges of the distribution are really important.

Somebody could plug in values that are unreasonable and get unreasonable results. I guess it is true for any model that the output is dependent on the quality of the inputs, but this is a sort of vulnerability of SOFEA that it is hard to get the data as we have heard and there is a potential for getting to inaccurate conclusions if you don't use the right data.

And finally, with the monitoring data, this is a very interesting question. This is something that I spent quite a bit of my own time on, is how do you evaluate these models. And in the real -- in the best of all possible worlds, what one wants to do is use emissions data, metrological data, and monitoring data for the same time period.

That's really what you need to do when you want to do model evaluation.

In your case, you are stochastically varying the weather. So you are sampling from five or 10 or more years of weather data. You can't really even be expected to match the concentrations in any given year.

In order to evaluate the model as you have currently configured it, you would be looking at long term monitoring data, like of 10 years or longer. I'm not sure that exists in California. But it may.

As an alternative, if that were not possible to look at sort of the long term data sets, it would probably be possible to tweak your model a little bit to use only one year of meteorology and disable that one feature of the stochasitic variation. And then run the model for that one real year and compare it against the measurements of that particular year.

And I guess the complication of trying to evaluate the model over many years would be the application rates and the usage rates are changing over that period.

So I think the model would be assuming -- let's see. The measurements are accounting for the fact that things change dramatically over that period, say, in terms of usage per township, but your model wouldn't necessarily be able to incorporate that. So it may be difficult to properly evaluate the model in that way.

That's it. Thanks.

DR. HEERINGA: Thank you very much, Dr. Cohen. Dr. Majewski.

DR. MAJEWSKI: I agree with Dr. Cohen that the best way to evaluate a model is with ground truthing and monitoring. The one example that was provided was the Kern County study or modeling exercise in which the 10 year, 24 hour averages were calculated or simulated and compared to the Air Resources Board Ambient Air Monitoring data for the area.

And the comparison of the concentrations versus the exceedence percentiles appear to be very good up to about 95 percent in exceedance, which kind of confirms what we have heard for the last two days, is that the upper percentile seems to be underpredicted.

And Dr. Cohen's suggestion that verifying a model using matched air concentration and meteorological data and use data, I think it is possible in California with the historic use data that is available, the historic meteorological data that's available, and the air resources board monitoring data that's available.

I don't know if they have or how much 1,3-D ambient data they have or how long they have been collecting it. But that may be an opportunity there to combine all the relevant data that's needed and see how well your model predicts what the ambient concentrations, measured concentrations are and then maybe focused in on fine tuning the model at the high concentrations or the 95 percent or 90 percent exceedance values or at the upper end and see why the model is underpredicting.

Thanks.

DR. HEERINGA: The next discussant on this particular question, Paul Bartlett.

DR. BARTLETT: I guess I agree with the previous commenters as far as the phenomenon that we have outlined that would result in underestimation, Dr. Arya's comment that in the ISC Gaussian model that the plume disappears every hour. So you would be underestimating ambient background -- the background as well as potential peaks in certain areas of overlap. So then that goes both to background and the peaks, I believe.

The significance of this problem, I like Dr. Majewski and Dr. Cohen's suggestion of working with some existing data sets and seeing how well it performs.

Another approach might be as well is to do a comparative modeling analysis of with Cal Puff. Or I'm not sure how easy it would be to use SOFEA with Cal Puff or AERMOD. And to do an exercise with a simulation.

So you get some understanding to the extent and significance of the underestimation. For chronic, it may not be significant.

That's an interesting idea what Dr. Cohen is mentioning as far as you may be unlucky as far as your location goes and be in an area where you may have purely from locational factors and meteorological factors might have much higher rates of exposure than other people.

I believe your method of allocation within township is getting at some of those situations that might arise there.

And this comes into another suggestion we had in the previous panels is that SOFEA and these models that do try to estimate some of these upper end phenomenas might give us some ideas of risk situations that we're not aware of right now.

Especially in locational factors of proximity, of unique proximities to usage. And we had not looked at chronic before. So I believe that's all that I want to add to the other comments.

I had a question on the -- I didn't find much documentation on the moving average technique that you used. I assume it was to smooth data for long term.

And the question, and I'm not sure what the agency is asking about here with varying duration of exposures and application and moving average, what you had in mind.

This is question 7C.

DR. HEERINGA: Mr. Dawson, I think it would be good for the panel to respond to that. I was looking for a little clarification in my own mind too there.

DR. BARTLETT: That question again is moving average. And I believe they used it for -- in one instance, in actual photographs we saw I didn't see a lot of documentation, since you are saying varying levels of duration of exposure, whether you contemplate it hourly, 24 hour or what you meant with the question on this.

MR. DAWSON: It is basically all of the above. Going back to the fact that when we go through our risk assessment process, we're identifying potential hazard concerns for the different durations of exposure, just depending upon the specifics of the case.

But for most of these, we are going to be looking at -- the general categories we look at are acute, which are, for most of these, 24 hours. Some of them are an hour.

Then we are looking at shorter term durations, which are up to 30 days or so. And then kind of a more intermediate or a little bit longer subchronic duration, which is out to several months. And then the chronic estimates, which are basically every day over the course of a year.

So that's the basic categories we're looking at. And we are trying to consider all those categories for each of these cases.

DR. BARTLETT: So I guess my comment is I didn't see enough analysis to know whether the moving average technique is needed or not. And maybe some people here that are more versed in statistical theory could answer that.

DR. HEERINGA: Maybe I could ask Dr. Van Wesenbeeck with regard to this. The moving average calculation, I'm aware that on Page 57 of your handout you used it essentially to look at the sort of best choice of length of simulation runs that was a convergent.

Is it also used to summarize or to stabilize estimates of distribution quantiles in sort of shorter period exposures other than one year period?

DR. VAN WESENBEECK: It is more the latter, that it is to look at subchronic situations where we can get a moving average over a 10 day or a 15 day or whatever period the user specifies.

And the way the model does that is it takes the

24 hour concentrations at each receptor and averages those over whatever the moving average period is specified.

DR. HEERINGA: Through a year long simulation.

DR. VAN WESENBEECK: Through a year long simulation. And it takes the distribution of those at the end of the year so that the risk assessor can use that.

The figure I showed at the end of my presentation where I was looking at how many years to simulate was really a different thing.

DR. HEERINGA: Very different use of that.

DR. VAN WESENBEECK: Yes.

DR. HEERINGA: With regard to that specific sub question, anybody on the panel, do they feel able to sort of step in and evaluate this at this point in time? I think that it -- I won't call for anymore then at this point. Maybe give a little consideration to it. We might come back to it. But thank you for the clarification on it.

Are there any additional comments from panel members? Excuse me. Mr. Gouveia, of course, we have scheduled.

MR. GOUVEIA: I think there has been sufficient discussion on the panel about spacing in the near field of the near field receptors for the chronic exposure case.

I wonder maybe this is a question for the other panelists. If there are studies looking at peak to mean ratios, spatial peak to mean ratios where an estimate could be made of what that peak concentration could be between two spatially separated receptors, there might be some defensible factor to multiply to the modeled receptor to get a peak concentration at an unknown location.

A similar method could be used to estimate or similar methods have been used to estimate concentrations at sub hour intervals or intervals less than have been modeled or measured.

There is quite a few peak to mean studies out there.

DR. HEERINGA: Questions by other members of the panel? Dr. Cohen.

DR. COHEN: If I could add or just follow on to that. I think that's another vulnerability of the SOFEA model is this grid size problem.

I think you discuss it that if you pick a smaller grid size, theoretically, you are getting more accurate results. But then your computer requirements and speed of processing go up.

And so it seems like, again, if somebody uses this and then decides, well, I want to do this quickly or I don't have a very fast computer or whatever and uses a fairly course grid size, they could really get fooled. I mean, especially in the near field situation.

So I would almost argue that or I would argue that if you are going to ask this model to give you answers for the near field, which I think is one of the key questions from a regulatory point of view, there may be need to be sort of a minimum grid size that you recommend that you almost hard wire in or strongly caution the user to make sure that they adopt.

And I'm sure that the California folks will use the grid size appropriately, but in a more general sense may not always be as expert.

DR. HEERINGA: Mr. Gouveia.

DR. GOUVEIA: My suggestion for grid size would be to relate it somehow to the size of the sources, the area sources. Maybe a factor of two less than the dimensions of the area source might be an appropriate starting point. Maybe a factor two less.

This also brings up another issue about the randomness of the areas that are used in the SOFEA model, the randomness of the distribution among the township.

I could imagine if these areas, these treated areas were close together, and quite often in agricultural areas they are, the treated areas are close together, juxtaposed to each other, the chronic exposures would be much higher at selected receptors.

So maybe a special SOFEA run could be done that places all the areas together just to see how high, how much higher the concentrations could be at the very high end.

Of course, the average concentrations would drop if the areas were brought closer together. The concentration for the -- on the average in the distant concentrations would be reduced. But these close in receptors might be higher because of that.

DR. HEERINGA: Thank you very much, Dr. Gouveia. I think David Maxwell is the final.

DR. MAXWELL: Dave Maxwell. And I'm just going to bring up just a few points because the rest of them have been addressed. I think there is a strong consensus about the ISC short term three model. The pollutants being lost after each hour is just a fact in the model.

So a question I would have is whether the Cal Puff model or the AERMOD model would be run just as a test using the same type of data for comparison purposes.

And it is true, apparently, the background and maximum values at least with the ISC ST-3 run, they seem to be underestimated.

The underestimation of the concentrations near fields where applications occur, they seem to -- would occur more than if they were a uniform grid. I think that's just the generality.

In looking at question C, sub part C of this set, weighing the receptor grid to the size of the area source I think is important. It has been brought up before. I think that's a good issue.

And I like to see an explanation in more detail what was just recently discussed about the moving average applied in the SOFEA model. Maybe if there could be a little more documentation. I think your theory is good. I just think a little more explanation would be useful on that.

As far as D goes, I just have a question on -- I know you had some slides yesterday on the monitoring, the air monitoring that you did. How many of those samplers did you have at your test sites?

DR. VAN WESENBEECK: We typically have four to eight. Usually, eight actually off-site samplers for each of our flux studies. Usually at 100 and 300 feet. Sometimes at 100 and 800 feet.

And we usually use the flux input from the aerodynamic method to model the off-site receptors directly and see how that compares. And I showed a couple of examples yesterday where it worked fairly well. It usually works reasonably well for us. But not always.

DR. MAXWELL: Thank you. That's all for my comments.

DR. HEERINGA: Do any other members of the panel have comments on question number 7 or its subcomponents? Dr. Winegar.

DR. WINEGAR: I wanted to address just question D regarding the types of monitoring data that would be required to define the accuracy of simulations.

As a monitoring kind of guy, this is kind of right up my alley, I guess. It seems to me if we're talking about both near field and tighter time resolution situations, that the studies with the six hour and 12 hour integrations kind of wash over a lot of the detail about what is going on during those time periods.

And it seems to me a gut feeling is that perhaps a better way to deal with both of these situations is to -- since ISC is dealing with an hour by hour calculation, you have an hour by hour, if you could have an hour by hour met data collection, which is easy to do, but also hour by hour concentration measurements, which is not as easy to do, but is indeed feasible.

In fact, there are even technologies that can do continuous measurements down into double digit part per billion for a range of VOCs including this compound. These technologies aren't cheap or -- well, they are fairly readily available, but it is not like a -- it is commercially available, let me just say.

So I guess that would be my comment, is that if there were to be any other studies, that you look into these type of technologies that would allow you to tie all of the time dependent phenomenon together on an equal time basis so that you could define the time resolution in a near field resolution.

DR. HEERINGA: Thank you very much, Dr. Winegar. Are there any other comments? Dr. Arya.

DR. ARYA: I have a comment on the use of alternative models in order to better handle this limitation of plume getting lost after one hour.

I think AERMOD would not be -- AERMOD will do the same thing. AERMOD is also a short term model, and your material gets lost every hour. So replacing with that will not get over that problem.

DR. HEERINGA: Mr. Dawson, I think I would like to turn to you to see if you feel that to the extent possible here that we have covered the elements.

I recognize I think that element C is -- we have not fully responded to that.

MR. DAWSON: I had one relatively simple clarification. And Dr. Johnson has one as well he would like to discuss.

Basically, mine was there have been a lot of discussion about comparison of the model outputs with monitoring data. And also Dr. Winegar just had mentioned different monitoring techniques.

If the panel could provide specific comments, for example, with the nature of how you might do a comparison if there are specific tests or approaches that might be recommended for that.

As far as the comparison of the results, those kind of things, if they could be entered into the record it would be appreciated.

DR. HEERINGA: We'll see that that's done. And I think include commercial names if they are available.

Dr. Cohen.

DR. COHEN: One of the things that we saw in the pseudo evaluation that you presented was essentially a comparison of the frequency distribution of concentrations. But we didn't see actually sort of the locational point by point, did this location get the right concentration.

Now, I know you probably can't do that in your case because you are stochastically varying the locations. You don't actually know where the sites were.

But in a real model evaluation situation what you would do is you would have specific locations where you were sampling. And those would be the receptors in your model run and you would compare the concentrations, you know, at these specific locations with the measurements at those specific locations.

And you want to have the -- the more locations, the better. And the higher time resolved data, the better. But in order to do that you would have to, I think, take your model to the next stage like you discussed of using real field locations based on satellite photography, real application information. That's quite a difficult process.

But that's what you would have to do if you really wanted to test out the model, I think. Is it really getting the right answers. You would have to -- you might be able to do it with in cooperation with a group of farmers in a region that would tell you, okay, we're applying on this day and we applied this much.

In order to characterize the near field situation, you might not have to, you know, talk to that many farmers, if you get like 10 in a region that are applying to the crops.

DR. HEERINGA: Dr. Arya.

DR. ARYA: I think I agree with the suggestion for model evaluation. Probably will be better to kind of limit the monitoring to an area, a smaller area where you also have information on the exact application of this material to the fields, the times, and rate and everything.

And hopefully you should have actual measurements of the flux also during that evaluation.

So probably it would be more useful to kind of limit to, maybe if there are some isolated areas where these applications are done and you are not getting too much exposure from other far away fields.

In any case, most of the, I guess, near field exposure will be from the area.

DR. VAN WESENBEECK: Just to comment on that. We feel fairly confident with the simulations from a single field based on validation with single field studies. So since the model is really just a superimposition of individual treated fields, there shouldn't be a huge difference in that regard.

Also, if you look at the figure on top of page 38, which is the location of the top 1 percent of receptor concentrations, they do all occur near treated fields.

So we know that the model is not doing anything strange in that regard. It makes sense.

DR. HEERINGA: Thank you very much. Yes, Mr. Houtman.

MR. HOUTMAN: Bruce Houtman. I just wanted to follow up on some of the comments about air monitoring, particularly as confirmation for some of the assumptions that are modeled.

In one of the field studies we did conduct, we brought out an FTIR unit to help at least at that point investigate maybe some real time air monitoring techniques that could be used to give real instantaneous feedback.

We had difficulty both in terms of sensitivity and interferences with that technology, which at that point led us to drop it and go back and continue to rely on absorbent tube method for air samples.

So if there is technology available that gives one hour air monitoring result and adequate sensitivity without interference issues down to part per billion levels, we would be very interested in that.

So if that could be maybe part of this documentation of this panel review, that would be very helpful.

I also submit this question about flux monitoring and air monitoring confirmation of modeling is really a fumigant issue. Every soil fumigant has its own data set for these source strength terms. Each of them vary a bit. But each have their own limitations.

Air monitoring as confirmation of modeled air concentration is an important issue that I think also fumigants are facing.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: Just one question actually to ask to the California folks.

Mr. Dawson, can you tell us actually what monitoring is occurring in California for telone?

MR. DAWSON: I'll take a crack at it. Basically, the studies that are available that we're considering a risk assessment for telone include the single field monitoring size that we have been talking about over the last couple days. And Bruce may want to correct me if I'm not exactly accurate.

The other types of study that are conducted as we understand it are those initiated by the California Air Resources Board. And they essentially consist of two different types of studies.

And the situation is also similar for methylbromide, but I don't believe it is similar for the others.

Those two types of studies are essentially what

I would call targeted monitoring data where they look at levels, ambient levels in areas of high use during the season of use.

So you might go to Kern County or some other coastal county or whatever and put the samplers and run them over a seasonal range of six to eight weeks, whatever the use season might be. So we have that data that we're considering.

And I believe -- there is also something called the TAC, Toxic Air Contaminant. It is something that CARB uses to identify and quantify background levels in urban areas. I believe it is 20 stations in areas like Burbank and Los Angeles. Those kind of things. We're using them as well.

They are monitoring at equally spaced intervals over the course of a calendar year.

DR. COHEN: In the targeted studies, the middle example that you gave, do you know approximately how many stations that they are having in the area or how far apart they are?

MR. DAWSON: I haven't looked at the telone data recently, but going off the example for methylbromide, which I have looked at more recently, it is, I would say, five to eight stations. Something like that.

DR. COHEN: Spaced a couple miles apart or --

MR. DAWSON: They could be within a county. So they might have --

DR. COHEN: So a little further apart than that.

MR. DAWSON: Right.

DR. COHEN: Do they do the study for an entire year? Just for the season of application. And how frequently -- what is the frequency of sampling and duration of sampling?

MR. DAWSON: In those sampling studies, I believe they are sampling four to five days per week and not on the weekends. So you would have six or eight -- the details are alluding me, but whatever the duration is and you would have the three or four days or four or five days per week and then times your eight weeks. That would be the number of individual samples.

DR. COHEN: Just as a comment. That kind of approach is fairly common. And actually, around the Great

Lakes there is a network that measures in their case like once every 13 days or something. And here you are getting a much better coverage.

But in general, any time you have a monitoring program where you are only measuring certain days, you have the potential of missing hot spots and missing peaks. And it doesn't happen all the time. You catch a lot of them.

But every now and then there is going to be -- I wonder why aren't they measuring on the weekends. Certainly some applications probably occur on the weekend.

MR. DAWSON: Right.

DR. COHEN: I guess it is just a question of logistics and personnel and all of that. From a model evaluation point of view, it is often kind of scary to use data which is sort of sensored in that way because you could miss a peak just in time by a couple of hours or half of a day because of small errors in the meteorology or the characterization of your model.

And actually your model evaluation may look a lot worse than it really is when you're looking at data which is discontinuous like that.

If it is possible to leave your samples out there unattended, I don't know quite how it works, but if it would be possible to get a more cumulative impact so you don't run the risk of missing peaks, that would be a much better way to do the monitoring, if it would be feasible to do that.

MR. DAWSON: That's a very good point. I think as we move forward with our strategy on fumigants, that these are things we need to think about and address. At this point we're handcuffed, if you will, by the nature of the data.

DR. HEERINGA: Yes. Mr. Houtman.

MR. HOUTMAN: Just a quick comment, just to make sure it is clear, that the Air Resources Board targeting monitoring that Jeff just described is what was the data set for that one particular site and time in that pseudo validation that was used.

And frankly, I do believe that is probably the best available ambient air monitoring data at least we're aware of. But they do that for actually other chemicals beyond just methylbromide and 1,3-D. Other fumigants are also a part of that.

MR. DAWSON: If I may follow up to what Mr. Houtman just indicated, the CARB data that we just talked about are really the only sources for this type of data for this category of chemicals we're aware of.

So if the panel is aware of other sources of this type of data, we greatly appreciate being made aware of this.

DR. HEERINGA: Dr. Winegar.

DR. WINEGAR: Well, I just had -- I guess to amplify on most of what you said in regards to the CARB data, I have been involved in a couple of methylbromide regional studies. And the network we did was four sampling stations over an approximately 10 mile area sample at four days a week from Wednesday through Saturday, actually, in that case.

But the routine CARB monitoring doesn't -- I believe, I think, they indicated that it doesn't usually go over the weekends just because most people don't like to work on Saturdays.

But that data, for methylbromide at least, there is pretty good data sets for the Monterey, Santa Cruz counties, San Maria area and Camarillo, Oxnard, Ventura county areas.

I don't know as much about the telone data sets that have been developed by CARB.

MR. DAWSON: Unfortunately, I can't remember the specifics of those off the top of my head. It has been a while.

DR. HEERINGA: For the record, too, the graph that Bruce Houtman referred to is on page 47 of the handout. Compares 10 year simulation average to the ARB measurements in 2001.

At this point, any additional points of clarification?

MR. DAWSON: Dr. Johnson had a point on the C, sub part C.

DR. JOHNSON: I think that an element of 7C that maybe isn't really clear from the way the question is worded is not so much an emphasis on using a moving average technique as it is a question about when you consider the different durations of exposure as Jeff outlined going from chronic down to acute exposure.

The SOFEA model presents a cumulative distribution of concentrations, which is based on all of the receptors in the modeling region.

And the question is is that appropriate for all of those ranges of exposures going from acute up to chronic.

DR. HEERINGA: With that added information, Dr. Arya.

DR. ARYA: I think so far as the chronic is concerned, maybe the averaging all the receptors in area might be all right. But for acute, I think it is more important to really consider near field receptors, which are near the treated field because they will give a larger concentrations.

DR. HEERINGA: With regard to less than one year chronic exposures, the time periods that you are concerned about most, are they to be seven day periods, one month periods or 24 hour periods?

MR. DAWSON: For most of the cases we're looking at now, it looks like that the acute, which is 24 hours and less, are going to be the key concern of our risk management decisions.

But we're still definitely wanting to look at the subchronic durations. But it looks like based on our analysis, that's how it is playing out.

DR. HEERINGA: So as I interpret it, then, the question, then, is really how stationary is any particular bystander with respect to an exposure point.

And if you allow greater lengths of time, they are obviously circulating in the region in a more random fashion than just you might expect in the worst case acute exposure where somebody might be in their home, in their yard for a 24 hour period.

MR. DAWSON: That's correct. But what we want, I guess, to get from this exercise is to first get a good handle on the nature of the air concentrations and then decide how we're going to overlay the mobility as Mr. Houtman described it on top of it to complete the risk assessment.

DR. HEERINGA: My assessment is that that is, in fact, a critical issue. And that is, as we discussed yesterday, I think the SOFEA model currently measures concentrations at these random receptor points.

And a tough issue in going from chronic to acute is how you are going to station the bystander with respect to a particular set of receptors upwind, downwind, near field, moving around, et cetera.

Dr. Cohen.

DR. COHEN: When you pick your meteorological data on an hourly basis, is each hour, then, you could pick from a different year stochastically or is each day? How do you do that? So you start with hour 1 and you collect some data from -- met data, you go to the next hour. Can you then take data for that hour from any year?

DR. VAN WESENBEECK: No. It picks a year and then it follows that year from Julian day 1 through Julian day 365 sequentially hourly. And then it picks another year.

DR. COHEN: So you are doing an entire year analysis. That wasn't clear to me in the -- so then each analysis is 8760 hours of analysis, I mean, 365 days of analysis for that one year.

DR. VAN WESENBEECK: Right.

DR. COHEN: Thank you.

DR. HEERINGA: Dr. Arya.

DR. ARYA: I would like to make correction to the statement I made earlier that may be all right to use the average of all the receptors in the region, you know, for chronic exposure. I think I would like to correct that.

That even for chronic exposure, the receptors located just outside or near field outside the buffer zone probably they should be used because they will give you higher concentrations -- rather than averaging over the whole region.

Because I'm sure the exposure, you know, those receptors who are exposed for long period of time, those who are near the treated fields certainly they are going to be exposed to higher concentration all year around.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: I'm sorry to go back to this.

Are you sure that -- when I read your paper and the material, it sounds like the weather year is something which is stochastically varied along with everything else.

You couldn't -- I don't see how you could do the analysis if you are doing a whole year --

DR. VAN WESENBEECK: It is stochastically varied in the sense that just the year of the weather is chosen, is varied through Crystal Ball. So we just have five year weather records of CIMIS data.

So say 1995 through 1999 inclusive there will be a Crystal Ball PDF that has those five years in it. Crystal Ball will pick one of those years and then it will start on Julian day 1, work through that entire year placing fields, making applications and running the model.

And then at the end of that year it picks another year and goes through that same process again.

DR. COHEN: Okay. Thank you.

DR. HEERINGA: Any additional comments on question 7? I appreciate the clarification on 7C. I think that made that much easier to follow.

Mr. Dawson, any further clarifications on question 7?

MR. DAWSON: No. I think we're fine. Thank you.

DR. HEERINGA: I'm going to ask for a little group thinking here at this point. I'll make a decision.

We are at the final question, question 8 and wrap up. And we could continue with that at this point or we could break for lunch.

I assume that it would be the preference of everyone here just to continue with question 8. Is there anybody here that -- Mr. Dawson, is that satisfactory with you at this point?

MR. DAWSON: Absolutely. Thank you.

DR. HEERINGA: Let's do that, then. Let's go ahead with question 8. If you would read it into the record, please.

MR. DAWSON: Question 8, sub part A. What types of sensitivity and uncertainty analyses of SOFEA are recommended by the panel to be the most useful in making scientifically sound, regulatory decisions?

Sub part B, what should be routinely reported as part of a SOFEA assessment with respect to inputs and outputs. Are there certain tables and graphs that should be reported.

Sub part C, does the panel recommend any further steps to evaluate SOFEA. And if so, what.

Sub part D, SOFEA uses a Monte Carlo based approach based on varied random number streams for each simulation. Can the panel comment on the appropriate statistical techniques that should be used to define differences between outputs for different scenarios?

DR. HEERINGA: Our lead discussant on this is Dr. MacDonald.

DR. MACDONALD: In the initial stages of model development, it is enough to run select scenarios and interpret the results one scenario at a time. SOFEA is now ready for more than that. There are good discussions of experimental design for sensitivity analysis in SAP minutes 2004,01 and 2004,03.

In the level two aquatic model session 2004,01, the work of Cline (ph) in 2004 was cited. This approach uses principles of experimental design, fractional factorials in particular and response surface methodology to determine which are the critical assumptions in the models and which factors drive the simulation.

Again, I advocate that the agency try these methods.

Part B, I haven't had the opportunity to try running SOFEA because of the Crystal Ball requirement. And I'm not a potential user of the software. So my remarks will be very general. I expect other panel members to make more specific suggestions.

I understand that SOFEA returns tables giving exposure at many locations at a sequence of times. In the first stages of testing you will need all sorts of plots to help you decide if the results make sense and to look for efferent values.

Time series plots, box and whisker plots and scatter plots will be useful here for as many variables and combinations of variables you can think of.

As an aside, I consider box and whisker plots the most useful tool there is for exploratory data analysis, but they are unfortunately very clumsy to create in Excel.

Further down the line, end users will appreciate geographical contour plots for median and upper percentiles of acute and chronic exposure. Note, however, that the results for upper percentiles will only be meaningful if the model captures all sources of variation and enough simulations are run under each scenario.

I was interested to note that the plot of concentration versus exceedance percentile for the pseudo validation shown in the agency presentation, handout page 47, which we keep coming back to, shows concentration on the log scale.

Even though statisticians like log scales because the plots look neater, I understand that the agency prefers to show toxins on linear scales.

Putting this plot on a linear scale would deemphasize the good agreement at low concentrations and exaggerate the poor agreement at high concentrations, giving a very different impression.

If we accepted as more important for models to be accurate at upper percentiles, diagnostic plots should be on linear scales.

Part C. SOFEA, like any other model at this stage of development, will need a line by line code audit by an independent programmer to ensure that the code does what it is supposed to do.

The hardest programming errors to detect are those that delivered results that looked correct, but are, in fact, wrong. A code audit should be able to pick up any errors of this kind. The Fortran code in particular needs to be audited because it is so detailed.

SOFEA relies on code within Crystal Ball and Excel. The statistical functions in Excel are known to be deficient. Serious problems with the Excel random number generator were identified in SAP minutes 2000-01 citing McCollough (ph) and Wilson 1999.

We need documentation and testing of the random number generator in Crystal Ball. And if it, too, proves to be a deficient, a better random number generator has to be used instead.

The broader question of determining whether the model is good enough is much more difficult to address. Because of the wide range of expertise on the panel, we have heard many suggestions for enhancing the model. Some of these may make a significant difference in model output under some scenarios.

Because we could go on forever improving the model, the question is not so much whether the model is completely realistic, but, rather, is it complete enough for regulatory purposes.

At this stage I would recommend incorporating the proposed enhancement that looks most promising and to doing more validations or pseudo validations in comparison to field data looking particularly for agreement in upper percentiles and under typical as well as extreme scenarios.

Comparison with observed field data seems to be more possible in this context than in other exposure modeling I have seen.

Part D. This is the correct way to run simulations with independent streams. In the exploratory stage of development, scenarios should be run several times with independent random number streams.

The variability and the results can be displayed with box and whisker plots or superimposed time series.

When you proceed to a more formal sensitivity analysis, using the methods advocated in part A, the variability between simulations due to independent random number streams will be taken into account in the analysis.

That completes my remarks.

DR. HEERINGA: Thank you, Dr. MacDonald. And Dr. Hanna is the second discussant.

DR. HANNA: I just add a little bit. For the part A, the sensitivity, I think -- I wonder or I think if it's possible really to examine the sensitivity of some of these run to the uncertainty or variability into sigma Y and sigma Zs.

That could be really done if we have a distribution of the sigmas based on different kind of field experiments or even comparisons with the calculated values.

So we can be even randomly picked and included in some of the model runs for certain year and look at really how much sensitivity we get as a result of the sigma Y and sigma Z.

This usually will be a multiplier since it is the R. The distribution would be like a log normal distribution.

The second point about that, I don't know about how important is the background, I mean, the concentration background of this field measurements or in this kind of experiments.

Is that something that really should be considered in the simulations especially for the acute kind? Even actually for the chronic kind of simulation. I don't know about that, if that should be addressed.

I would go to C because my B and D are kind of a combined in my response.

In C, as was mentioned, or we discussed before, maybe more evaluation related to the terrain effect and the location and also to the longer period of meteorological record as we discuss. Because with more years, probably will be able to capture the worst case scenario or be more likely to capture the worst case scenario.

Going on to B and D, I really like the way that you have done your analysis. But I have some suggestions, is really give more attention and not more attention, but kind of more information related to the receptors.

And, of course, in most of the applications, you have a large number of receptor. But at least can be a selected number of receptors that you distribute or show graphics of varied statistics like the median, coefficient to variation, which is the standard deviation over the mean.

And also, the range, what is happening with these receptors if we picked 100 receptors or something like that. What is the uncertainty range of the variability range.

That is practically a range between the -- if we take a difference, say, between the 2.5 percentile and the mean and divide it by the mean and looked on the other end of the 97.5 percentile, the difference between the 97 percentile and mean and divide it, then the mean --

We can come up with a range really of what is the kind of -- even if it is related to the uncertainty indirectly, what is the kind of range that we can expect in this kind of Monte Carlo runs that have been established. Thank you.

DR. HEERINGA: Thank you, Dr. Hanna. Dr. Yates.

DR. YATES: I guess under item A, it seems to me that there have already been a number of evaluations done on SOFEA. I'm sure there are more than what I have listed here, but some of the ones that came to mind were if made comparisons between direct and indirect flux calculations, compared model and measured chronic exposure. That would be the pseudo validation figure and analysis.

Compared measured and modeled downwind concentrations using directly measured flux values, investigated the effect of spatial and temporal changes in source terms where the fields are alternately active or deactive during the time period.

Now, those things all help I think to give some comfort in the way that SOFEA works. But it seems to me that in terms of evaluation, there is probably -- if there was 10 times as many steps in evaluation, we would probably want 10 times more.

It seems like evaluation, you are never

satisfied. You always want to see more and more. But I think overall that a pretty good job has been done.

Clearly, there are things that could be done in addition to these steps that have already been taken. But I thought that what has been done was pretty good.

I think that there needs to be a look taken at the uncertainty in the cumulative emissions, which I would expect would be fairly low, relative to period emissions, what I would expect to be pretty high, since it seems that for, let's say, acute exposure assessment the period emissions would be very important.

The use of the direct methods for providing the emission inputs I think is good as a way to reduce uncertainty at sort of the front of -- when you are obtaining the information.

But even so, since the intent would be to take this information and use it at different locations and times, there needs to be a look at how much change in variability you would find in regional and temporal situations.

So some kind of uncertainty for flux estimation across space and time would be useful also.

I'm sure there have been studies that have looked at a more numeric sensitivity for the ISC and PRZM and CHAIN-2D. It might be useful to summarize this information.

And if anything is missing, to do an actual quantitative sensitivity analysis on the input parameters and have it in one place so that when regulators begin using this they will know which input terms are the most important to characterize accurately.

But I would guess that all this has been done. It is probably in the literature. Someone could take a look at it and summarize it, I would think.

Part B. It seems to me that using -- one of the advantages of using the Excel as the user interface is that really all the information for the input and output is right there in the file. And so in trying to think of what should be reported, it is right there.

So I mean, you open it up. If you need to know something, you open it up, take a look at the probability density function that was used and then you go to a different spreadsheet. You can look at the output. The only thing really missing are graphs.

And while I'm sure that some types of graphical information would be needed by just about everybody, one of the strengths in SOFEA is the idea that you can produce what if scenarios.

In those cases, you probably have to create a new type of graph. I didn't really have any suggestions for what in particular to put in there.

I think in a way that would be -- probably the user of the program will eventually create a new worksheet with the kind of graphs that they need.

And the way that SOFEA works, it creates columns with the output data so that it would be pretty easy for someone to come in later and create the graphs they need and just save that worksheet and continue using it in the future, and the graphs would be produced automatically.

So I don't have any specific suggestions in that area.

And then, of course, further evaluation of SOFEA would be good similar to what was done for that pseudo validation test.

The main problem, I think, is really going to be is there data out there that could be used to do those validations. It seems to me that most available data has already been used in that manner.

DR. HEERINGA: Thank you, Dr. Yates. Dr. Shokes.

DR. SHOKES: Most of the statistical things that Dr. MacDonald mentioned after they are done we look at the other use of the model and actual practical use. I think it does need to be checked out. Be sure it is doing what it says to be doing.

That may well have already been done. It looks like from some of the validations you have done with it that it is working and does seem to work fairly well.

On the inputs into it, I would personally like to see if -- just be sure that some of those practical soil things get in there where the fumigant's actually working to be sure that all those factors come into play, the weather stability, another consideration there. I think it is being handled fairly well there.

Some of the things that would be nice to have that are hard to get a handle on, and I don't really know how you do it, if you look at things like soil degradation as well as the atmospheric dispersion and degradation.

The things for routine reporting looked to me like you are doing a pretty good job, what the model is doing and the outputs from it.

I think things like flux rates and fumigant concentrations and exceedance frequencies and distances from the source at which exceedances occur and maximum daily emissions losses over time through emission and in atmosphere, all those are important.

As far as further evaluation, I would have to agree. You can evaluate something to death. You keep evaluating and evaluating and people always want more.

But I think there would be a need to evaluate this model and validate it with different types of fumigants to find out if it really does work well with other types of fumigants under other conditions.

And particularly, using as much real data as is available and see if the model could be used as a good tool for and a good regulatory tool and a risk assessment and management tool for other fumigants in other areas.

And I don't care to comment on the statistical stuff because I'm out of my league there.

DR. HEERINGA: Thank you, Dr. Shokes.

Dr. Potter is scheduled as the next discussant on this question.

DR. POTTER: I think my colleagues have provided a rather exhaustive list of things to do. So I will try to keep mine brief and focus in specifically on item C.

I think it would be insightful if you could mix the model runs using emission profiles with different shapes.

I think we heard yesterday that really the driver in the chronic risk is cumulative loss rather than instantaneous loss or at least the variation in instantaneous loss associated with a particular application.

It is a natural step forward, I think, and perhaps the work has already been done, but it would be very insightful to see some effort to really use the model to answer that particular question.

That also might help, I think, myself and I think probably many of my colleagues on the panel get to some kind of comfort level in terms of this idea of using a generic flux curve to calculating emission rates.

I think that would be a very productive thing to do.

One other comment in terms of data handling, I made several runs with SOFEA. And I think in your directions you said that you needed to save each sheet, each run separately, otherwise, it overwrites the output files.

But I did that. And then I renamed it. And then I would open it back up and then it would start up and then the screen would turn blank. So maybe I made a mistake there or perhaps there is an error in that.

DR. VAN WESENBEECK: I believe that's probably an idiosyncracy of Crystal Ball. You have to set Crystal Ball so that it asks you or set Excel so it asks you whether you want to enable or disable macros.

When you rename the spreadsheet anything but

SOFEA version 1, when you open it, you have to disable the Crystal Ball macros. Otherwise, it gives you that blank screen.

Even when you disable macros, you can still go in and manipulate all of the data.

DR. POTTER: If I go back into that file that I renamed and resaved, all I have to do is disable macros at the front end, I should be able to see it. Is that correct?

DR. VAN WESENBEECK: That's correct.

DR. POTTER: That's all I have.

DR. HEERINGA: Thank you, Dr. Potter. Are there any other comments from other members? Dr. Yates.

DR. YATES: I wanted to I guess go on record. I didn't mean to say that the model shouldn't be evaluated anymore. It was more that I thought that a good start had been taken on evaluating the model and I really couldn't think of anything different that they should do.

It seems to me that any future evaluation would really follow along the same lines of what they have been doing with new data or, you know, like, for example, what

Dr. Potter was saying, maybe a different flux study as the input.

But in terms of something that they missed, I couldn't see anything at least up to this point that has been missing in terms of their evaluation.

DR. HEERINGA: Let me throw out a couple things just to make sure we have covered this. One of the issues that has come up repeatedly is the issue of the time period for some of the inputs to the ISC model.

We are using, I guess they are fixed at hourly inputs, but we're currently using sort of a uniform sixth hour values on input. Is that correct, Dr. Van Wesenbeeck?

DR. VAN WESENBEECK: For the flux input?

DR. HEERINGA: Yes.

DR. VAN WESENBEECK: Yes. We're using six, six and 12 hour inputs, but then it is split into hourly.

DR. HEERINGA: Same value each time.

DR. VAN WESENBEECK: Same value each time.

DR. HEERINGA: Is the same true with the meteorological inputs?

DR. VAN WESENBEECK: The meteorological inputs are just the actual data from CIMIS or --

DR. HEERINGA: Hourly.

DR. VAN WESENBEECK: Hourly.

DR. HEERINGA: Thank you very much. So I think there is one area there potentially to look at. What would happen if you used different flux profiles, which would have varying hourly inputs, more of a step function.

And then also possibly consider in a sensitivity analysis. What actually happens with your cumulative results. And maybe later with acute results or shorter duration time periods if you added some stochastic variability to those, even those profiles.

I think the previous two models we have reviewed had capability for that looking again at the acute short term exposure.

Dr. Cohen.

DR. COHEN: One additional evaluation procedure that you could do and I think you probably have already done it, on page 17 of your presentation, you showed the validation at each sampler location. Essentially, this validation -- during your field studies where you put the samplers out and you measured.

Now, you have just shown us data for two of the samplers and yet there were eight samplers.

You have also shown us ones that looked to be not in the prevailing wind direction, although perhaps during your study the prevailing wind went the other way.

I thought in the description of your study you said the wind went from northwest to southeast. But then you are showing us some southwest, which could have been -- the wind could have been blowing in that direction during your study.

I guess one of the things is, and we all do this, modelers, we tend to show results that are the best. I'm just wondering what the results for those other eight receptors looked like.

And I guess in any evaluation, full disclosure is probably the -- can be painful, but the best way to do it.

DR. HEERINGA: One other item which I would like to bring up and maybe get assistance from some of our experts here, the issue of calms and how they are treated, I think particularly as we think about going to shorter term acute exposures in all of these models.

Is there a sensitivity test that could be designed for the ISC model imbedded in SOFEA that would, in fact, represent a conservative set of assumptions about exposure in contrast to the current regulatory default for treating calms?

In other words, if we maintain -- I'm going to stick my neck out here. If we maintained a stability condition, in other words, a high stability condition, but input a low velocity wind, would that produce off-site exposures that would tend to be more, conservatively higher or higher end? Dr. Cohen.

DR. COHEN: The treatment of calms is extremely difficult. And I don't think there is going to be an easy work around to that within the ISC model.

One approach that I have heard of, which I'm not necessarily advocating, but one approach I have heard of is that when you have a calm hour and then you go to the next hour, what you would do is double the source strength for that next hour.

You are sort of saying the stuff got emitted in the calm hour and it kind of just stayed there and then the next hour when the wind blew, it blew the previous hour's stuff plus the new hour's stuff out to the receptor.

I actually don't think that will give you a high enough concentration, but at least it is one step toward trying to add a little bit of realism to this problem.

DR. HEERINGA: Thank you very much, Dr. Cohen.

Dr. Spicer.

DR. SPICER: Well, in carrying forward with that idea, of course, it would be a simple matter if you got flux estimates at each hour just to simply reserve the flux in the calm period and put it in the next as opposed to doubling it.

That would be a little more logic to that. Obviously, any sort of ad hoc technique would be exactly that and would need to be verified as best possible with available information.

DR. HEERINGA: I guess we recognize that that departs from the sort of regulatory guidance for the use of the ISC model.

But in terms of a sensitivity analysis and one of the major issues that has come up with regard to the micro meteorology, if that's the term, if that's correct, picking up these things as I go here, in local conditions, that it might actually be an appropriate sensitivity analysis to run to look at that major assumption.

It is one that has come up in each of these three sessions.

Dr. Arya.

DR. ARYA: With regard to the suggestion that may -- the basic limitation whenever you encounter wind speed -- low wind speed, if you are going to use ISC and the dispersion curve ISC uses, there is limitation, really.

There is no -- like if these conditions occur at nighttime, which often they do, calm conditions, there is no stability category they can use. There is no dispersion coefficient in the ISC model they can use.

There is no way to treat winds during nighttime which are actually even lower than two meters.

DR. HEERINGA: Thank you very much, Dr. Arya.

Are there any additional comments on question 8 that the panel would like to make?

Mr. Dawson, are there points of clarification you would like to seek on this?

MR. DAWSON: No. Dr. Johnson has --

DR. HEERINGA: Absolutely.

DR. JOHNSON: In 8D, the second half of the question on statistical techniques, I'm wondering -- there are two cases that we have talked about. One case where we have measured values and we want to compare those measured values to the model output. And we saw an example of those measured and modelled values in one of the graphs, the pseudo evaluation.

And the second case where many of the panel members are suggesting various permutations to running the model, for example, varying the period flux, let's say, but keeping the same cumulative flux and finding out how that affects the output.

In that case, you would have two cumulative distributions relatively continuous, let's say, because the model gives you such a large number of output points.

How do you compare -- the question -- the input -- what I'm interested in knowing is suggestions for statistical techniques to compare the two outputs in those situations, the case where you have measured, say, a finite set of discrete measured values versus the output for the model and the case where you have two different outputs from the model.

DR. HEERINGA: I think we have heard the question. And the wheels are turning. Let me take a shot at it. Sometimes by sort of throwing an idea out there we stimulate better ideas.

You are really asking for how do you compare distributions derived under two different methods, each with their own variability. Distributional comparisons even testing a set of data against a hypothesized model distribution, there are formal tests for those.

And unfortunately, they tend to be extremely powerful tests. And that is they tend to reject almost uniformly, in fact, I don't even recommend them because almost invariably will come back with an answer that, no, this is not a normal or a log normal distribution.

I'm thinking about things like Cormigrov Smearnoff (ph). Interocular tests work pretty well with graphics. But then you also need to have some measure on variability and bounds on the curves themselves.

I think that the difficulty with the formal statistical tests here, particularly in the extreme tails of these distributions, is that they are extremely powerful against the alternative hypothesis that these are not the same in distributions.

I don't know, Peter, do you want to offer anything? I don't want to put you on the spot, but --

DR. MACDONALD: That's pretty much what I say about them word for word. This gets back to the question. Is the model good enough for regulatory purposes? And for regulatory purposes, it doesn't have to mimic real behavior exactly.

Your distributions don't have to follow specific distributions exactly. But they have to be good enough to make a sensible recommendation.

And now we're getting a little more into art rather than statistics at that point.

DR. HEERINGA: I think my recommendation at that point is to ensure that you have run the simulation in a sufficient number of iterations and independent replications to make sure that at least under the assumptions inherent in the model process that you are achieving stability of the simulation distribution.

Obviously, the field sampling data is subject to sampling variability. You could obviously do computations directly there. So with regard to the field sampling data, you could put sort of variance based error bounds on it.

But I think the graphical presentations and the graphical examinations would probably be the best tool. And to identify any one statistical tool in this context, really evaluate yes or no, are these the same, I would recommend against that approach. It requires some professional judgment on that point.

Dr. Arya.

DR. ARYA: I would like to comment on the model uncertainty. There have been some studies where the results of Gaussian type models have been compared against very good field experiments, you know, from point sources, normally.

And I think like everybody is aware of that, a typical uncertainty of the Gaussian type models for short range concentration predictions usually often this factor of 2 is quoted.

They indicate that even with Gaussian models, with Pascal Gifford dispersion curves, only 50 percent of the observations may lie within a factor of 2. Other 50 percent lie outside of factor of 2.

DR. HEERINGA: Dr. Johnson, I'm not sure we gave you a take away answer that you can just --

It is very clear that statistically there is no single answer to this question. And that sort of the shorthand solution of finding a statistical test to evaluate the comparability of two distributions, particularly in the upper tails, is sort of a very risky and not often practiced business in my experience.

At this point, are there any additional comments or points of clarification on this?

DR. COHEN: Just a comment. This has been brought up before. But when you are trying to compare the distributions, and you will do this, of course, you do want to look at the high ends. And so a statistical approach might not even necessarily weight the high ends.

It doesn't make any difference if you get the low end right because they are just so much below the level of concern that it is from a regulatory point of view you don't really care.

So when you are looking at any of these results, and I'm sure you are doing this already, but when you look at the results, and you look at how is it doing at the high ends. That's your key question.

DR. HEERINGA: Dr. Macdonald.

DR. MACDONALD: Just following on this discussion, really. Rather than comparing the whole tail, really, just comparing one or two selected quantiles is probably going to be more useful and get you get back to a univariate measure which makes the sensitivity analysis involving a large number of factors much easier.

DR. HEERINGA: Any additional comments on question eight?

At this point, I think I would like to conclude our discussion of the charge questions.

But before we wrap up today, I want to make sure that we have gone back and provided opportunity for not only Mr. Dawson and Dr. Johnson to make sure that we have covered the points that they would like covered, but also for the panel members to make any final comments that they would have with regard to the SOFEA model and material that we have covered in the last two days.

Mr. Dawson.

MR. DAWSON: No. I think we're very happy with the results of the discussions.

DR. HEERINGA: At this point I guess I would like to turn back to the panel, then, and see if there are any general comments related to the exposure modeling either chronic or the application of the SOFEA model to acute modeling or the SOFEA model in general.

Mr. Gouveia, any additional final comments.

DR. GOUVEIA: Nothing I have already said. Thank you.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: I just have one more comment just to make sure it gets into the record. Going back to this page 17 where you are looking at the validation of each sampler in your field test of the two samplers that you showed, I just wanted to point out that when you look at the graphs, you know, they look pretty good. And you certainly see some diurnal, the same diurnal variations, and the model is clearly capturing a lot of the dynamics of the situation.

However, systematically, it seems to me that when there is a difference it is almost always true that the measurements are greater than the model.

And in the tail of the -- after many, many days or several weeks, it may be the difference between two small numbers.

But what concerns me most in the period, in the couple days after the application where you are getting the highest concentrations and, yes, there are some days when it appears that the model's getting almost exactly the right answer, that almost looks to be too good of agreement, but then there is at least one day in each of the cases where you are underpredicting by a fairly large fraction, like a factor of three or something, if I'm reading the graph right.

So I guess this goes to this general point that we have all been making, that you may not be getting high end of the concentrations.

DR. HEERINGA: Thank you, Dr. Cohen. Dr. Potter.

DR. POTTER: I have nothing.

DR. HEERINGA: Dr. Winegar.

DR. WINEGAR: Just a quick response in terms of Dr. Cohen's comment.

This reminds me of the discussion we had with the other two models about the correlation between the measured and the flux or the measured flux and the modeled flux and the acceptability of different -- of the R squared value.

And there wasn't really -- we didn't come up with a definitive answer what is good. But most of the panel seemed to be pretty comfortable with even R squared as a .5 or less. That's my only comment at this point.

DR. HEERINGA: Dr. Ou.

DR. OU: Don't have any additional comment.

DR. HEERINGA: Thank you very much. Dr. Majewski.

DR. MAJEWSKI: I just want to say that I have come to a new appreciation of models. And this one I think -- well, they have all been very thorough and I think they do their jobs well.

And I can appreciate the difficulty of balancing the field work, getting the flux studies and the appropriate sampling periods requirements for the models.

I know there has been some discussion about having one hour sampling periods, and I have personally have done two hour sampling periods for three days, and I'm still here. It is survivable.

But I don't think a one hour period would be survivable or possible. And another thing is I'm not sure this is the case with 1,3-D, but detection limits start playing an important part with the shorter sampling periods.

DR. HEERINGA: Thank you, Dr. Majewski.

Dr. Yates.

DR. YATES: Actually, I didn't have a comment until Dr. Majewski just made me think of something.

The one hour sampling periods would be a problem if a person is going out to the field and installing a new Orbow (ph) tube or whatever each hour.

But thinking back to the way that the flux measurements we're taking with some sort of an automatic system where there's three these tubes put into the box, it would be possible to use a system like that that would allow maybe going out in the field every three hours.

I think in one point we had a system like that allowed up to five samples to be taken automatically. Then you would be able to go into the field once every five hours.

So it could be possible with a little bit of automation to be able to get one hour samples without killing the graduate students.

DR. HEERINGA: Dr. Maxwell.

DR. MAXWELL: I want to commend the gentlemen that came here to present the information. I think it was very informative. I want to also applaud you for coming up with a model that evaluates acute and chronic exposures, which I think is very critical.

So I know that you have a lot more people involved than the three of you, but I think that's a wonderful thing that you are doing. We appreciate the opportunity to evaluate the model.

DR. HANDWERGER: I'm used to modeling DNA and protein. So this was a very educational experience. Thank you.

DR. ARYA: And even though I'm interested in dispersion models, but this was my first exposure to this kind of application, soil fumigation, and it was a very great learning experience.

I have no additional comments. It was nice to meet you all.

DR. SPICER: I guess that means I get to be controversial. Is there a legal definition for bystander?

MR. DAWSON: A legal definition for bystander, no.

DR. SPICER: That's why I was curious.

Because it seems -- the two panels previous to this seemed to essentially define a bystander as someone standing at the buffer zone in application of a field aiming, of course, towards the acute exposure.

So you have the problems that we discussed before associated with that with the fluxes being underpredicted associated with calms and the measurement issues and that sort of thing.

And in that sense, I think ISC probably was a reasonable choice. But I have got personally deep reservations about use of ISC under these circumstances for the chronic exposures, the issues associated with actually what the airshed is, how it is different from the township and then the fact that ISC uses these hour vectors.

And I guess ultimately one thing that I would be -- would hope that would come out of this is, and I think I mentioned this in one of the previous questions, was looking at the sensitivity of the receptor grid when you look at these chronic exposures.

Because I think that the sparse grid that's used presently, although it is user input, I recognize that, it doesn't seem like you looked at the variability of it, that is a potential concern.

And then I guess the other comment that I have is about the structure of this. I recognize that what was attempted here was to have case studies that the people who were developing the case studies were interested in the modeling methodologies.

But from a comparison point of view, and this may be impossible from a political point of view, but for a comparison, it would have been helpful for example to have had your tool applied to at least one of the other fumigants in the other case studies and vice versa so that there would be some basis for comparison.

Because right now all three tools are different and all three fumigants are different. They have different characteristics. It does make it more difficult to do a comparison.

In fact, we have ended up, I'm afraid, leaving you a very difficult task. May be solving some things, but certainly raising others.

DR. HEERINGA: I think Mr. Dawson would want to comment too.

I think it was the intent from the very beginning with the SAP not to set this up as a comparison of at least a side by side evaluate comparison of the three models, but independent evaluations with the obvious overlaps in terms of inputs and potential down-the-road uses.

MR. DAWSON: Correct.

I would just like to mirror Dr. Heeringa's comments that that was our intentional plan. And for those of you that follow our program per se, we envision this process in a way that's very analogous to the way that we have handled the situation with the dietary models where first we need to see what tools we have and what are the specific thoughts on the tools in general.

And then move potentially to the next step that you are describing, which is kind of a comparative analysis and see how the tools respond under different conditions with the various data sets for the different cases that we're looking at.

So that's kind of next on the boards for us.

DR. HEERINGA: Dr. Hanna.

DR. HANNA: I have no further comments.

DR. HEERINGA: Dr. Macdonald.

DR. MACDONALD: No further comments.

DR. HEERINGA: Dr. Shokes.

DR. SHOKES: I have very little comment. I want to say after the acute exposure to the first two models, I feel like it is chronic now.

But I do appreciate some of the things that went into development of this model as in the others. It is a tremendous amount of work, a tremendous effort.

It is easy to sit and hear and evaluate that and say, well, you should do this or you should do that. And we look at the practical aspects that Dr. Majewski pointed out that some of these things are not easy to do out in the field.

I also think it would probably, if I were the one developing the model, it would be a wonderful thing to sit in a room with a group of people like this beforehand and get those inputs as to what should go into it.

And I still think when they evaluate it, they would still point out other things that needed to be done. But I think it would be helpful to have a brainstorming session with people that have such expertise.

But I don't think anybody could afford to do it.

DR. HEERINGA: Paul.

DR. BARTLETT: I want to say I really appreciate the work that has gone into this and many dimensions that's been added this model. I would also like to echo what Dr. Spicer said.

I'm involved in a comparative modeling group that's a multi-year program on long range transport of semivolatiles. And I find it very rewarding and illuminating. And I think all our models are improving as a result of it.

And much more transparency in how they work by basically choosing analyzing -- well, looking at each other's parameterizations, making comparisons. And then our next, our third stage is going to be using the same domain, the same weather data and see you how they work.

So I think that's a good way to go.

DR. HEERINGA: I would like to again extend my thanks on behalf of the SAP to all the members, the panelists, to the EPA staff who are here to assist with the presentation discussion and to the Dow Ag Sciences group too for their presentation of the SOFEA model.

At this point in time, Mr. Dawson, do you have anything?

MR. DAWSON: I would just like to mirror those comments. We really appreciate the work of the panel, the time that you have taken out of your busy schedules to be involved in this project with us.

I feel the last couple of days' worth of work have been very thoughtful and have provided us with an exhaustive examination of the model.

Also to you, Dr. Heeringa, for chairing, we really appreciate your efforts. And to the people from Dow, we, of course, appreciate your efforts, the time and the very quick schedule to pull this all together.

And finally, to the SAP staff for their help in assistance for setting this meeting up. And Dr. Johnson who has been very intimately involved in helping us prepare for these efforts over the last four to six months.

Thank you very much.

DR. HEERINGA: At this point I would like to turn to our designated the federal official, Joseph Bailey, if you would have any final comments.

MR. BAILEY: I think that Jeff has certainly covered thanks for everyone that I intended to mention here.

And I do appreciate the panel's participation in providing for a very engaged discussion and very thoughtful comments provided to the agency on these questions posed.

Thank you.

DR. HEERINGA: At this point in time before we draw the meeting to a close, just ask the panel members if we could meet briefly in our breakout room to make sure that we are in agreement on schedules for the preparation of our written summaries of our comments and development of the minutes of this meeting, our final report.

The rest of you, have a good afternoon, safe travels, and thank you for your participation.

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[Whereupon, at 1 p.m., the
meeting concluded.]

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