FIFRA SCIENTIFIC ADVISORY PANEL (SAP)

OPEN MEETING

AUGUST 24 - 25, 2004

FUMIGANT BYSTANDER EXPOSURE MODEL REVIEW:
PROBABILISTIC EXPOSURE AND RISK MODEL FOR FUMIGANTS
(PERFUM) USING IODOMETHANE AS A CASE STUDY

TUESDAY, AUGUST 24, 2004

VOLUME I OF II

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Reported by: Frances M. Freeman, Stenographer

C O N T E N T S

DR. ROBERTS: My name is Dr. Stephen Roberts. I'm here to serve as Chair for today's session.

The topic today is Fumigant Bystander Exposure Model Review: Probabilistic Exposure and Risk Model for Fumigants (PERFUM) Using Iodomethane as a Case Study.

I don't know if the titles of these keep getting longer or not, but it's certainly a very interesting topic. And the SAP staff have assembled an outstanding panel to address this issue.

I would like to start first by introducing the panel members. So let me, starting with Dr. Heeringa on my left, ask each of the panel members today to briefly introduce themselves giving their name, their affiliation and the expertise they bring to today's deliberations.

DR. HEERINGA: I'm Steve Heeringa. I'm director of the Statistical Design Group and a research scientist at the Institute for Social Research at the University of Michigan.

I'm a biostatistician and a permanent member of the SAP panel, and I'll be chairing the Thursday and Friday sessions.

DR. PORTIER: I'm Ken Portier, a statistician with the Institute of Food and Agricultural Sciences at the University of Florida, and a permanent SAP panel member.

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

DR. SHOKES: I'm Fred Shokes. I'm a professor of plant pathology and director of the Tidewater Agricultural Research and Extension Center in Suffolk, Virginia, a component of Virginia Tech.

DR. SEIBER: I'm James Seiber. I'm director of the Western Regional Research Center, a USDA Agricultural Research Service operation in Albany, California. Formerly with the University of California, Davis.

DR. MAXWELL: Good morning. I'm Dave Maxwell with the National Park Service Air Resources Division in Denver, Colorado. I'm a meteorologist and manage air quality monitoring projects to the tune of about 10 million dollars in visibility and air quality monitoring.

DR. WANG: I'm Dong Wang. I'm associate professor of environmental biophysics with the University of Minnesota in the department of Soil, Water and Climate. Formerly with USDA ARS, Soil Salinity Lab, Riverside, working with fumigants about 10 years modeling, mostly; some field experiments.

DR. WINEGAR: My name is Eric Winegar. I'm the principal of Applied Measurement Science. My background is in monitoring and measurement, analytical chemistry and exposure assessment.

DR. OU: I'm Li-Tse Ou. I'm a scientist with the University of Florida. My area of expertise is the fate of the pesticides in the soil.

DR. SMALL: I'm Mitchell Small. I'm a faculty member at Carnegie Mellon University in Pittsburgh. I'm in the departments of civil and environmental engineering and engineering in public policy. My areas of expertise include environmental modeling and statistics.

DR. MAJEWSKI: I'm Michael Majewski. I'm a research chemist with the U.S. Geological Survey. My background is in the atmospheric environmental fate of contaminants.

DR. BAKER: Hi. I'm Dan Baker with Shell Global Solutions in Houston. I have been there 23 years. The latter half I have been working on emissions and air quality issues.

DR. BARTLETT: I'm Paul Bartlett, Queens College, City University of New York. My area of expertise is air transport, modeling and mission factors, semi volatiles and other organic contaminants.

DR. SPICER: I'm Tom Spicer, professor and head of chemical engineering at the University of Arkansas. My field of expertise is atmospheric dispersions, specifically for episodic sorts of accidental releases of contaminants in the atmosphere.

DR. YATES: I'm Scott Yates. I'm currently acting research leader of the Soil Physics and Pesticides Research Unit at a USDA ARS research facility in Riverside, California.

My area of expertise is fate and transport of pesticides and fumigants in general in soils and volatilization into the atmosphere.

DR. ROBERTS: Thank you. And I'm Steve Roberts. I'm a toxicologist at the University of Florida.

Our designated federal official today is Myrta Christian. Let me turn the meeting over to her for some announcements.

MS. CHRISTIAN: Thank you, Dr. Roberts.

I'm Myrta Christian, and I will be serving as the designated federal official to the FIFRA Scientific Advisory Panel for this meeting.

I want to thank Dr. Roberts for agreeing to serve as chair to the FIFRA Scientific Advisory Panel for this meeting.

I also want to thank both the members of the panel and the public for participating and attending this important meeting of the FIFRA SAP to review the Probabilistic Exposure and Risk Model for Fumigants (PERFUM) using Iodomethane as a case study.

We appreciate the time and effort of the panel members in preparing for this meeting taking into account their busy schedules.

By way of background, the FIFRA SAP is a federal advisory committee that provides independent, scientific peer review and advice to the agency on pesticides and pesticides related issues regarding the impact of proposed regulatory actions on human health and the environment.

The FIFRA SAP only provides advice and recommendations to EPA. Decisionmaking and implementation authority remains with the agency.

As the DFO for this meeting, I serve as a liaison between the panel and the agency. I am also responsible for ensuring provisions of Federal Advisory Committee Act are met.

As the designated federal official for this meeting, a critical responsibility is to work with appropriate agency officials to ensure that all appropriate ethical relations are satisfied.

In that capacity, panel members are briefed with provisions of the federal conflict of interest laws.

In addition, each participant has filed a standard governmental financial disclosure report. I, along with our deputy ethic officer for the office of prevention, pesticides and toxic substances, am in consultation with the office of general counsel have reviewed these reports to ensure all ethic requirements are met.

For members of the public requesting time to make a public comment, please limit your comments to five minutes unless prior arrangement have been made.

For those that have not preregistered, please notify either myself or another member of the SAP staff if you are interested in making a comment.

There is a public docket for this meeting. And all background materials, questions posed to the panel by the agency and other documents related to this SAP meeting are available in the docket. Overheads will be available in a few days.

Background documents are also available on the EPA web site. The agenda lists contact information for such documents.

At the conclusion of the meeting, the SAP will prepare a report as response to questions posed by the agency, background materials, presentations, and public comments.

The reports serve as meeting minutes. We anticipate the meeting minutes will be completed in approximately eight weeks.

Again, I wish to thank the panel for their participation. I am looking forward to both a challenging and interesting discussion over the next two days. Thank you.

DR. ROBERTS: Thank you, Myrta.

I'm pleased to see that we have with us this morning the director of the office of pesticide programs, Mr. Jim Jones. Good morning, Jim, welcome.

MR. JONES: Thanks, Dr. Roberts.

I want to thank all of you for willing to serve on this panel today and tomorrow. And for those of you who will be here on Thursday and Friday, I want to thank you as well in advance.

Two of the hallmarks of the agency's work in the office of pesticide program are transparency and sound science. I mention that because two of those characteristics are part of the reason why we're here today.

Transparency, in the sense that we try to do our best to do our business in front of the public. And we think that that's very important that the business of the agency be conducted in a way that the public can watch what we're doing and participate in that.

Sound science, in the sense that we don't feel at EPA that we have all the answers we often feel. It is very important for us to reach out to individuals with expertise specific to the issues that we're dealing with and get independent peer review of our work.

And those two are the very important reasons why we're here today and tomorrow and, for some of you, for a little longer than that.

I'm going to give a little bit of the regulatory context within which we are operating. I realize that you are here to provide scientific expertise, but I think it is very important for you to understand the general context within which we're operating around the chemicals that this work is going to apply to.

The agency has an old chemical program, and we have a new chemical program. There are a number of fumigants that are in our old chemical program. And within the next year or so, we're going to be making regulatory decisions, decisions around their safety and the ultimate regulatory disposition of those chemicals in the next 18 months or so.

We also have before us a new chemical. Actually, today's analysis uses that new chemical as somewhat of a test case that's a fumigant. And that chemical is also before us for a licensing decision.

The agency has made a choice to look at these fumigants all at the same time so that we're not just trading off potential risks from one to another. We're looking at all of them at the same time so we can make a comprehensive logical risk management choice around these fumigants.

We have convened a number of SAPs following the Food Quality Protection Act to help us deal with some of the very difficult hazard and exposure issues associated with pesticide risk assessment.

Today's issues aren't going to be about hazard, obviously. They are going to be about exposure. Some previous SAPs have really helped us as an agency, as an office, to sort through some very complex exposure issues associated with how to estimate exposure of pesticides in food when you have 12 or 13 chemicals that may share a common mechanism of toxicity. Those are some of the probabilistic SAPs we had around the organophosphates.

The SAPs also helped us sort through how to measure pesticide exposures through the source of drinking water when there is huge degree of variability in pesticide use in the United States and how pesticides may enter drinking water.

Today, we're talking about today and for the next four days, two for many of you, two more for some others, and in another meeting we're going to be having in about two weeks, we're going to be talking about a completely different source of exposure.

That is the exposure to what we refer to as bystanders. Bystander I'll describe simply as an individual who is near or around a treated field, that treated field being a field treated with one of the fumigants that we have in front of us for regulatory decisionmaking.

We certainly do have measured estimates or measurements, actual, of these chemicals. But those measurements tend to be somewhat limited in that we don't have as many data sets, not as robust as you optimally would like it to be.

So what we're exploring are potential models to help us better characterize the exposure to bystanders from these fumigants. And better characterization through this route of exposure will certainly enhance the agency's decisionmaking as it relates to these chemicals.

And so basically, that's what we are here for the next couple of days is to help get some advice about some of the models that are available to the agency in its efforts to estimate exposure to bystanders, individuals who are near or around treated fields, fields being treated with these fumigants.

I'm confident that the advice that we get, not only from this panel, but from the two subsequent panels that are going to be looking at some of these models, will be instrumental in the agency's ultimate determination as to how to estimate exposure to bystanders from these compounds.

So I thank you all for your service. I know that you all have very busy and active professional lives and this is no small endeavor for you to come and, not only sit for two days, but to invest the time and energy it takes to review the scientific documentation before these meetings and the work that goes on after these meetings to ultimately write the reports. I want to thank you all for your service.

And, thank you, Dr. Roberts, for your chairmanship.

DR. ROBERTS: Thank you. I think those remarks help the panel place our discussions today in the right perspective, so we understand that perspective.

We also have with us the director of the health effects division of the office of pesticide programs. I would add a veteran of many SAP meetings, Margaret Stasikowski. Welcome.

MS. STASIKOWSKI: Thank you. And actually, this maybe the last SAP that I'm at. I'm leaving, some say retiring, but I'm actually changing careers. So I will be leaving the agency on October 1st.

And one of the things that I will really miss is the Science Advisory Panel and the advice that you have provided us during my tenure, which has been eight years as the director of the health effects division.

And you have done a wonderful job helping us addressing some of the cutting edge risk assessment issues that for the last eight years as we implement FIFRA and FQPA.

Today, over the next three meetings, this week and early in September, we are asking you to look at three different modeling approaches for assessing exposure from soil fumigants.

We are asking you to review this in a similar way, independently, this is not a comparative model assessment, in a similar way that you looked at the CARES, Calendex and Lifeline probabilistic dietary exposure models.

You have done a wonderful job. We are using the models and have improved them significantly as the result of the input from the Science Advisory Panel.

So today, we are asking you to review PERFUM, Probabilistic Exposure and Risk Model for Fumigants. That's today and tomorrow.

That presentation will be made by Dr. Rick Reiss, who is making a presentation on behalf of Arvesta Corporation. He was a consultant to Arvesta.

Thursday and Friday we are asking you to review Fumigant Emissions Modeling System. The presentation will be made by Mr. David Sullivan from Sullivan Environmental Consulting.

And early in September we'll be asking you to review SOFEA, Soil Fumigant Exposure Assessment System. I think that's a very interesting acronym.

Without further ado, I would like to introduce Jeff Dawson, one of our most seasoned exposure assessors in health effects division who will give some introductory remarks.

But before that, I would like to introduce two people sitting here to my left who are here from California Department of Pesticides Regulation. And we really are very happy with the fact that we are working very closely in this issue of fumigants risk assessment with California.

California has so much more experience in developing the approaches to fumigant risk assessment that we really couldn't do this without them.

So I would like to introduce Dr. Terri Barry and Dr. Randy Segawa from California Department of Pesticide Regulation. Thank you.

DR. ROBERTS: Dr. Dawson, I think you are next up.

MR. DAWSON: Thank you.

Thanks, Margaret for the introduction.

What I like to do today is give a 15-minute primer or so to help set the stage for the scientific aspects of this discussion. So if everyone will look up there at the screen.

Basically, what we're going to do is talk about these four topics very quickly. I'm going to give you a little bit of information about the background for the science that we're going to be talking about.

I'm going to talk about our current methodologies for looking at fumigants. So that will be a good basis for you to compare how this model may differ and in what aspects.

And then a very brief summary of the PERFUM model. Because you are going to hear a lot about that in much more detail from Dr. Reiss right after my presentation.

Then we'll talk a little bit just about the general theme of the charge questions. As we move later into the meeting, we'll read the specific charge questions.

As far as the background information, we'll touch very quickly on the modeling approaches, the source of the case study that we're looking at today, the purpose of the model and our ultimate goal with this meeting.

So as you just heard, we're looking at these three different models over the course of the next three meetings up until the middle of September. And again, we're focusing on the PERFUM approach today.

The PERFUM approach is -- what we're going to be doing is looking at a case study based on iodomethane, which is a chemical, as Jim Jones said, we're considering for registration and licensing.

And the specifics of this case study we're looking at are based on a variety of field monitoring data. And the last bullets, few bullets there basically summarize the types of data that we have available to us for the basis of this modeling.

Basically, what they are is they are emissions data that are representative of different types of application methods shank injection with a flat fume, shank injection with a raised bed type of application, and then a drip irrigation method with a raised bed. For example, like you would have with growing strawberries.

So the purpose of this model, and Jim did an excellent job of really explaining why we're interested in these models, and that is to have a better understanding of the distributions of bystander exposure after an application of fumigants, soil fumigants such as this. And we're really interested in using these to help us characterize the higher end exposures.

And also we would really want to use these tools, I think, to look at how uncertainties and variability affect the exposure levels.

And so our goal here is manyfold, but, basically, we're interested in the scientific validity of the model, how transparent is it to go from the inputs to the outputs and understanding the whole process, understanding what types of data are required to operate the system, looking at how systems such as this might be used for evaluating exposures across the country in different growing regions, different kinds of crops, and also understanding how portable this system might be for looking at -- using the methodologies here for looking at other chemicals. For example, the six soil fumigants we're looking at over the next year or so.

So our current approach is very similar, basically, more or less identical to what California Department of Pesticide Regulation is using. And it's based on an agency model called the ISCST model.

And we'll talk a little bit about the inputs we use in our current approach and then the outputs that we get from that.

So many of you on the panel are familiar with this model, but the ISCST model or the industrial source complex model was developed by the Office of Air and it is routinely used for their permitting programs and routinely used in regulatory decisionmaking.

It is a steady state Gaussian plume approach. And it can look at all different types of sources. For example, it can look at point sources, which are things like smoke stacks, linear sources, so you might look at pollution from a roadway where there is a lot of traffic, and area sources.

And in this case, we're using treated farm fields as our example. And we're using it to deal with farm fields as an area source.

As I just said, DPR uses this model as well. And for those of you who are not aware, this model is publicly available. You can go to that web site right there and download the system and all the documentation associated with it.

So the inputs that we routinely use, they basically fall into five categories. And the first three are listed here. For example, for field size and geometry, and geometry is just the shape of the field that we're using, we use a range from 1 to 40 acres. Again, that's similar to what DPR is doing.

The shape of the field we're using is a square. And we look at varied atmospheric conditions. We basically go to the lowest wind speed that's allowable in the model up to about 10 miles an hour. And we look at varied environmental stability. That's just a measure of turbulence in the atmosphere. So we look at a range of inputs that go from a calm day up to a reasonably turbulent type of day.

DPR uses, on that one bullet there, that's the inputs that they have used. For example, for their methylbromide permitting, to use a specific set.

The third major category of input is the different application equipment and what are called control technologies. That's just basically categorizing the types of data we have available for field monitoring. For example, in this case, we have data for drip and irrigation and shank injection methods. And then different -- in addition to that, different methods for reducing or trying to control emissions. For example, tarping and the use of raised beds as an approach.

Based on the data we're looking at in the case study, the next category is field emissions. This is the actual emission data that we have used for the case study.

And from the data we had available, we calculated constant, what are called, flux rates, which is an emission from a treated field in this case. And the actual numbers run from 66 to 107. And the units are microgram per square meter per second. That's just the surface area on the field and the amount coming out per second.

For the categories of application equipment, sorry, I just showed you, we calculated flux rates for each different combination of data that we had available.

And what we saw in this case is that drip irrigation was the lowest emitter, and that the highest emitter was the shank injection flat fume approach.

And then the first four inputs are really more chemical specific type of inputs and more scenario specific for this. But there is also other settings and parameters within the model that you would routinely set for whatever kind of analysis you are doing.

These are just a few examples of what we have done in our assessment. We use rural conditions. We treated it as an area source. And because it's a treated farm, we're using a release height of zero meters. So the emissions are released right at the surface of the field.

This slide just shows the kind of outputs that we're getting from the model. You can see our treated field there on the left, a square. And then basically what we're doing is we're modeling the wind direction going downwind 100 percent of the time.

So we're making the assumption that the wind direction is not changing. And then we're calculating air concentrations on that receptor grid, I'll call it, on the right.

So at those different locations we would get some sort of a calculated air concentration at various distances downwind that we use for our risk assessment.

But the key to take away from this is that one of the parameters we're using is that that wind direction is 100 percent downwind all the time.

And this is just what a table of results might actually look like in an assessment. And this is just something I extracted from the charge document that's available on the web site.

And what you have here are different distances downwind in the second column. For example, I presented from 25 meters down to 1,000 meters downwind. And then as you go across the columns there, you see the air concentrations.

And the reason the columns are different, as you go from left to right on the columns, you go from a calmer day to a much more turbulent type of day. So the more turbulent the atmosphere, the lower the concentrations get.

And you can see that. For example, at 25 meters on a calm day you are at 2,116 micrograms per cubic meter in the air. If you go to a situation where there is 10 mile an hour winds in a less stable atmosphere, you go down to 214.

Obviously, the concentrations go down with this. You can see.

What we do with these concentrations is we calculate a measure of risk called an MOE or margin of exposure. It is shown in the equation there at the bottom. Basically, what we do is divide these concentrations into some sort of regulatory threshold called the HEC or human equivalent concentration.

Now what I will do, just so you can kind of compare and contrast what we're doing and with what PERFUM potentially could offer us, we'll talk about PERFUM in the next few slides. And again, PERFUM is, and you will hear much more from Dr. Reiss in a minute, but PERFUM is based on the use of the industrial source complex model, just like we're doing.

A key difference here is it uses five years of historical meteorological data from different stations. I think in the case study today there are four different stations that were looked at in Florida and California.

And then it also can allow you to look at variability and the emission terms, which we basically assume a 24 hour average. Again, it is important to us because it is potentially a tool we could use to look at uncertainty and variability.

So just for comparing with what we're doing and then with the case study Dr. Reiss is going to present, he also used 1 to 40 acre fields and, as I just said, five years of meteorological data from two stations in California, Bakersfield and Ventura, and two stations in Florida.

He also used varied emission -- he also used the variety of application methods and emission control type of data that we have available to us. And as part of the system, he also was able to integrate in the actual flux rate changes over time, where we're using an average.

This is just an illustration of flux data from two sites. This is a broadcast flat fume applications. In our case study, we used the data from the pink line, which is Manteca. That's just the site in California. And the other one is Watsonville in California.

Basically, the same kind of application method.

You see how -- it is a little bit different over time. Essentially, what this is is a graph of the percent of flux rate on the Y axis or a measure of how much is coming off the field as a percent of application rate versus the time after application on the X axis.

Another key input for the PERFUM model is the use of actual meteorological data over the five-year period. This is what is called a wind gross (ph) plot for Bakersfield, California, that shows basically how the wind speed and direction changed over time. The size of the bars represents the amplitude of the wind speed and obviously the direction shown.

This is the kind of output you get from PERFUM. Basically, what you do is you calculate the red contour line first. That's just a measure around the perimeter of the treated field to -- it is a measure of the distance at which you get to a certain threshold concentration that you designate.

The black line is a measure of the distance at a selected percentile of exposure. So I think in this case it was 95th percentile of exposure off the red line. And then the shaded area there is -- you can use that to define the exposure exceedances over time. That's what that represents.

Very different from our approach where we're looking at a single receptor line downwind.

And basically right now I would like to quickly wrap up. This slide really represents the theme of the charge questions that we'll be talking about later. We're very interested in understanding if you believe that the documentation of the system is adequate and reflective of what it does.

We're also interested in your evaluation of the overall system design and the required inputs that you need to operate the system.

And then, finally, we're interested in how the results were presented, are they clear, can you follow it through from the beginning to the end and have a clear understanding of what they represent.

DR. ROBERTS: Thank you.

Are there any questions from the panel for Mr. Dawson based on his presentation?

Dr. Small.

DR. SMALL: I have a question. I'm not sure if this is -- it is kind of a background issue. I'm not sure if we're going to get to it in our charge questions. I don't have a lot of experience with fumigants.

One of the basic assumptions in the use of the ISC model is that everything is in a vapor phase. And I'm just wondering for those who have had experience should I worry about particulate phase association. Are there ever issues with wind blown soil or dust particularly during high wind periods on which there could be some particulate association as well.

Perhaps those who have worked with it can talk about the reasons for not considering that sort of mechanism or pathway.

DR. BARRY: We have really only worried about the vapor phase. I think really that's probably for fumigants all we need to worry about. Because they are not really applied in high wind situations. And we haven't worried about the soil, dust blowing from the site.

I don't really know if that's an issue. Maybe one of the panel members would have some expert answer.

DR. ROBERTS: For the record, the last response was from Dr. Barry, just so we can sort of keep it all straight, if folks could identify themselves before they speak. I think you are about to respond, Dr. Reiss. Is that correct?

DR. REISS: Yes. One point to consider is that they are injected into the ground when they are applied. So just by gravity you wouldn't expect a particulate to escape from the field.

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

DR. YATES: Another thing with fumigants is that they tend to not absorb the soil particles nearly as strongly as other pesticides. The absorption is quite low.

So in general, absorption effects really aren't probably that significant.

DR. SMALL: Because they are VOCs (ph) rather than SVOCs (ph). DR. YATES: Yes, they have very high vapor pressures and low absorption.

DR. ROBERTS: Dr. Baker, did you want to add something to that?

DR. BAKER: Is there the potential for any vehicle traffic on the field after application which would enhance the flux of the fumigant?

DR. SEGAWA: Randy Segawa with Department of Pesticide Regulation. Normally, in fact, in all cases at least in California, there are prohibitions to reentering that field once it has been treated for at least a week.

DR. SHOKES: Fred Shokes from Virginia Tech. I noticed the peaks on the diagram you showed us, the graph. Do you know what the factors are that cause these peaks?

DR. BARRY: Terri Barry with DPR. Those are often diurnal changes in the flux. We tend to see in our field studies higher flux values at night. So you are seeing a diurnal pattern, just like you are going to see in Dr. Reiss' presentation.

And it is atmospheric conditions that lead to the higher flux at night.

DR. ROBERTS: Dr. Yates.

DR. YATES: For the flux studies that we have conducted, we tend to see higher fluxes during the day, but lower air concentrations because there is a lot more air movement.

At night, generally, we see lower fluxes, which is usually due to stable conditions occurring, which tend to kind of repress the ability -- basically, it changes the concentration gradients at the soil surface so you tend to have less flux at nights.

But basically, Terri is correct about the diurnal fluctuations do cause the emissions to vary through the day where you have generally, at least from my experience, it is high values of flux during the day and low values at night.

DR. ROBERTS: Dr. Wang.

DR. WANG: To follow up on Scott's comments on the high emissions, it's a diurnal variations that is showing. That's why it's also attributed to the effects of the diffusion coefficients and also possibly the permeability. If there is a tarp cover during the day, especially later afternoon, so that's when you will likely see a higher partitioning to the vapor phase and higher movement diffusion rate and higher permeability. So that's probably why you will see a (inaudible) emission flux.

DR. ROBERTS: We're going to have a presentation now from Dr. Reiss about the model. Let me just ask if there are any questions regarding clarifications from Dr. Dawson, Mr. Dawson's presentation before we move on to Reiss'.

Seeing none, then let's go ahead and get to Dr. Reiss' presentation on the model.

DR. REISS: I prefer to stand. Is that all right? Good morning, everyone.

I just want to start with an overview of the presentation. Give you a general guide as to where I'm going and to when I'll approximately be done. I'll start with an introduction and try to lay out the issues.

Then I'll give a detailed description of the PERFUM model that we have built. And I think the major inputs of the models are the flux rate and a characterization of meteorological conditions in the growing areas following these applications. So the following two sections will deal with those issues.

We'll give some results of the case study analysis using iodomethane. And then we'll describe some of the uncertainty analysis that we have done, and, finally, some conclusions.

This shows a picture for one of the field studies that Arvesta has conducted to characterize the flux rates of iodomethane. This is a raised bed application. You can see the tractor moving across the field. And as it is moving across the field, it is laying shanks that injects the fumigant into the ground, and then a tarp immediately is laid over the field to limit the emissions and also increase the efficacy of the product by trapping it into the soil for a long period of time.

As you can see in this particular circumstance, there are houses nearby, which is one of the motivations for looking at bystander exposures. This is probably closer than in most circumstances, but it happens.

So setting this up, after application, there is a potential for escape from the field surface of these fumigants causing potential downwind exposures.

In California, buffer zones have been established that restrict entry around the field after application. And the purpose of that is to mitigate these inhalation exposures to these bystanders.

So what sort of questions do risk managers need to know about bystander exposure?

First, and the most obvious thing is what the distribution of concentrations of iodomethane or any other fumigant downwind of these applications. And then if we have a distribution of concentrations, what sort of criteria can we use to establish a protective buffer zone.

Then finally, once we establish a protective buffer zone, we want to specifically look at what are the exposures and the risks at the perimeter of that buffer zone in an issue of risk management.

As I said, the PERFUM model was developed with funding from Arvesta Corporation, which is currently seeking a registration for iodomethane. We have used iodomethane in this analysis as a case study.

Iodomethane is a preplant soil biocide. It has activity against insects, plant parasitic nematodes, soil borne pathogens and weed seeds.

In the marketplace, it is going to be labeled MIDAS as a trade name. That's going to be a combination of iodomethane and chloropicrin in various formulations that are listed there, the four different ratios of the products.

May be used for growing strawberries, fresh market tomatoes, peppers and some other types of plants. But the focus here is on field crops like strawberries, tomatoes and peppers.

It is a liquid at ambient temperatures with a moderate vapor pressure. When these shanks or it's injected by drip irrigation, the compound turns into a gas mostly.

These plastic tarps as I showed are laid down over the field surface immediately after the application. These are used to mitigate exposures and increase efficacy.

As Jeff just described, there are three potential application methods that iodomethane can be used for, a shallow shank, broadcast flat fume, which we'll just refer to as flat fume, two raised bed methods, one with injection, shallow shank injection, we'll just refer to that as a raised bed, and then the raised bed drip irrigation, which we'll just refer to drip irrigation.

And I have pictures of all these which we'll show in a moment.

The current toxicity threshold that EPA is considering for iodomethane is 120 micrograms per meter cubed. That's one of the inputs into the model. We're interested in buffer distances up to about 120 micrograms per meter cubed. That's averaged over 24 hours. So we're looking at 24 hour exposures for iodomethane.

This will likely change with some new data that are being developed. But for the purposes of this field case study, we're using 120 micrograms per meter cubed.

Typically, per field you would expect one application per year.

So let's move on to a description of the model. I don't want to get into all of the mechanics of an air dispersion model. Some of you, looking at your bios, are very familiar with air dispersion modeling.

For some others, it may be a newer topic. I just want to give the basis for air dispersion modeling. In a steady state mode, it is what is called a Guassian dispersion.

You see here kind of a classic graph that's been used to describe what is going on. You have the stack source here. It is emitting a plume out into the atmosphere.

In both the cross wind direction, you are assuming a Gaussian description of concentrations. And in the vertical direction, you are assuming a Gaussian distribution. And there are coefficients in the model called dispersion coefficients that characterize the shapes of those distributions depending on certain circumstances.

So what does a dispersion model do? Quite simple. For a source emitting some gas or particle into the atmosphere, it estimates the concentrations at any location around the source.

If I have a source out here, I can predict with my model, I can get some estimate of the concentration in all directions around that source.

The model, the EPA model that we're using as a basis for this spits out hourly concentrations. Those can be averaged over longer periods such as we'll do in this study for 24 hours.

The input requirements particularly for a fumigant application we need the source dimensions. So if it's a one acre field, we need to define coordinates that define a one acre field. In this case for the case study that we're showing we're assuming all square fields, although you can do all different dimensions if you would like.

You need a flux rate or sometimes called an emission rate. For an area source, we prefer the term flux rate because it is not just mass per time, but it's mass per area per time. So it could be pounds per acre per day, for example, would be a unit of the flux rate.

We need a characterization of the meteorology following the application. That includes the wind speed, wind direction and atmospheric stability on an hourly basis.

I just want to talk a little bit about atmospheric stability because it's a key parameter in the model. And some of the different data sources that we have used to characterize the meteorology we needed to use different methods to characterize the stability.

It is a measure of atmospheric turbulence and it's expressed on an ordinal scale from A through F with increasing stability, so F would be a most unstable conditions and A would be the least unstable conditions.

If you were to emit a gas into the air, all things being equal, it would disperse more rapidly during A stability than it would during F.

During the daytime, you can have anywhere from A to D stability, and at nighttime from D to F. So you generally see more stable conditions at the nighttime periods.

So I want to give a little bit of background on fumigant buffer zones before we get to the guts of the PERFUM model. The most work to date has been done by the California Department of Pesticide Regulation, who is here today, primarily for methylbromide. So a lot of the work we're presenting here today builds on that.

DPR has established buffer zones for methylbromide that vary by application method and application rate. So they have basically a table for methylbromide where you would have the application rate on one or actually a flux rate on one axis and a field size on the other axis and you would sort of look up what buffer zone you would need for that field size and flux rate.

So a brief description of the DPR approach. They back calculate a flux rate from a field study or sometimes use a direct calculation. I will describe how that is done in Section 3. Then they run EPA's ISCST 3 dispersion model, which is a commonly used dispersion model that's been in various forms around at EPA for 20 or more years.

And they use at when they are using a 24 hour exposure, they use a wind speed of 1.4 meters per second, which is about 3.1 miles per hour, a C class stability, which is moderately unstable, and a constant wind direction for 24 hours.

Then they estimate the distance required for that concentration to decline to a level of concern.

Here is just a brief picture that shows what that might look like. We have a source here to the left. And this contour line shows -- we're basically inside of the contour line. The concentration is above the threshold concentration that we're interested. And outside the line, the concentration is below that threshold.

So when we want to set a buffer zone, one way to do it is to say what is the maximum distance from that source before the concentration declines to a level below, a level of concern. And so that would be this distance here.

Now, one of the key assumptions in this approach is that the wind direction is constant for 24 hours. And as we have shown, that's generally not the case. But there is others -- like C class stability, you could have more stable conditions than a C class stability.

So the goal in PERFUM is to try to look at all those parameters put together using historical meteorological data and see where it comes out.

So the PERFUM approach, we want to run the ISCST3 model with five years of historical meteorological data. And this is not hard to get. If you are not familiar with air modeling, there is a plethora of data sources out there of historical meteorological data on an hourly basis that you can use to generate these data sets.

One distinction I want to make is that it is a little bit different than people when they are running like an air model for permitting conditions where they have a source that's mostly constantly emitting, like a stack source or an industrial source.

In that case, they are running the model for five years. And each day, that is a concentration of it would have been that day for those meteorological conditions.

In this case, the fumigant's only emitted maybe once per year. So what we're really doing in this case is developing a time series or a set of potential 24 hour average concentrations that could have existed if the fumigant was applied on that particular day.

If we knew that the fumigant was applied on a given day, then we could assign it that. But we don't know what day the fumigant is going to be applied. So we have this five year set of 24 hour concentrations, which are all possibilities for what might be the actual case.

We're going to estimate concentrations in all directions around the field. So this gives us sort of an exposure probability. So if you were talking about somebody who is at the perimeter of the field, by estimating the concentrations in all directions, you would be able to give some sort of exposure probability of them being exposed to a given concentration.

We're also going to use the actual measured diurnal flux rates whereas in the DPR and EPA current approach they are using 24 hour average flux rate. And we're going to show later in the presentation in the case study that that can be a critical factor.

Particularly, you are best off having your applications at the beginning of the day so that more of the emissions, relatively speaking, are during the unstable, higher wind speed conditions during the daytime period as opposed to the very stable conditions at night.

So this is a graph that Jeff also showed, an example of probability of exposure. This is just the PERFUM output for one day for a field. And the contour line shows the distance to the level of concern, 120 micrograms per meter cubed in this case.

One way to set the buffer zone is to say we want to make sure that 95 percent of the perimeter of the buffer zone is below the level of concern. And that's this line here. This line basically sets this length at five percent of the total circumference of the field.

This is just one way to do it. I'm not advocating any particular way in this presentation. But really it is a risk management decision in the end, is to how to actually set the buffer zones. The PERFUM model is going to output concentration data in a variety of formats that leaves a variety of options for how to set those buffer zones.

So what sort of distributions are output from PERFUM that you could use to set a buffer zone. The first one we call the whole field. These are the distances to threshold in all directions for the field.

We divide the field into sort of slivers or spokes. And we calculate the distances that it takes to get to the threshold concentration that we're concerned about in all those directions.

So the number of buffer lengths I might get for a five year data set, I would have the number of spokes I have divided the field into or the number of pie slices, if you will, times the number of days. So I might get 100,000 different numbers for a five year data set.

The other distribution that it outputs is the maximum daily concentration. And this distribution is simply the maximum distance to the threshold in one direction. So you are basically taking -- this value here is the maximum distance to get below the threshold from the field. So that distribution would just include that one value for that day. So you would get -- the number of buffer lengths you would get would be equal to the number of days that you modeled.

So options for setting the buffer zone. You could look at this whole field approach and you could set the buffer zone based on an upper percentile of the whole field concentration distribution. Or you could take the maximum daily concentration approach and you could set the buffer zone based on an upper percentile of maximum concentration distribution. Or you could do some combination of the two.

We have an additional program that's part of PERFUM, which I'll describe in a moment, which you can use to analyze any buffer length that you were interested in.

So how is PERFUM structured? It is a Fortran model built with the Lahey 95 compiler. And most of EPA's mathematical models are built with the same for of platform and are in Fortran.

This provided a good opportunity for us. We found that -- the first version of PERFUM that we developed, we basically ran the ISCST 3 model, took that output and then PERFUM was a post processing program that analyzed that output.

There was a lot of disadvantages to that. One, the output was rather large. And there were a few other sort of technical details that we couldn't accomplish that way.

So what we did is we went to the EPA web site and we downloaded the ISCST 3 source code, which EPA provides, and we were able to compile that on our own compiler. And we basically built the ISCST 3 model into PERFUM.

So we converted the model into a subroutine that's called by PERFUM. So the way PERFUM basically works is that you run the main model, it sets up all the input information that you need, and then it calls ISCST 3 as a subroutine, and it runs through five years of meteorological data.

And after each day or each hour in some cases, it calls PERFUM subroutines that tabulate those results in a way that we need them to output the data. And then when it is finished with the five years of meteorological data, it returns to the main model and outputs the results in the format we need.

One of the other features in the model is that we're able to treat the flux rate as a probabilistic variable. We're going out in these field studies and we have a method to measure these flux rates. But, of course, there is an uncertainty in those measurements, like all measurements.

In the model, we, basically, treat that uncertainty by each time we call a flux rate. So for each period in the model where we need to call a flux rate, we perturb that flux rate based on the standard error of the measurement of the flux rate, which I'll describe how we get in a moment and multiply that by a Z score.

So we're assuming a normal distribution. So we're basically perturbing on a normal distribution basis.

That's slightly different from the version I provided in the original submission. We had a T distribution with 11 degrees of freedom. I found it easier with some changes I'll describe in a moment to convert that to a Z distribution, a normal Z score distribution, the typical normal distribution.

Had very little difference in the model. But as I said, I couldn't help but continue to tinker with it a little bit.

One of the things I think I did to improve the model is we have added a random number generator. Whereas before we just fed the model 2000 random T values that we got that I derived in Excel, I have added now so you have a random number generator using the Lahey Fortran compiler. It generates a random number between zero and one. Then I have a module that calculates the Z score, a normal distribution Z score from that random number.

And I used that to perturb the flux rates. One of the issues I'm interested in getting the panel's views on is, as a regulatory model, I have chosen a specific random number seed in my program. So basically it is using the same set of random numbers every time it calls the model.

The advantage of that I think for a regulatory purpose is that you would get the same result every time you ran the model. Whereas if you were to randomly pick a seed each time, which you can also do in the compiler we have, you might get slightly different results each time you ran the model, because it would pick a different distribution of random numbers.

There could be a temptation there to run the model over and over until you got the lowest result you could find.

I took a look at this, and looked at the stability of the estimates using 10 different random seeds. And we found that it is really a very stable result when you are looking at the 95th or 99th percentile.

If you are looking at the whole field distribution, the coefficient of variance was below 1 percent, .6 percent. So you're getting less than a one percent difference in the model results.

If you go beyond the 99 percentile, you are up at 99.9, 99.99, you can start to get upwards of maybe about a five percent difference in model results for different runs.

But if you assume you are going to regulate somewhere in this region, I think you are getting repeatable results that are usable.

One of the inputs to the model that we need to give it is a receptor grid, meaning we need to tell it at what points around this field do we need to calculate the concentrations.

So this is an example receptor grid for the model. I think it is for a five acre field. I'm not sure. But they all look the same. You see the field here in the middle in green. And there is all these different rings around the field of receptor points.

Each of these points is a receptor where we're going to calculate the concentration in the model. There are actually 28 rings in all the scenarios. And there is a number of spokes. I think there is more than 100 or 200 in this particular example.

So in addition to these rings, we defined a spoke with this blue line as an example of a spoke where it is just a set of numbers, one number in each ring around the field.

And the advantage of assigning these spokes is that we can do an interpolation of the concentration results and we can actually calculate the buffer zone even if it is in between two of these different rings.

This is just a blow-up of the northwest corner of one of the plots. And you see the rings in a little bit closer proximity. This might be 10 meters away and that might be 20 meters away and 30 meters away. So we're calculating the concentrations at every point along these arcs.

We wanted to get the model to run as quickly as possible and get accurate results. When we first ran the model, we had what we called the fine grid, what we now call the fine grid coordinates. These included 96 to 232 spokes around the field or pie slivers around the field where we're calculating concentrations, depending on the field size.

And what we found is that -- we changed that number, we reduced that number by a factor of four to develop a course grid scenario. And the model ran more -- ran, obviously, a quicker, about fourfold quicker. And it got results at the 95th percentile and 99th percentile that were accurate -- as accurate, nearly as accurate as the fine grid.

So as a matter of computational efficiency, the course grid could be used for most circumstances.

So let me just review some of the features of the model. The model outputs the full percentile distributions of buffer lengths for what we define as the whole field and maximum concentration percentiles.

It gives the percentiles from the first to 99th percentile. It also gives the 99.9 and 99.99 percentiles.

The model repeats these calculations for up to 10 user supplied application rates. The advantage of this is that the buffer zones, the buffer zone tables, for example, for methylbromide are established as a function of application rate.

And it takes quite a while for the model to run through a whole and other flux rate. If you were to just run the model over, it would take as long as it took to get the answers for the first flux rate.

But, fortunately, the model is linear between application rate and emission rate -- emission rate and concentration. So we can take advantage of that linearity and just do a simple ratio to adjust the concentrations from one run with a particular flux rate to another run with another flux rate.

So the model will do that and it will output the results for up to 10 different user supplied application rates.

Also, a lot of these fumigants are applied in particular seasons. Maybe it is applied -- the growing season is from April to June or something. So what we have done is we have outputted the buffer lengths on a monthly basis so you can see the -- and I will show a distribution later of how they tend to vary by season.

We didn't employ this for the case study analysis, because when you look at national buffer zones -- establishing a national buffer zone, it is a little hard to generalize about what particular months the application might occur. But it is something that the model outputs then could be used for seasonal analysis.

We have several field sizes, 1, 5, 10 and 40 acres. This could be expanded to other field sizes and dimensions either by us by providing a receptor grid or the user can even input their own receptor grid with a different field size or even a different field geometry.

Just like if you were to run ISC, it outputs the ISCST 3 output file. You need to check that for errors.

You can also get a summary of a lot of the output information from that file.

And we have included about 60 error and warning messages to help the user debug potential problems or inconsistencies in the input data or diagnose potential model problems.

So one of the key interesting things about the model is how long it takes to run. We ran just -- I'll give an example, on my system, which is a 2.4 gigahertz processor with 512 megabytes of RAM. We used the course grid option. And the model runs take anywhere from 5 to 23 minutes depending on the field size. So it is relatively fast.

And we provided with the model a series of DOS batch files which you can use to do multiple runs in the same session or just let it go in the evening like we have done. And I think to run the 120 scenarios that we ran for the case study, you can do that in generally two evenings. So it is not too onerous.

Recent refinements to the program. As I described, we did some modifications to the random number generator. There is some additional error checking. We improved the interpolation algorithm in a very minor way.

And in particular, I want to note that we have developed some scenarios to investigate the impact of multiple fields emitting at the same time. I'll describe those in the uncertainty analysis.

Finally, we have added a second program into the package called PERFUM MOE. We call it a risk management tool.

If you are not familiar with how EPA often does risk assessments, they define a margin of exposure as the human equivalent NOEL. So they might get a NOEL from an animal study and they may convert that to a human equivalent concentration, and they divide that by the exposure.

You can see by the form of this equation that -- well, they can translate into safety factors or something like that.

So generally, EPA, for many their of applications, as a policy decision seeks to have an MOE of 100. And what that would translate into is an exposure that's 100 fold less than the concentration known to -- the lowest concentration known not to cause an effect.

Other MOEs besides 100 are used in various circumstances in the matter of policy and uncertainty in the database. But for the purposes of this example, we have looked at a 100 fold MOE.

So this program, what it does, is that for a given buffer zone distance, and no matter how you get it, whether you get it from PERFUM or it is just a buffer zone that you are interested in looking at, say I just want to know what the exposures would be for a 300 foot or a 500 foot buffer zone, this program will calculate the distribution of margins of exposure.

So like from the first to 99 or 99.9 percentile at the perimeter of the buffer zone.

So for a person that's at the perimeter of the buffer zone or a location at the perimeter of the buffer zone, more accurately, this program will calculate that distribution of margins of exposure. It is a way for risk managers to look at what the potential risks are for a given buffer zone.

Also, it is a way if you were to define a buffer zone that doesn't assure that in 100 percent of the circumstances you had a 100 fold margin of safety or margin of exposure, you would be able to estimate how high -- or how low in this case, how much lower below 100 it could get.

So really, the severity of any exceedance above 100 or below 100.

There are some key conservative assumptions to bear in mind when you are looking at the results of this. Our calculations assume that a bystander spends 24 hours following the application at the perimeter of the buffer zone.

If it was the residents, that could very well be the case. But for many bystanders, they may spend less than 24 hours at the perimeter of the buffer zone.

But as a conservative assumption, for one, and, secondly, because it is difficult to define the probability of someone not spending 24 hours there, we have assumed a 24 hour exposure at the perimeter.

The calculation also assumes that the bystander is either outdoors for 24 hours or that the indoor exposure is the same as the outdoor exposure. We don't yet have chemical specific data for iodomethane, but we know it is like methylbromide, a relatively sticky compound.

So it is quite possible that the indoor exposure is less than the outdoor exposure. But for the purposes of this case study, we have assumed that the indoor and outdoor exposure is the same.

Part 3, I want to talk about how we have estimated the flux rates for our case study for iodomethane and generally how -- some methods to estimate those flux rates from field studies.

For iodomethane, Arvesta, the sponsor, has currently conducted seven field studies. Those include two with flat fume, two with the drip irrigation application method and three with the raised bed application methods. And there may be more field studies conducted subsequently.

We have calculated the flux rates using a methodology developed by California Department of Pesticide Regulation that uses the field study results and the ISC model to essentially back calculate the flux rate. It is sometimes called the indirect method.

So just to give you an example as to how a field study is designed and analyzed, let's go through this one example. You have a square field here. And we have established 12 monitors to measure the iodomethane concentration following the application. We have put eight monitors at about 30 feet from the field in the four directions around the field. And then we have additional monitors at the corners about 140 feet from the corners. We have a total of 12 monitors to characterize the iodomethane concentration following the application.

So that example showed 12. There were others where we had only had eight. These are generally charcoal air samplers, and they are also put at about 1 to 1.5 meters above the ground.

The measurements are collected in periods generally encompassing the daytime and nighttime period. So we get separate estimates of the flux during the daytime and then during the nighttime. And I'll show an example of how we divided those periods in a moment. And that's used to capture the diurnal variability in the flux that we have already discussed.

The samples are collected for a minimum of about 10 days. But the peak was always in the first 24 hours with iodomethane. That may not be true for all fumigants. But for iodomethane, in the seven field studies we have had, the peak emissions always occurred in the first 24 hours. So that's the focus of our analysis. And they generally declined to negligible emissions in five to seven or maybe a few more days.

This is an example for the Manteca study how we divided the flux periods.

This basically shows the 24 hours following the application. We established monitors to capture an average concentration over the first three hours, the next three hours, the seven and eight, the next two hours after that. So we had three separate measurements of the flux for that first eight hour period.

Then there was a long nighttime sample. I think that's about 13 hours. Then what is really the next day, we had an additional sample that starts in the next daytime period. So that's generally how most of the studies were designed.

So how do we get a flux rate out of the ISC model? We know the ISC model will predict concentrations downwind of a field following an application. But we don't know what flux rate to put in.

But we know that there is some flux rate that must statistically best predict the concentration profile that we observe. So we use a method developed by DPR to best -- to determine the flux rate that best explains the data that we observe in the study.

So what we do is we run the ISCST model with a nominal flux rate. And then we use a statistical method to calculate the best fit flux rate. That method takes advantage of the fact that the concentration and flux rate are linearly related.

So we don't need to keep running the model over and over with different flux rates to know what the concentrations would be for different flux rates.

Once we run the model once for one flux rate, just by a ratio we can determine what the concentrations would be for another flux rate.

What is done is we do a linear regression of the model, the measured concentrations. We have maybe our eight to 12 measured concentrations here on the Y axis and our modeled concentrations at each of those exact receptor points on the X axis, and then we calculate a slope and an intercept.

And then what we do is we can't adjust the flux rate with both the slope and the intercept. So we're looking at just a slope using -- multiplying that slope from the regression and multiplying it by that nominal flux rate that we first ran the model, and that gives us the flux rate that best explains our observed data.

Sometimes there is a problem when you first do this. Sometimes the fit is poor or sometimes the intercept term is statistically significant. And I'll show an example in a moment. So you need to consider the following options. And DPR suggests the following.

First, you sort the data independently and rerun the regression. The theoretical basis for this is that the model predicts -- is known -- when they validate the

ISC model and other dispersion models, they find that it predicts the maximum concentration quite well, but it doesn't necessarily predict the location of the maximum concentration. So sorting the data sort of removes that spatial element.

You could also sort the data and constrain the intercept to zero if you are still having a problem getting a large intercept.

So here is an example where we have our measured concentrations on the Y axis and our modeled concentrations at those same receptor points at the X axis. You see there is a relatively good agreement between the two.

And so let's try to fit a linear regression to it. So we get the following result. We get an R squared of .97, which is excellent, a slope 4.8 and an intercept of .0028.

The problem was the intercept was statistically significant. So our concern here is that some of the explanation -- some of what we're trying to explain in the measured model data is incorporated in that intercept, which is unusable to us in calculating a flux rate.

So let's sort the data independently. We see these red dots, and then rerun the regression. Again, we get an excellent fit. Even a little bit better than before. Almost .99. However, this intercept term was still statistically significant.

Finally, we go to our last option. We just calculate a regression with the intercept constraint through zero. So we get a slope of .5126. We just multiply that by the flux rate that we ran the ISC model. And that gives us the sort of best fit flux rate or the most -- the flux rate that best explains our observed data.

This just shows another example where the intercept was low when we first ran the regression. Again, you have a very high R squared, about .95.

If you look at the data, you generally get some very good correlations for the first 24 hours. But there are occasionally samples where you don't. And then the correlations generally could decline if you go farther away from the first day.

And the reason there is, I think, first, the measurement variability when you get to lower concentrations is going to be more substantial.

So we need an estimate of the uncertainty on that slope estimate to use in our model so we can perturb the emission rates.

And the way we do that is basically estimate the standard error on that slope. And I convert it just for convenience to a coefficient of variance that I can use in the model.

There are some other options. There has been a lot of talk about how to go about estimating these flux rates. I just want to go through a few other options that you can possibly consider.

You could do a linear regression with the intercept constrained through zero just from the start. Not as one of the options. But just use that from the start.

The advantage of that is that it would minimize the mean square error between the predicted estimates with the adjusted flux rate and the measured concentrations.

It may not be the best fit slope, because, if you remove that intercept, you may miss some of the data.

You could also use log transform data in a linear regression. The advantage here is that that would normalize the data. One of the assumptions of a linear regression is that the data are normally distributed.

But because these data vary over many more orders of magnitude, they generally aren't normally distributed. Although, we found that the residuals -- when you look at the residuals of the regressions, there doesn't appear to be a bias. So that that may mitigate that concern.

One of the problems here is that it minimizes the mean square error of the logged values instead of the raw values. The effect that has is that it places more emphasis on the lower concentration values that may have higher uncertainty.

Another idea would only include values above the limit of detection or maybe even above some higher concentration. I generally found that that normalizes the data and you get somewhat larger standard errors, which maybe are more reflective of the standard error at the maximum concentrations that you observed. So these are some other options to consider.

I should mention that whatever option is used, the model and all the models I think you are going to hear, could accommodate that. It is just a matter of how you derive that input data.

I want to go through some of the field study data that has been conducted. This shows a flat fume application. You see, basically, a flat field, and the tractor is laying down shanks to inject the material. And this tarp is immediately being rolled over the field to trap the emissions.

Arvesta has done two flat fume studies to date, one in Manteca, California, and the other in Watsonville, California.

Let's look at some of the results that we got. This is also a graph that Jeff showed a few moments ago. I'm showing the flux rate as a percent of application, which is a convenient way to present it.

So this tells you that, say, for this data point here, maybe that's at about 18 percent, that means that in the first period for the Manteca study 18 percent of the applied mass was emitted from the field. So these are -- as a function of the application rates, some healthy amounts come off the field.

And this just shows the profile as you go through the five to seven days after the application.

These spikes you see here are generally diurnally related. And as we already talked about, there could be a variety of reasons for that, soil temperature, the soil permeability of the tarp, the amount of the material in the vapor phase.

But it is something that for methylbromide was the case and is something that has been repeatable in the iodomethane studies in almost all the data we see. We see this diurnal profile show up quite often -- I should mention, lower flux rates during the nighttime period and higher flux rates during the day.

This shows a raised bed application, the same chart I showed at the beginning. The beds are made prior to the material being injected. The tractor goes off, the shanks are injected, and the tarp is laid immediately afterwards to trap the emissions.

So far, Arvesta has conducted raised bed studies in Oxnard, California, in Plant City, Florida, which is a popular growing area in Florida. And the most recent study was done in Guadalupe, California. That was just done in May. So it wasn't included in the submission package that you all received, but we just recently analyzed that data.

This shows you the raised bed flux rate as a percent of application and the mean time since application. You see really a remarkably similar profile between the three application methods.

There is obviously some experimental variability for whatever reason. You have this peak a few days out for the Plant City study, but you really see a very similar profile among these three studies all with the same application method.

This shows the drip irrigation. Basically, a sprinkler system. The fumigant is injected with irrigation water into the sub surface strip lines and the plastic tarps are already covering the treated beds.

So far, drip irrigation studies have been conducted in La Selva Beach, California, that's near Watsonville, and in Camarillo, California. That's relatively close to Santa Barbara.

And this shows, again, a comparison of the decline profile for the drip irrigation studies. Again, just like the raised bed, they look very similar, decline profile, for the two drip irrigation studies.

Let's summarize the data that we have from our seven field studies so far. Again, I'm putting in this terms of percent of application emitted in the first 24 hours.

You see for the flat fume we have at Manteca and Watsonville 47 percent and 35 percent. That was actually the largest difference we saw between measurance with the same application method.

For raised bed, we had three different methods, and they were 55 to 61 percent. They were very tight.

And for drip irrigation, we had 42 to 50 percent between La Selva Beach and Camarillo.

One of the observations you can draw from these data is that during the first 24 hours as much as half or more of the application rate is emitted during that period. That's a significant amount.

And it explains why these bystander exposures are a concern. It also reduces the sort of variability or uncertainty that we might be worried about with bystander exposures. Because we're already assuming a pretty substantial amount of the mass is emitted during that first 24 hour period.

One of the things we wanted to look at is what are some of the factors that are causing the variability we observe.

One of the obvious things is temperature. We have done studies in the winter or at least the fall, the summer, the spring. So we have a variety of temperatures that we have observed during our studies.

This shows a graph of the flux rate as a percent of the application emitted versus temperature, the average daily temperature or the average 24 hour temperature for that first daytime period or that first 24 hour period.

There is no apparent correlation between those. That's not to say that temperature has no effect on the emissions that we observed. We do see this diurnal profile, but it may suggest that we're looking at maybe a diffusion limited process rather than a temperature limited process.

I just want to underscore the importance of the first 24 hours. A majority of the iodomethane emissions occur during this first 24 hour period after application.

Therefore, the first 24 hours of emissions produce the peak exposures. And the second day of emissions were generally about half of what we observed during the first day.

Let's take a look at a chart of the second day emissions. This just shows a plot, the blue bars showing the flux rate for the first 24 hours, and the red bar for the second 24 hours.

In all cases, the flux rate during the first 24 hours was half or more than twice the flux rate during the second 24 hours. That shows us that from a risk standpoint we're most concerned about the first 24 hours.

There is also -- in addition to this direct flux calculation where we look at measured concentrations downwind to the field and try to infer a flux rate from the model, the ISC model, there is also a direct flux method.

In Manteca, the one study, we actually did both the direct and indirect flux methods. Let's look at what we got for that. For the direct flux rate method, sometimes called the aerodynamic method, there is monitors placed at varying heights at the center of the field, typically.

And there is a fluid dynamic calculation that is used to calculate the flux rate based on those observed concentrations along these mass that are vertically situated.

So it provides for us an independent verification of the flux rate. And you will see that for Manteca, the one where we have a comparison of the two methods, they were very comparable.

The blue line shows the estimates we got for the direct flux method. And the dashed red line shows the estimates for the indirect method. They track pretty well, I think, as you can see.

There is no apparent bias. In some cases, the direct flux method was higher. In other cases, the indirect method was higher. If you look at the overall flux rate for the first 24 hours, I think there was about a five percent difference between these two methods.

That could be fortuitously close, maybe, but it gave some good reassurance that the direct flux method is giving comparable results.

DR. ROBERTS: I think before Dr. Reiss goes on to Part 4, it would be a good idea to take about a 15 minute break. And then we'll resume his presentation, and after that, give the panel the opportunity to ask him questions.

Let's reconvene in 15 minutes.

(Thereupon, a brief recess was taken.)

DR. ROBERTS: Let's go ahead and get started with Dr. Reiss' presentation on Part 4.

DR. REISS: We talked just before the break about characterizing the flux rates of iodomethane following the applications. The other major input we need to the PERFUM model is a characterization of the meteorological conditions in the growing regions following the applications.

So how can we derive suitable meteorological data? We have a problem. Most of the historical meteorological data that have been used for dispersion modeling are built for stack sources, large point sources.

Most of these data have come from urban airports. If you look on EPA's dispersion modeling web site, there is a variety of data sources from the National Weather Service. And they are primarily from large urban airports.

So there is a concern that these may not be representative of the growing regions. So the solution is we looked at other sources of meteorological data that have historically not been used for dispersion modeling. And we did some comparisons afterwards to determine whether using these data actually alter the results in any way.

So what are the potential sources of

meteorological data that we can consider? First, as I already mentioned, in National Weather Service. This is an historical data set available on EPA's modeling web site and most commonly used in dispersion modeling.

It is an observer collected system, meaning it's collected by meteorologists. And as I said, it's commonly used in meteorological applications.

That observer collected system has now been replaced by the ASOS system, the Automated Surface Observing System. It is maintained by the Federal Aviation Administration. And it is basically, as I said, an automated system where there is instrumentation that automatically measures these parameters and stores them in a data set requiring relatively infrequent maintenance.

In California, we also have the California Irrigation Management information System or CIMIS. That's an automated system run by the state of California, used for irrigation management planning. Obviously, since it is for agriculture, there are stations that are close to some of these growing regions.

And then somewhat similar in Florida we have the

Florida Automated Weather Network or FAWN. That's an automated system run by the state of Florida, used for agricultural management. Obviously, there are also stations in the growing regions.

Let's look at some advantages and disadvantages from these four data sets.

The NWS, it is widely used. It's high quality control. The data on the web site from EPA has already been quality controlled beyond what was originally collected. However, there are a few stations and there are not many in the growing regions that we're most concerned about for these fumigant applications.

ASOS, there is a much larger number of stations in the ASOS system. There are many stations -- they are all collected at airports, generally, but there are many other stations, many collected at small airports.

For example, Watsonville in California is a common growing region. And there is a small airport there. There is an ASOS station there. It's not all that uncommon that a lot of these small airports that are generally in rural areas may have an ASOS station.

There is quality control, but it's done on an automated basis. It's not done by a meteorologist. And there is some issues with collecting cloud cover, you can imagine with an automated system.

Cloud cover, I should point out, is one of the variables that's used to calculate the stability class with some methods. You can imagine it is somewhat difficult to collect a measure of the cloud cover from an automated system that basically is sending some sort of signal up to look at the opacity.

There has been some analysis done by EPA or by a contractor that have found that it doesn't always correlate as well with the observer collected data.

CIMIS, there is a large number of stations in the growing areas of California. It has quality control, but, again, it's automated. And it's also collected at a two meter height, which is not standard for dispersion modeling.

Most data for meteorological circumstances are collected at 10 meters. Some of the NWS data is collected at six meters. So there was a concern that the data collected at two meters may not be representative.

The FAWN data, there are stations, obviously. It is an agricultural network. So there are stations in the key growing regions. However, there is very little quality control in the FAWN network.

When I analyzed the data, I found many inconsistencies in the data, circumstances where you had a two meter per second wind speed on one hour and 100 meters per second the next hour and then back down to two meters per second.

So I ended up calling the people who run this FAWN network. And they were pretty honest and said the quality control is, I think, "rudimentary." So there is not a lot of quality control in that system. But when you look at the data set in Florida, there aren't many other stations in the growing regions.

What we did is we decided we would take a look at all of the networks, stations from all of those four networks. And we would do some analysis of that data, and we would use the model with data from all those sources.

Also, we focussed in California and Florida.

Those are the two primary areas where this product could be used. It could be used in many other areas, but for the purposes of this case study we decided to focus on these two areas.

A model can be run with any meteorological data set you create. But for the purposes of the case study, we decided to focus on California and Florida.

This is a map of California. You may not be able to tell, but the Bay is up here, San Francisco and Los Angeles is down below the map. You see we have some coastal stations, a CIMIS station in Monterey, an NWS in Santa Barbara, a CIMIS station in Ventura.

Then there are also stations in the inland regions, which are also key growing areas. We're lucky to have both, a CIMIS and an ASOS station in Merced and then a Fresno -- and a CIMIS station that are very close to one another in Fresno, CIMIS and NWS station. And a Bakersfield station, ASOS station.

This is a map of Florida, obviously. We have stations here from the FAWN network. We have stations in Tavares, Dover and Bradenton. We asked some agriculture extension people what the best FAWN stations would be to represent these sort of applications. And they thought those three were good.

Winter Haven is an ASOS station, also in a growing region. Fort Myers, I'm not sure that there is much growing down there, but it is another -- it is one of the only NWS stations that is really close to something we're interested in. And also Tallahassee, there is some growing up there.

I'll talk briefly about how we process this data. The data come in all sorts of formats. And you need to put it into an ISC compatible format. You also need to estimate what is called the stability class.

The ISC input file includes the temperature, ambient temperature, the wind speed and the wind direction. All of these stations provide that. You just need to reformat the data. But you do need to calculate the stability class using the data that you have.

For the National Weather Service station, we use what is called Turner's method. Basically, we used an EPA program called PCRAMMET. Turner's method is the most commonly used method.

It's basically the stability class is a function of the wind speed whether it is daytime or nighttime, the solar angle and the cloud cover.

We also used Turner's method, we had all the available data to do that for the ASOS network. The PCRAMMET program wasn't designed to look at the ASOS data or to use it in that format. So we just took the PCRAMMET code and converted it into our old program to reprocess the ASOS data.

For the FAWN, there is no cloud cover data. So we used a method called solar radiation delta T. It is basically a method where you are looking at the difference in the temperature between two and ten meters, which is collected at the FAWN stations to get a measure of stability. That is also an EPA recommended method in their meteorological guidance.

For CIMIS, California DPR has already processed that data. And we requested it, and they gave it to us. That uses a method called sigma theta, which is the standard deviation of the wind direction.

That is also an EPA recommended method to calculate stability classes. And it gives a good measure of turbulence.

So how did we select stations to use for our case study?

It was prohibitive to run the model we thought and analyze the results for all the possible stations that are out there. So what we did is we picked 15 stations among these four data networks and used flux data from Oxnard and Manteca, two of our studies that were analyzed in an earlier date, and used the five acre field.

We just ran the PERFUM model for those 15 stations in two different flux studies. And we used five years of data for each station, except Santa Barbara where there was only three years of data available.

And we chose a representative set of four stations from those 15 as I'll show in a moment.

Some observations about the results with the different stations. Generally, the NWS stations gave the lowest buffer zone estimates followed by ASOS, CIMIS and FAWN.

Although, when you look at NWS, ASOS and CIMIS, the difference wasn't that large. The FAWN stations are an outlier on the high end. And I haven't completely figured out why. One of the reasons is it has relatively low wind speeds at least at some of the stations.

There is not a predictable difference by location if you look at inland versus coastal or agricultural versus urban area. Although with 15 data points, it's a relatively small data set. So we can't draw any broad conclusions.

These are the results where we calculated the buffer zones with each of the 15 stations. And I just took the average of Manteca and Oxnard and plotted it from highest to lowest.

When it says percentile, the highest buffer zone result was for Tavares. With 15 stations, that's about like the 91st or 2nd percentile. You see here the three highest buffer zone results were with the FAWN network. Dover, Bradenton and Tavares.

Followed by that, you see a mix of ASOS, CIMIS and the National Weather Service. When we had comparable stations like a station in Merced with CIMIS and ASOS, we got very similar buffer zone results. When we had CIMIS and NWS close together in Fresno, again, we had very similar results.

So we were relatively confident that we were getting repeatable -- relatively similar results between these different meteorological networks except for FAWN.

But to be conservative, we decided to consider -- we would include FAWN at least for this case study analysis. What we did is we divided this profile up into core tiles and picked one station shown in blue for each of the core tiles. So for the Bradenton FAWN station, the Ventura CIMIS station, the Tallahassee NWS station and the Bakersfield ASOS station, we chose those four stations for the complete analysis that we'll present.

And the buffer zone estimates you got -- the average of those four stations was very similar to the average for all 15 stations. So we feel confident those are representative of the 15 stations that we have. And we have two stations in Florida, Bradenton, Tallahassee and two in California, Ventura and Bakersfield and a mix of inland and coastal sites.

After all that prep work, let's talk about some of the results of the case study analysis that we did. How did we go about this? We did model runs. At the time that we did the submission, we had six field studies available, two raised bed, Oxnard and Plant City, two flat fume, Manteca and Watsonville, and two drip irrigation studies, Camarillo and La Selva Beach.

We chose four meteorological stations, I just showed a couple slides ago, Ventura, Tallahassee, Bakersfield and Bradenton. And we have five field sizes that we're interested in looking at that range from the possible field sizes that could be out there in agriculture, 1, 5, 10, 20 and 40.

So that equals total number of runs of six times four times five for 120 different model runs. And like I said, we were able to do this in a couple evenings of computer work.

We weren't there during the evening. It actually ran while we were sleeping.

This shows a distribution of buffer lengths for a five acre field using the Manteca flux rate and the Tallahassee meteorological data.

And I have given the distributions for what we have defined earlier as the whole field and the maximum concentration shown in red.

You see for the whole field distribution up like that. There is this discontinuity here because it is very difficult to estimate the buffer zones when you are in very small distances from the field. So what the model does, when it is very close, it makes a decision as to whether it is zero or whether it is 20 meters. And it doesn't calculate anything in between.

But since we're looking at upper percentiles, that's not going to affect things. So if you look at the '95th percentile here, you are up around a little more than 500 or 600 feet buffer zone.

So if I were to choose the 95th percentile of the whole field distribution, that's what I would get for a buffer zone.

If I were to choose the maximum concentration distribution and look up at the 95th percentile, I would get a buffer zone of about 1300 feet or more than twice as much.

So what sort of factors influence these buffer length estimates? Obviously, we're going to show the flux rate or the type of application which influences the flux rate is going to affect the result you get.

The meteorological data, we want to look at whether we're getting different results with different meteorological stations.

Then one of the things we found in this analysis is that the diurnal profile was very critical in determining what buffer zone estimate you would get, in particular, when the application started. Of course, the field size is important.

Now, as I said earlier, the model outputs this maximum concentration distribution and the whole field distribution, the whole distribution. But for the purposes of discussions, I need to choose something to present some results.

So what I did just for discussion purposes is I defined the buffer zone as the 95th percentile of the whole field distribution for the slides that will follow.

So this first slide shows some of the ranges and buffer lengths for different meteorological stations. So I have my six field studies here on the X axis, and the buffer length I got with a five acre field using those different meteorological stations.

That shows the mean, and these bars show the range. So if you look at the coefficient of variation, it was about 13 percent among different meteorological stations.

So if I am looking at the same flux profile and I'm interested in what the variability is across different meteorological stations, it came out to about 13 percent.

So there are some differences, but it is not that large. I probably should have put this from zero to 900. It may over-exaggerate the length of those bars.

This is somewhat obvious. But if you have a larger field study or a larger field size, you are going to have a larger buffer zone. So you see with the one acre field, the buffer zones are down 250, 200 feet. For a five acre field, they are rising. Then all the way to a

40 acre field for this case study you are looking at buffer zones around 2000 feet or more.

As I said a moment ago, the diurnal profile turned out to be a key factor in determining the buffer zones when we used the PERFUM model, and we're actually accounting for that diurnal profile.

Let's look at an example with the raised bed. We had two raised bed application. One in Oxnard and one in Plant City, Florida.

The Plant City study is shown in the red. That study, if you look at the X axis on the top, started at -- well, it actually started at about 7:30 a.m. and finished at about 9 a.m. Because there were some other activities going on, the measurements began after the application was complete.

You see some of the emissions. Started kind of low, then it went up around 1 to 4 p.m. And then after about 4 p.m., they dropped and pretty precipitously. So you saw a strong diurnal profile there with the emissions after about 4 p.m. dropping to a significantly lower level than they were earlier for this early start application.

If you look at Oxnard, you have the blue dot showing the start of the application at 12. And it finished at about 8:30 p.m. This is unusual for typical field practice. But because of all the other associated activities that are required to do these measurements, it just ended up for this day the application started late, later than expected, and it finished later than expected, much later than expected.

So what was the impact of that? It is kind of interesting. You had some emissions going up here shown on this green line. But then at 8 p.m. when the application ended, you still have a lot of the mass in the field, and you still had some high emissions from this 8 p.m. all the way through 5 a.m.

So for the Oxnard situation, you had a lot of the emissions occurring during that more stable nighttime period where you have higher stability and lower wind speed and it is less conducive to dispersion.

So what impact did that have on the buffer estimates that we would get?

This shows the buffer lengths for the whole field distribution for Oxnard and Plant City.

Let me go back and mention. The mass that came off of the Oxnard study and the mass that came off the Plant City study over the first 24 hours was virtually identical. About the same amount of mass on a percentage basis. The only real difference is this diurnal profile.

Now, if we were to look at the 95th percentile of the whole field distribution, with Oxnard, we would get a buffer zone of 860 feet. With Plant City, we would get a buffer zone of 485 feet, almost a twofold difference in buffer zones just as a result of that diurnal profile for the same mass emissions.

This shows the drip irrigation applications. It is not quite as dramatic, but you still saw the diurnal profile explain some of the differences in the buffer length estimates that we got.

This is the Camarillo study. It started at 8 a.m., finished around noon. And the red bar shows the flux peaked at around noon to 3 p.m., and then dropped pretty precipitously after 3 p.m.

At La Selva Beach, it was a little bit

different. The application started at 12 p.m., finished at 6 p.m. And you had a peak emission up here that's between 6 p.m. and 8 p.m. So you had some very high emissions still in that early evening hour, even up to about 9 p.m.

So for La Selva Beach, you had higher emissions during the early evening period than Camarillo.

And you saw some difference in the buffer length estimates. Even though Camarillo had higher emissions, 50 percent of the mass came off in the first 24 hours for Camarillo whereas La Selva Beach it was only 42 percent.

So even though it had higher mass emissions because of the diurnal profile, the buffer zone at Camarillo was 480 feet and the buffer zone at La Selva Beach was 650 feet. So despite that difference in emissions, that diurnal profile made up for that and a little more.

As I said, we also output the monthly variation of the buffer zone in the PERFUM model for any user who wants to do a seasonal analysis.

So looking at the buffer length for four different meteorological stations in the Manteca data set for a five acre field, we see -- it is not always consistent, but a general profile where you see this dip during the kind of summer period, maybe April through August.

And you have the highest buffer zones in the January, February, November, December area.

From a meteorological standpoint, that is what you would expect. You have generally more stable conditions during the winter, lower wind speeds. You have a shorter daytime period during the wintertime. So you have a longer nighttime period where the conditions are more stable.

So that wasn't surprising. And it shows that these buffer zones can be -- let's look at Tallahassee where you had more than an 800 foot buffer zone during January and then about a 500 buffer zone for -- that would be July. You get a pretty significant difference in some cases if you just look at the seasonal variation.

This just shows another plot showing more or less another profile, but for the Camarillo study.

As I described earlier, we have two programs, really, PERFUM and PERFUM MOE. The PERFUM MOE program is what we call a risk management tool. It is used -- once a user establishes a buffer zone that they are interested in or that they may have set from PERFUM or they just may have a general interest in knowing, you can use the PERFUM MOE program to calculate the distribution of margins of exposure for the locations around that perimeter.

This shows a profile for Oxnard for a five acre field as an example.

For the blue line, I have chosen the buffer zone that I got from the 95th percentile from a PERFUM run using the whole field distribution. So that turned out to be 860 feet.

I ran that through the PERFUM model. And as expected, if I look at the 95th percentile, it is about 100. Because of the geometry, the calculation, it could be a little different than that. But the goal here is to have -- at the 95th percentile, to have a margin of exposure of 100.

Then you can see at the lower percentiles, 90, 85, 80, what the margins of exposure there are, and then you could also see above the 95th percentile what sort of margins of exposure you are potentially having.

So at the 97th percentile, you have a 74 fold margin of exposure. And all the way up to the 99.9 you have a 24 fold margin of exposure. That is showing that even at the 99.9 percentile your exposure is still 24 fold below the human effect, the human equivalent no effect level.

The red bar shows what it would be if I chose a buffer zone from the 90th percentile, and you see proportionately lower numbers for that.

I have one other example of a margin of exposure curve here. This shows for Manteca a 10 acre field. The blue line shows what it would be for a buffer of 545 feet. The red line shows for 436 feet buffer. That's from the 90th percentile.

Again, it is about 99 -- margin of exposure at the 95th percentile as we have determined from the calculation. And then upwards to the 99th percentile it was about a 36 margin of -- margin of exposure, about 36.

I would submit that this is a flatter margin of exposure curve than you see in most environmental circumstances. Say if you are looking at a worker exposure to a mixer loader or something like that where I think that upper tail in my experience has been much higher, where the difference between the 95th percentile and the 99th percentile could be very large. You see that with looking at like drinking water concentrations or something like that.

So I think the relative flatness or how ever you want to interpret this margin of exposure curve, I think it is very useful information that could be used by risk managers to determine a safe buffer zone.

Part 6, the uncertainty analysis, we wanted to look at some various sources of uncertainty in the model. Obviously, every model has uncertainty. Every measurement has uncertainty. And we're using various measures of flux rate and meteorology and whatnot in our model as inputs.

This just lists some of the key areas of uncertainty. The estimation of flux rates. And I'll talk about that a little more in a moment in detail.

The characterization of meteorology in the growing areas. I think we have uncounted for that uncertainty relatively well by looking at different meteorological sources and also using five years of historical meteorological data, which is the EPA standard when you do permitting applications. So we're looking at a relatively long period of time.

The air dispersion estimates. Obviously, there is uncertainties inherent in the dispersion models. I talked about those in the report. And I think for this particular application and area source where we are looking at concentrations very close to the source, relatively speaking, compared to a point source where you might be looking miles away, I think those estimate are comparatively speaking better.

Indoor versus outdoor exposure. We don't know what the indoor exposure is relative to the outdoor exposure, but we have some idea that it could be lower. We just want to look at what the impact of that could be.

Time activity assumptions. We're assuming for our calculations that somebody -- that a person is at the perimeter of the buffer zone for 24 hours. That may not be true at least for most people. So we want to look at the impact of that assumption.

The potential for exposure from multiple fields. I have some additional analysis which I have done on that, which I will show.

And we have looked at the variation of exposure and application likelihood by season in some of the graphs I have shown you earlier with the variation by month.

Let's talk about the uncertainty in flux rates in a little more detail. There are two components to this uncertainty.

There is a measurement uncertainty for individual studies. So when we do a particular flux study, we derive an estimate of those flux rates. But there is an error associated with that estimate. And we have modeled that using the standard error explicitly in the PERFUM model. So I think we're explicitly accounting for that error in the model.

But there is also variability across studies based on field conditions, such as temperature, soil type, organic matter content, et cetera. There is a lot of different things that could happen out in the environment.

We have seven field studies. We have a pretty tight range. We feel pretty good about that. But there could be additional variability that is out there that would show up if we were to do more studies.

So what we did is we wanted to look at that variability by using the larger database for methylbromide. For methylbromide, there has been maybe 30 to 50 studies that have been conducted. A lot of those have been conducted and summarized by DPR.

So the first uncertainty scenario, we went ahead and looked at the methylbromide data summarized by DPR. We came up with a coefficient of variance of 47 percent among the different studies.

Instead of using the coefficient of variance from our flux studies, we used the coefficient of variance of 47 percent in PERFUM to model that variability.

And you would expect when you increase the variability around a mean estimate, you would expect higher buffer lengths at the upper percentiles. And we'll show that in a moment.

Now, even though we're assuming a higher uncertainty in scenario one, we're still assuming the same flux rate that we had in our studies.

The second scenario we decided we would choose a higher flux rate than we actually measured. And what we did is we went to the methylbromide data and we calculated a 75th percentile flux rate based on the coefficient of variance here for methylbromide and the mean flux rate that we have from our own studies. So let's look at the results for that.

The red bar just shows a normal PERFUM run. And the yellow bar shows the scenario one where we used the higher coefficient of variance. And the blue or, I don't know what you would call that, a green bar shows scenario two.

You had a very small difference between the first, the normal run and the scenario one until you got to maybe a 40 acre field where you saw some differences.

So increasing that coefficient of variance had some effect on that upper percentile buffer zone, but it wasn't that dramatic.

Now, when we did scenario two where we assumed actually a higher flux rate than we actually measured, we saw upwards of, I don't know -- you see here for a 20 acre field where it is about maybe an 1,800 foot buffer zone compared to a 1,500 foot buffer zone.

So it is expected if you put in a higher flux rate you get a higher estimate for the buffer length.

We feel pretty comfortable with the studies we have, particularly, as we added the Guadalupe study that was conducted after the submission.

We have a pretty tight range. So we consider this sort of a bounding analysis on the flux rate as to what kind of variability you might expect on that.

Indoor exposure and time activity. As I said several times, the buffer zone calculations assume the person is at the perimeter for 24 hours and that they are either outside for 24 hours or if they are indoors they are getting the same exposure as they would when they are outdoors.

At least combined together, these likely represent rare circumstances, but they can certainly happen.

One approach you could have is you could try to build that explicitly into the model with probabilities. Maybe you could calculate a distribution of exposures based on distribution of time activity data.

But in starting to think that through, there are some challenges to doing that. We know that most people will not spend an entire 24 hours at the perimeter of the buffer zone.

But actually quantifying the amount of time that they would be away from the perimeter and maybe -- if they weren't at the perimeter, they could still be exposed by being somewhere else downwind of the application. It was a pretty difficult thing to quantify.

From the chemical characteristics and general experience, we know that indoor exposures are generally lower. But in this case, chemical specific data are unavailable. So we didn't feel justified in explicitly incorporating that into the case study for iodomethane either.

But we wanted to do some uncertainty analysis to look at what are the potential effects of these variables.

We did four alternative scenarios. For the first one, we just looked at the indoor and outdoor ratio. We assumed from 8 p.m. to 8 a.m., the nighttime period, a person is indoors with an indoor to outdoor ratio, concentration ratio of .7.

So if we have 100 micrograms per meter cubed outside, we're assuming 70 micrograms per meter cubed inside.

For alternative scenario two, we did the same thing except we assumed a lower I/O ratio of .3.

For alternative scenario three, we looked at time activity. We assumed someone is away from the influence of the field during a normal workday period, from 8 a.m. to 5 p.m.

So this person may live in a residence that's near a field, but from 8 a.m. to 5 p.m. they go off-site and are not exposed.

Alternative scenario four, we combined the two. We looked at someone who is away from the influence of the field from 8 a.m. to 5 p.m., and that they are indoors from 8 p.m. to 8 a.m. with an I/O ratio of .5.

So they are only outdoors from 5 p.m. to 8 p.m. That's not that unusual. Most people spend about 90 percent of their time on average indoors.

What are the results. This shows an example. We used the Manteca data with a five acre field and the Ventura meteorology.

For the maximum scenario, we're calling sort of the normal scenario, we got a buffer zone of 646.

When we assumed an I/O ratio of .7, it dropped down to 597, a little bit different. When we assumed an I/O ratio of .3, it went down to 545. So a little bit different too.

You get a more dramatic difference when you assume that someone is away from the field from 8 a.m. to 5 p.m., about 423, about a third less.

And then finally, if you assume both someone is away from the field for the workday and they have an I/O ratio of .5, you get a buffer zone that's even less than half.

So this looks at some of the potential variability associated with making those conservative assumptions.

Multiple applications. In all the scenarios we looked at so far, we're assuming one field is emitting it at one time. We're not looking at the impact, you know, what is the potential impact of different fields emitting at the same time and those plumes from those fields overlapping.

So the first scenario we looked at, we assumed that different fields were being applied in the same vicinity and that those applications all occur at the same time.

That's really, I think, a low probability event where you would see fields worsening 1,500 feet apart. I think that's a low probability event that you would actually see that occurring, two different fields being applied at the same time and they are 1,500 feet apart, but it could happen.

So it is a low probability event. But there is not a clear basis to establish the probabilities of it happening. So we'll just look at it from a worst case basis.

The second scenario, which is more common, is when a grower applies a large field in different sections.

And we have assumed four quadrants. So you have a 20 acre field and they apply in four different quadrants on either four consecutive days or with a lag period between applications.

This is actually a common practice for large fields in agriculture. And the methylbromide regulations require a lag period between applications to mitigate these effects.

So this is our multiple field scenario where all the fields are emitting at the same time. And these are generally supposed to be different farms. This is the principal application we're concerned about, and we assumed that there were additional applications at these four corners.

Again, I think that would be a pretty rare circumstance, but for the purposes of this analysis, we developed a PERFUM scenario just to look at this in a worst case basis.

We're also assuming one acre fields. The result we get -- we might want to look at this for larger fields, we might get a different result.

But for an one acre field, we got a relatively small impact. The buffer zone was about 270 feet when you have all the fields emitting at the same time whereas the buffer zone was just less than 250 feet for the first source only.

And if you assume that the buffer zone was set modeling something like this, this is the potential discrepancy that you might have.

Now, the reason that this isn't a large impact is in my view is that say the wind is moving in this direction. You are going to have the plume from the original field right here, whereas the only other field in this case that could impact that significantly would be this one. And it is relatively far away.

So just a matter of the geometry of the

situation and the fact that the wind is going to be blowing in one direction at one time. I mean, you are not going to have a situation where this field is blowing this way and this one is blowing that way.

We're assuming these are relatively close together and they are influenced by similar meteorology.

So let's look at the more common situation in agriculture. Where you have multiple fields say a 20 acre field and you apply the field by quadrants. We developed another PERFUM scenario that you could look at this issue.

Say today I'm doing an application on this quadrant. A day ago, I applied this one. Two days ago, that one. Three days ago, that one. And the wind direction is going this way. So my impact is going to be right along this way.

And all three of these fields, these three other quadrants could potentially contribute to the concentrations right there. So let's look at what we get there. Also, let's look at what we get when we have a one day lag.

In the first scenario I used, I just used the emission profiles that we had for the three days after the application in the field study.

In this second scenario, I chose the profiles that we had two days ago, four days ago, and six days -- well, that would be two days after the application, four days after the application and six days after the application. And those are lower numbers than the first three days.

So if you have no lag -- let me back up. It is difficult to define the PERFUM whole field distribution for this example. So what we did is just looked at the maximum concentration buffer lengths.

And when we had no lag between applications, we had a difference of almost, well, about 200 feet. You have about 1100 feet and 1300 feet there. The blue bar is when you had just the first source and the red bar is when you have all sources. If we incorporated a one day lag, we saw virtually no difference in the emissions.

So this model, this scenario could be used to look at that issue, look at how much of a lag time you need before you mitigate the effects between different fields.

And the model in this case will output both -- it will output the distribution simultaneously for what you would get for all sources and what you would get for only the first source, only the main source you re interested in. So you could immediately compare the two.

Finally, I want to get on to some conclusions. We have described the PERFUM model and briefly summarizing its capabilities. We can estimate buffer lengths for a whole field and maximum concentration distribution, which we have talked about. We can probabilistically treat emission rate uncertainty.

We have two additional scenarios that can be part of the PERFUM package where we can look how to model multiple application rates. We can calculate the buffer length distribution on a monthly basis to use for seasonal analysis.

We have an additional program PERFUM MOE which can estimate the distribution of MOEs at any proposed buffer zone that someone is interested in.

We can also account for multiple applications.

That just repeats what I said above there.

Some of the lessons learned from the case study. The diurnal profile of emissions is an important factor in estimating the buffer lengths. If you want to have the lowest 24 hour exposure concentration, you generally want to have your application start as early as possible, early in the morning as possible.

The margin of exposure curve is relatively flat. I think flatter than most other -- many other environmental circumstances.

Multiple exposures may not have a large impact, particularly, if there is a one day lag between applications. I would still consider the multiple exposure stuff we did as kind of a prototype.

We might want to do some more analysis on that. But what we have done so far shows that if you have a one day lag you are going to significantly mitigate the impacts.

So that concludes my presentation. I look forward to questions and discussion.

DR. ROBERTS: Thank you, Dr. Reiss, for a very thorough presentation. I suspect that the panel will have some questions for you.

So let me open it up to questions right now. We'll start with Dr. Hanna.

DR. HANNA: My question is about the situation when you have calm wind conditions. How did you read that? Did you follow the exact approach to using the ISCST-3 model or different?

DR. REISS: Yes. We used the calms processing routine that we had in the ISC model. I would also note that, in several of the field studies we did where we're estimating the flux rates, there were calm conditions.

It was not an infrequent occurrence in the field studies that we had. So essentially, the flux rates we have estimated at least in some cases are kind of calibrated to those sort of calm conditions.

DR. HANNA: I have a second question quickly. Are you able to identify or quantify the uncertainty in a kind of real conclusion? Are we off of a factor of two or 40 percent or 10 percent? Is this can be concluded? Are you able to get this measure based on your analysis?

DR. REISS: There is a lot of literature about the uncertainties in dispersion models. And I think it rarely comes to any definitive conclusion in what I have read.

Some people say dispersion models have a factor of two variability. I think for the particular circumstance that we have where you are looking at an area source, you are looking at estimating concentrations very close to a field, you essentially have flux estimates that go into the model that are sort of calibrated to the model.

That may not be the best circumstance from a purely scientific standpoint in terms of developing a phenomenologically correct model, but from a regulatory standpoint I think there are some advantages there.

I mean, we're using flux rates that we essentially got from concentration estimates downwind. And in some cases, in worst case conditions.

I'm reluctant to put a number to the uncertainty. I think that's just something in dispersion modeling that's not settled. But I think it is substantially less than a factor of two.

DR. HANNA: Thanks.

DR. ROBERTS: Dr. Seiber.

DR. SEIBER: My questions are more on how this will be used in practice. Let me see if I understand it right.

My understanding is that a permit would be applied for if a fumigation was to take place let's say two or five days subsequent. And in the permit application, a given acreage would be specified, certain size, certain application type, rate and so forth.

And then PERFUM would be used to calculate a buffer given that input. This is still predicting, in other words, what might be a protective zone around that application that's going to take place a couple days in the future.

I just want to see if I'm on the right track here.

DR. REISS: What PERFUM will do, I mean, I can't speak for how EPA in California will eventually regulate these things or how California is currently doing it.

But, for example, with methylbromide, the DPR model is used to develop essentially a table that growers can look up based on their application method and the field size. And they can get the buffer zone off that table.

So it is not -- it is used prior to the application. Those are set probably when -- I expect that these buffer zones and these buffer zone tables would be set prior to the chemical being registered. And so the grower would just go to that to look it up. And then those would come from PERFUM or some other method.

DR. SEIBER: All right. And then kind of following that reasoning on then, if there were let's say a residence or a subdivision or whatever within that calculated protective zone or buffer zone, then the permit would not be allowed at least under those conditions.

Am I still on the right track here?

DR. REISS: I might defer to somebody who actually regulates the chemicals. I mean, I don't think they are disallowed. I think there are several options they have.

Do you want to take that?

DR. SEGAWA: What Dr. Seiber described is very similar to what we're currently doing in California for methylbromide, where prior to the application a grower applicator would go to the local ag commissioner with a plan, specify a number of acres, application rate and things like that, and our agricultural commissioner would specify the size of the buffer zones based on the information by the DPR.

And if there is a house or school or something inside the buffer zone, they would have to make changes, break up the field, for example, into smaller blocks in order to do the fumigation.

DR. SEIBER: Okay. Again, carrying this kind of -- I'm just trying to figure out how it would be used and particularly how it would be validated.

Would there be field data collected for some number of applications, let's say, 1 out of 10 or something, and this would probably be up to the state enforcement agency, I suppose.

And what I'm getting at there is, since this was all a predicted buffer zone based on historical weather and so forth, would somebody then go back and say here is what the actual weather was when that application occurred, and maybe even measure some concentrations out at the buffer zone and see if, in fact, the buffer zone had been accurately set.

I'm getting to the question of how are you going to validate and give us some feeling of confidence that this is going to do the job, so to speak.

And I know it is early. And probably this is down the road. But it would help me understand how you are going to tell whether it is within 50 percent or 20 percent or whatever level of accuracy you agree is -- what you are after in a case like this.

DR. REISS: I can't speak to what sort of measurements might be made following the registration of the products.

We did conduct seven field studies where we measured the concentrations downwind of the field. So the model is predicting those field studies. This model is (inaudible) calibrated to predict those field studies.

So we do have measurement data and some experience with collecting that data and using the model to accurately predict what happened in that field circumstance.

DR. SEIBER: But that's a little different. You are using a field to let's say collect downwind concentrations. You are back calculating the flux. Same field. And then you are going to go out and say, here is what the buffer zone ought to be for that field.

But in reality, here is another field over here that needs to be fumigated and you don't have that ability. It is not the same field. It is a different one. If you follow my logic.

DR. REISS: Sure. And we have done seven different field studies. So we have some estimate of the variability that you get between different fields.

I think it was relatively tight distribution. But we'll account for that variability in the model using the ability we have to perturb the flux rate.

DR. SEIBER: Anyway, I don't know whether anyone else can answer questions on this, but maybe Randy would tell us what the actual practice would be.

DR. SEGAWA: Randy Segawa in response to Dr. Seiber's question.

What we did for methylbromide in California was that we have had buffer zones for those types of fumigations since the early 90s. And when we first put in the buffer zones, it was based on essentially data from two fields.

As we got more data, and today we have data on some 30 or 40 fields, we continue to go back, look to see if the buffer zones that we have prescribed were protective in those new fields. If not, then we adjusted the size of the buffer zones accordingly.

DR. ROBERTS: I have Dr. Yates, Dr. Wang and then Dr. Spicer.

DR. YATES: First, just to kind of follow up a little bit on what Jim was saying. There are a number of methylbromide flux studies that have been reported in the literature that were obtained using aerodynamic chambers, but not through this back calculation.

If you look at the distribution of, say, the total emissions from those studies, it varies from somewhere like 30 percent all the way up to about 80 percent.

So it seems to me that you could see quite large variability. And this is for cumulative emissions, which tends to be a little bit better behaved than, say, a two hour average.

If you look at two hour average type measurements, they can be all over the place. So I think that the variability that you are likely to see when you take data collected on one field and apply it to another field is going to be very, very high.

DR. REISS: Right. That's certainly possible. And I think when we get down to what actual input we'll use in the model in terms of a flux rate, we're going to have to look at the variability that we have had for the seven iodomethane studies or more when it comes to that and all the methylbromide data and look at that variability and decide someone is going to have to make partly a policy decision as to how high on that distribution you think you need to be to be protected.

DR. ROBERTS: Was there a follow up question, Dr. Yates?

DR. YATES: I have other questions too. I can get back. It is not related to this one. Maybe there is some follow up here first that should be addressed.

DR. ROBERTS: I know Dr. Wang and Dr. Spicer both had questions.

DR. WANG: This follows along the same lines, so it is probably a good time to ask.

DR. ROBERTS: Go ahead, Dr. Wang.

DR. WANG: One of the main conclusions you said is the timing of application has a significant impact on the maximum concentration that will occur.

I think one of the explanations is that there are two things occurring. Once you apply the fumigants, it is usually subsurface. So the physics, the diffusion process in the soil is that the center mass will move -- has a less -- that process, the time that's going to come out is less dependent on the ambient atmospheric conditions than once they reach the air. The same time, the diurnal pattern is that the temperature change, the pressure, the stability.

So these two factors, if they tend to coincide, you may amplify the concentration. Meaning if you apply, say, around noon or late afternoon, then just about early afternoon they may come out. And then that's also the time now you tend to have either unstable condition or stable condition.

If they come out near the evening, then that's when you're going to see a very high concentration.

But to follow up on that is that you posted several factors that has uncertainty sources. And that seems to be the main point. My question is that how do all these uncertainty sources fit together or how would you integrate them in your assessment since you cannot (ph) really just look at one at a time. They tend to occur all at the same time.

So if you do a true risk analysis, you may look at the most, the worst case scenario. Not just for one source, but maybe multiple sources could be occurring simultaneously.

Do you see my point?

DR. REISS: I think so. Let's take the flux rate in meteorology, two of the key variables. The model runs through five years of meteorological data. So it is for every -- it is basically a time series of -- I think that comes out to about 1800 days or more.

So each day it is choosing a flux rate based on that variability. So you are occasionally going to have the worst case meteorological condition happen when you have the high highest flux rate that you get from your uncertainty analysis.

I mean, we don't just -- as a matter of risk assessment, we don't want to just simply compare all of the worst case variables together. We think as a matter -- it provides more information to do this in a probabilistic way that provides the actual probabilities of -- well, an estimate of the actual probabilities of all these things occurring simultaneously, and then, thus, an actual probability of observing various concentrations.

DR. WANG: This could be all doing a joint analysis of the different uncertainties. It could be run --

DR. REISS: It is run jointly. The meteorological variability and the flux rate variability are run as a joint analysis.

I think with multiple applications it would be desirable to have -- to incorporate that into the model in a probabilistic way, but I have -- you would have to choose probabilities for whether the multiple application occurs -- whether it is occurring on a given day, how often would a multiple application occur, how close together would the fields be, in what direction the impacts would be.

I mean, it would matter -- the orientation would matter relative to the wind speed of whether it would be a multiple impact. Choosing all those probabilities I think is very difficult to do.

So I don't -- I couldn't explicitly incorporate it into the model. But we developed these additional scenarios that people can run to look at the impact of multiple scenarios.

DR. WANG: Currently, PERFUM, the model, does not join all these uncertainties together in a more integrated manner to assess, say, the risk of maximum concentration. Does it do that now?

DR. REISS: It does. It integrates the uncertainties in the meteorological inputs and the flux rates.

For the meteorological inputs, it is running five years of meteorological data. So it is accounting for the uncertainty or not the uncertainty, but the variation that you observe in the actual environment by running five years of data.

And by using a statistical approach to estimating the uncertainty in the flux rate, yes, it is jointly -- the model jointly accounts for the uncertainties in both of those variables.

DR. ROBERTS: We have Dr. Spicer, then Dr. Maxwell, then we're going to go back to Dr. Yates and Dr. Small.

DR. SPICER: I was curious if the PERFUM methodology had been applied to methylbromide. For example, in the tables for a given application rate at this point in time you look up an exclusion zone of 1,000 meters, then does the PERFUM methodology -- how would that compare under the same set of inputs for the methylbromide? Is it longer, shorter?

DR. BARRY: You mean for the buffer zones?

DR. SPICER: Yes.

DR. BARRY: One of our colleagues at DPR did do a comparison of taking the single maximum distance versus all the way around the field. And the maximum distance, of course, would always give you a longer buffer zone. And also to expand on what Randy Segawa was saying earlier, we had 34 studies and only two of those studies had buffer zones shorter than what was modeled when we measured on site in the field.

So we have also done that field assessment, too, for methylbromide.

DR. SPICER: It is quite evident that you have done extensive work on the methylbromide. That's all I was asking, trying to ask, was if you applied the PERFUM methodology to methylbromide, would PERFUM predict longer distances or shorter distances or roughly the same?

DR. BARRY: They would be roughly the same.

Because what Rick has done in both -- basically, what we have done with five years of weather data, after we had used our standardized weather conditions, see stability in one point for meters per second (ph), we assembled data from CIMIS stations in California and did similar analysis to what Dr. Reiss has done.

And the single direction would give similar measurements to what we would get using PERFUM. And the multiple direction would give similar results to the multiple direction. Because he's basically doing the same process that we did for validating our methylbromide buffer zones.

DR. SPICER: In this process, though, what you are doing is aren't you using essentially data from remote locations and applying them to a local dispersion scenario?

DR. BARRY: Yes. That's true.

DR. SPICER: So there is no way to account, for example, for drainage flows or flows that are influenced by local topology, those sorts of thing?

DR. BARRY: No. I would say that we do not account for that, because it is remote from a specific field. Because you don't know where the applications are going to occur. But we are using stations that are in agricultural areas where the applications commonly occur.

They are not very far away from -- in fact, some of them are, actually, in the same geographical area, for example, Watsonville or Salinas (ph) where much of the methylbromide is applied or the fumigants are applied.

In California, the Central Valley, in some of the areas where these fumigants are used it is flat and open. You are going to get pretty consistent meteorological conditions between areas that could be 20 miles apart.

But that, of course, is going to be specific to a region. Not all parts of the country are going to be like that, of course.

DR. ROBERTS: Dr. Maxwell, then Dr. Yates, then Dr. Small.

DR. MAXWELL: This is Dave Maxwell, National Park Service. I actually have three questions, but I'll go one at a time.

How is mixing height addressed? Is it addressed any differently through ISC or are there multiple sets of mixing height data used?

DR. REISS: We're talking about concentrations relatively close to the field, and the plume generally doesn't rise up to the mixing height within that distance.

What we did is we analyzed -- for the National Weather Service data, we actually used the mixing heights that came out of PCRAMMET using upper air data. And then we did a sensitivity analysis where we changed those mixing heights, just a nominal value of about 300 meters, which is what we used for the other data sets, and it changes the answer by a very, very small amount.

So the mixing height isn't a large factor given the concentrations are close to the field.

DR. MAXWELL: Thank you. Next question. I don't know who could answer this. Who are the health effects of iodomethane?

DR. REISS: What are the health effects. The endpoint we're looking at is something called late resorptions in a rabbit study. It is a reproductive outcome, basically.

DR. MAXWELL: The last question is how is the 120 micrograms per cubic meter threshold determined? Who determined it?

DR. REISS: It is determined from this rabbit study. So there was a no effect level. There was a concentration that the rabbits were exposed where no effect was found.

There are various conversions to take that concentration and convert it to a human equivalent concentration. And actually, DPR and EPA kind of disagree on how to do that. I couldn't explain to you the mechanics of that, but you basically get a human equivalent value from that study, from the animal study.

That's commonly how -- if you are familiar with criteria pollutants, there is a lot of human data commonly with pesticides. You are looking at animal data. That's why you generally look at 100 fold safety factor, tenfold to account for inter human variability and tenfold to account for the extrapolation between animal and humans.

DR. ROBERTS: I think Mr. Dawson would like to respond to those last two questions as well.

MR. DAWSON: I want to make it clear that 120 value is really for example purposes. Along with this work with the exposure models, we're currently in the process of further evaluating the toxic effects of iodomethane.

Again, I just remind everybody this is for example purposes only, and we're still determining the final numbers.

DR. ROBERTS: Thank you for that clarification, Mr. Dawson. Dr. Yates I believe is next.

DR. YATES: I have two questions.

The first one, in the studies that you did on the flux rate, you reported the interval flux as like percent of the application. Did you go through -- for all the data that you had, did you sum up the percent losses to see if they exceeded 100 percent?

DR. REISS: Yes. There were a couple that did, but not by more than a couple percent. So I mean, within the experimental variability that you would expect.

DR. YATES: Do you think it is reasonable that you would have 100 percent loss of the fumigant?

DR. REISS: That's what we're observing. In some of the studies, we didn't see nearly that. But yeah, I suppose it is possible. Certainly.

DR. YATES: And let me move on to the second question because it may help to answer a little bit on that, too.

You did a direct and indirect comparison. But you didn't really describe much about how you ran the study, the direct flux study. Could you kind of just tell us -- you said it was aerodynamic. But this was a flat fume with a high density polyethylene tarp?

DR. REISS: Yes.

DR. YATES: What kind of instruments did you have out in the field, do you know?

DR. REISS: I can't speak to that. I wasn't present during the study. I really just analyzed the data. I don't know if there is someone here that can speak to the instrumentation.

MR. GILLIS: My name is Matt Gillis from Trical. I was involved in setting up the equipment for measurement of direct flux during that study.

The equipment used was thermocouple temperature censors and anemometers placed at a log gradient above the surface. Air sampling pumps, air samples were collected at those same heights. And then that data was entered into flux calculations for aerodynamic flux.

DR. YATES: So the equipment was all put in the middle of the field. And you had a tarp there. How did you -- what efforts were taken not to do any damage to the tarp?

MR. GILLIS: A ramp was placed along the glue seam of the tarp to access the samples. The air sampling method utilized a wind vane where the air samples were actually collected upwind of the point of attachment into the field.

So the wind never blew across the mast post. It was always clean air in the gradient.

DR. YATES: Thanks.

That's very similar to the kind of studies that we have done with methylbromide. And under very warm conditions, you can have fairly high flux rates, but it would still seem that 100 percent is probably -- might be a little on the high side.

So I guess there aren't too many of these studies that look at the direct and indirect methods for estimating flux and make a comparison. So they tend to be of interest to me.

But are planning to publish the information at some point.

DR. REISS: We are, yes. One comment I would make on that. If it is less than 100 percent, say we're overestimating that, that would be an undesirable situation. But we still -- our flux rates are calibrated, essentially, to our model, meaning if that 100 percent was overestimated, then we overestimated the flux rates.

But because we've, essentially, calibrated the flux rates to the model, there is not a particular bias in any direction.

DR. YATES: Right. I agree about the calibration for the particular study that you obtain that information for. But when you try to apply it elsewhere, then I think where there is mismatches I think can tend to be a bit of a problem in terms of developing buffer zones at the other place.

So it really would be ideal if the indirect method would give a nonbiased estimate of the true flux from the field.

But anyway, I just wanted more of an explanation of that study more to interpret your figure. I hope you do publish it, because, like I say, there aren't very many studies out there that do that comparison between direct and indirect method.

DR. ROBERTS: Dr. Small, Dr. Bartlett, then Dr. Baker, then Dr. Spicer.

DR. SMALL: I have a very few specific questions here.

In your uncertainty analysis method, you have sequential calls for the random number generator, which you then apply, originally to the T distribution, now to a normal distribution that's described as standard air of your estimate of your slope, which is used to estimate your flux.

Do you call that random number generator sequentially, independently? In other words, in one period you call it, you get a value, and the next one you have an independent call? And if so, what are those time steps that you operate over?

DR. REISS: We have operated -- we set the model -- we have periods in the flux study which range from 2 to 24 hours. And I only choose a random number for each period.

So I perturb the flux for that entire, say, 2 to 12 hour period using one random number call. And the reason is because if I were to break those into individual hours, I would lose a lot of --

DR. SMALL: But the sequential ones are independent. If there is one two hour period followed by a four hour period, those are independent --

DR. REISS: Those are independent because they were independent measurements --

DR. SMALL: How many of them are there typically in one day?

DR. REISS: Anywhere from two to four.

DR. SMALL: And then when you calculate your buffer zone, you use a 24 hour average concentration?

DR. REISS: That's correct.

DR. SMALL: Not a maximum concentration?

DR. REISS: Using a 24 hour -- that's based on the advice we have from the toxicologist that that's the --

DR. SMALL: Using the 24 hour average with independent calls. Good.

Second question. I notice in the report that you have a couple cases where you miss the early first day ambient concentrations because you couldn't get set up quickly enough. So you used the second day values back on the first day.

How many of those cases are being used in your current model now? Which specific ones have that correction, second day used for first day?

DR. REISS: I can't answer which specific ones have that correction.

I know the Watsonville and Plant City I believe don't. I think most of the others have at least a few hours where we had to borrow, as we say, from the second day.

DR. SMALL: And back calculate to the first day.

DR. REISS: It is still an estimate of the flux rate over the 24 hours following the application.

DR. SMALL: Third question.

How did you treat your -- you mentioned level of detection. When your data for your ambient concentrations are at or below the detection limit and you use those in your regression model, do you use the detection limit, zero, half the detection limit, some other approach?

DR. REISS: We use zero, but I have done sensitivity analysis. Unless you do the log transform method, it makes no difference at all.

DR. SMALL: It is harder to use zero with the log transform.

DR. REISS: That's true. But you have to do something. That motivated my using the limited detection. But most of the values, I mean, could range up to 10,000 -- I mean, the maximum values generally range up to 10,000 fold more than the limited detection. And they generally dominate the regression result that you get.

DR. SMALL: This is motivating my last sort of area of questioning. That's on this whether or not to include the intercept or not. That's on the issue of background.

Any idea for iodomethane or other things whether or not there is some type of background? Do you have upwind measurements.

DR. REISS: It is virtually zero. There is no other sources. I mean, right now it is not a regulated product, not a registered product. So there is not other fields being applied.

DR. SMALL: So you expect it to be zero.

DR. REISS: We have in a few studies background measurements where it is basically zero.

DR. SMALL: Thank you.

DR. ROBERTS: Dr. Bartlett, then Dr. Baker.

DR. BARTLETT: Paul Bartlett.

One of one of the questions I have is 24 averaging, the 24 hour averaging. I realize -- relating to questions about physical chemical, biological properties of the substance.

One is why are we concerned about a 24 hour exposure? This is the first question. Is this an acute effect? Does the substance -- is there any bio cumulation for toxic effects?

If it is an acute effect, it seems to me that what the peaks are in like three, four hours might be more relevant. We see in the area of particulate exposure right now it is regulated at 24 hour averaging when what is relevant is, if it is children or elderly people, if they are exposed to a peak level for a few hours. So it seems like in some sense regulation maybe moving to a shorter time period for exposure.

The other question that's related to this averaging is the physical chemical properties reemission. You mentioned that methylbromide is sticky indoors. I'm not sure what this substance is like. That implies a dermal and other forms of exposure might be relevant or there might be a lag effect if this does get indoors. It may be trapped in there for some time.

And also, this is an issue for multiple sources, different time periods. I want a little clarification on that.

DR. ROBERTS: Not to cut you off, Dr. Reiss, but I think I understood from Mr. Dawson that the toxicology of this particular chemical is currently under evaluation. And presumably, that would include, not only the endpoints, but the most appropriate dose metric. But I'll let Mr. Dawson respond.

MR. DAWSON: That's correct. We spend quite, along with the development of this or consideration of this exposure model, we're going through quite an extensive process to define what is the appropriate duration of exposure that we want to look at.

At this point, all factors are pointing to us considering a 24 hour interval is the appropriate averaging time. But again, we still have some final decisions to make on that based on the toxicology data that we have for this particular case.

DR. ROBERTS: But presumably if another interval appeared to be more appropriate from your analysis, the model could valuate the concentration over a different interval.

MR. DAWSON: Right. And whatever the factors pointed to is the averaging time that we would want to look at.

DR. BARTLETT: The question with your results is that diurnal variation is quite high. So if people are being exposed regularly during the day, for instance, when it is high and you are using a 24 hour average like of a sequential application or something like that, I think the hourly data would be valuable to some extent. Especially if we don't know how it is going to come out. That that at some point may be of interest.

DR. REISS: Let me comment. One thing on the diurnal variability. There is a large diurnal variability in the flux rate. But because the lower flux rates occur at night, which are more stable conditions, you have a much lower variability between actual concentrations. That's one point.

The model calculates the hourly data. The only reason we don't output the hourly data is, one, because we have been told by the toxicologist that they are interested in the 24 hours.

If that changes, we can change that. But from a matter of computational efficiency, it is much more difficult to get that 24 data summarized. So just as a matter of efficiency, we have limped it to 24 hours for now.

DR. ROBERTS: Mr. Dawson.

MR. DAWSON: Just to follow up. Jim Jones mentioned this morning we're looking at six chemicals in our analysis of soil fumigants. In some of those other cases, we're focused again on much shorter durations, for example, hour type intervals for exposure.

So we're definitely interested in this type of component with the system looking at the shorter averaging times.

DR. ROBERTS: Did that answer your question?

DR. BARTLETT: Yes. I just would like to throw out one other question.

When you screened out temperature as an effect for some -- you have a lot of unexplained variance between your different studies. And of course, you have a history with methylbromide.

But there are a lot of meteorological factors and other known factors and multiple regression or some other ways to clean it up -- may point to that they may be significant when you said that they weren't significant. What are your feelings with that?

DR. REISS: That's possible. We're seeing -- actually, there seems to be some dependence with wind speed. You get a little bit higher flux rate with higher wind speeds.

There is bias in the model, because the model is less accurate for highly convective conditions, the ISC model, that is. So we have to consider that when we look at that conclusion.

But we're just getting to the point now where we have seven data points and we're going to get a few more. So I think it is something that we would want to pursue.

In an ideal circumstance, you would be choosing a flux rate each day that depended on the -- that was a function of the meteorology. And there is some explanation for -- the meteorology does explain to some extent.

But we haven't been able to, and I don't think they have been able to do it for methylbromide either, to develop an equation, so to speak, that explains all that, because it is very difficult to get one of these data points and there is a lot of variability out there.

DR. BARTLETT: I just throw out one thing from a recent experience in work on PCBs, is that topography had a lot more influence than we thought in the difference between meteorology between the station and the microclimate of the actual measurement. So that can account for quite a bit of difference sometimes.

DR. REISS: With the flux studies we did, we had on site meteorological measurements and very little topographical differences between the measurements and the actual field.

But yes, in a real circumstance you would have that.

DR. ROBERTS: Dr. Baker, followed by Dr. Spicer, Dr. Portier, Dr. Wang and Dr. Seiber.

DR. BAKER: I need just a little bit of background on the application. Is the application performed by a trained specialist, some sort of certification program?

DR. REISS: Yes. As you can see from those pictures that we showed, it is a pretty sophisticated operation. There are trained companies that do those applications.

DR. BAKER: Is there any potential for mishandling and subsequent release of the material which could possibly lead to some lingering background in the field studies but also might contribute to some exposure in a real application?

DR. REISS: Exposure in the real application, meaning the exposure to the workers, is also measured during many of these field studies. And it is not that we're not concerned about it. It is just a separate issue that is being dealt with by separate analytical methods and separate mitigation methods.

DR. BAKER: You notice the impact of the time of the day of the application. And certainly there was a big difference between several hours of the time difference.

In the uncertainty sensitivity analysis, would you comment on including the potential to include maybe a small one or two hour perturbation as a probabilistic feature in terms of the start time, because I know sometimes I intend to start things at 8 in the morning, but it more likely is 9 or 10, versus, I guess, the current application.

As I say, you would rerun with a new start time and just have a separate run in those two options of including that probabilistically versus rerunning the model.

DR. REISS: Right. We have been a little reluctant to decouple the actual meteorological diurnal meteorology we observe in the studies and just say that we would have the same profile if we started several hours later or several hours earlier.

But I think the point you make about at least a few hours could be something that is worth considering. And that might help to refine.

At some point, from a mitigation standpoint, if you are going to account for the time of the application and calculate in the buffer zone, some decisions are going to have to be made about that. And then that might be a more appropriate time to consider how to model it -- that's whether it is even feasible to account for that in mitigation.

DR. ROBERTS: Dr. Spicer.

DR. SPICER: One of the plots that you presented was of ambient temperature as a function of the flux. Was that the air temperature or the soil temperature?

DR. REISS: It's the air temperature.

DR. SPICER: And so because the soil is covered, would the soil temperature not tend to be higher after it's covered with this tarp than the air temperature would be?

DR. REISS: It would be much lower. I have assumed at the moment that they would correlate, the ambient and soil temperature would correlate. But I probably should make that graph for soil temperature as well.

It's a graph we made, I'll admit, the last couple days. But I think we want to do that with soil temperature and wind speed as well.

DR. SPICER: But it is recorded, though.

DR. REISS: It is recorded. We have that data, yes.

DR. SPICER: Although it is not a direct comparison to this, there is a substantial amount of literature that has to do with estimating flux from liquid pools. And that's shown to be proportional to the vapor pressure, which would be a function of the temperature and then also the wind speed.

DR. REISS: Right. I think if you look at the physical chemical things that are going on, you could have a situation where it is volatility limited or a situation where it is diffusion limited.

You can have a rate limiting step. It would depend on the chemical and the circumstance. You have a very -- you have a tarp, a plastic tarp that limits the diffusion.

So the likely -- it is quite possible that the tarp, the diffusions of the tarp is the rate limiting step in the volatilization.

DR. SPICER: So it does actually diffuse through the tarp, then?

DR. REISS: That's how it gets out. It diffuses through the tarp. Actually, some of the diurnal variability you observe is likely the result of the diffusivity of iodomethane changing with temperature during the day and night from the tarp.

So the tarp becomes a little more permeable during the daytime when it is warmer.

DR. SPICER: It was discussed earlier with regard to the flux measurements in the area of the curve (ph). What was the difference between the smallest closure of mass, if you will, between the integrated flux rates and the largest one?

DR. REISS: You are talking about the cumulative.

DR. SPICER: Yes.

DR. REISS: The highest went up to around 100 percent. I believe we're looking at maybe 70 or 80 percent at the lower end, but I'm not absolutely sure of that.

DR. SPICER: That's certainly encouraging. Let me make sure I understand something else about the flux measurements. The concentrations were made uniformly at one to one and a half meter height?

DR. REISS: Yes.

DR. SPICER: So there was no accounting for any vertical variation in the concentration, then?

DR. REISS: No. We did not account for vertical variation, except during the direct flux, which is really -- that's how they did it.

DR. SPICER: Sure.

Of course, your point is taken that the model's calibrated for those tests. However, implicit in that you are looking at the atmospheric dispersion. So the vertical dispersion coefficients will also depend the meteorological conditions.

You are having to assume by doing that that you are getting the vertical dispersion coefficient correct.

Now, granted, because you have all of these at one elevation, you are not having to assume that about the lateral dispersion coefficient. But that's the only issue that might be of concern to me, is that there is no measure of that vertical concentration distribution.

DR. REISS: That was one of the reasons we did the direct flux calculation, is that we would have an independent measurement of the flux. And it is only one data point, but the results were very encouraging. We got a very similar result.

I understand there could be some variability associated with the vertical dispersion coefficients as well.

DR. SPICER: And that would especially be important during the calm conditions too.

DR. REISS: Right. I would point out, we -- we had a lot of measurements with calm conditions, a lot of the field studies we had relatively calm, even during the day.

DR. SPICER: I might not understand that completely. How did you actually model that, then, with the ISC since the ISC essentially --

DR. REISS: We ran it without calms. We ran it in the no calm -- we turned off the calms processor to do that.

DR. SPICER: So you've got the calms turned off when you calculate the flux, but you have data that has the calms in it.

DR. REISS: That's right.

DR. SPICER: What importance would that have, do you think?

DR. REISS: Well, if you turn off the calms processor, you are essentially modeling the concentrations with a one meter per second wind speed when you have a wind speed below that.

So you are estimating a higher concentration than you would have, I'm sorry, a lower concentration than you would have. Let me make sure I get that right.

You are assuming a higher wind speed than you observe when you run it without the calms processing. So if you assume a higher wind speed, you get a lower concentration.

So I think the way that works is you would underestimate the flux rate.

DR. SPICER: It gets confusing.

DR. REISS: It was confusing. Let me think about that. I'll answer it again after the break. How about that?

DR. SPICE: Fair enough.

DR. ROBERTS: Dr. Portier is next.

Before we go to Dr. Portier, let me fill you in. I think we'll continue to take questions if we have them. But probably no later than 12:30, then we'll break for lunch.

If we still have more questions, at that point we'll take them up again after lunch.

Dr. Reiss, I assume you will be available after lunch if we continue to have questions?

DR. REISS: Yes. I'll be here.

DR. ROBERTS: Let's continue with Dr. Portier, followed by Dr. Wang and then Dr. Seiber.

DR. PORTIER: Ken Portier, University of Florida. Could you bring up slide 11, continuing with the model?

In reading the material -- I understand the basics behind the model, but can you assure me that this model has been developed for a height of zero? There was a lot of discussion and things about the point sources, line sources, area sources, a lot of what happens at the top.

My question is when you move that point source down to zero, does this model -- has this been calibrated, validated for the zero height position?

DR. REISS: Yes. The model -- actually, EPA has done some recent validations, because they are developing a new model AERMOD, to replace ISC. It is not yet approved. At some point we'll have to grapple with that issue as to whether to adopt it.

But in the course of looking at AERMOD, they have done comparisons between AERMOD and ISC using the available tracer study they have. And they have several area sources that are ground level area sources.

And in those comparisons, the predictions are actually better than four point source. So yes, I think there -- it has been looked at. There is every reason to believe it would be better for an area source because, well, in this case we're measuring concentrations closer to the field. You are dealing with less meteorological variability, I think, as you go up in the atmosphere.

DR. PORTIER: I had a follow up question on Slide 18. I was trying to understand what the sequence of the simulation is.

Essentially, for each day, for this grid, you are going to simulate all the concentrations for 24 hours, average them up. You get an average for each point on this grid. You can develop this red profile for every day, 1,825 days, five years. Correct?

DR. REISS: Correct.

DR. PORTIER: And then to a certain extent you find the maximum distance to the point. And that's the --

DR. REISS: Maximum distribution.

DR. PORTIER: -- maximum that you have a distribution of.

DR. REISS: Correct.

DR. PORTIER: If I overlaid all 1,825 of these contours on this same graph, they would be all over the place. Right?

DR. REISS: That's correct.

DR. PORTIER: I could figure out for every point how many of these 1,825 days is inside that contour or outside that contour, figure out a probability of exceedance --

DR. REISS: That's exactly the way we have done it. So the 95th percentile, when you look at the whole field distribution, that's exactly what it is. 95 percent of those receptor points -- not receptor points, but buffer length, slivers, are below it and five percent are above it, if you were to define it that way.

DR. PORTIER: And then when you cut it back and did monthly or other areas, you did it the same way, but just for limited tenfold patterns.

DR. REISS: That's correct.

DR. PORTIER: That was just clarification, an easy question.

DR. REISS: I like it.

DR. ROBERTS: Dr. Wang, then Dr. Seiber, then Dr. Shokes.

DR. WANG: A main contribution or strength of this PERFUM is inclusion of the different databases in the meteorology and -- from the ISC models. There are four sources that you have tapped into, the National Weather

Service, then CIMIS and FAWN and ASOS.

Seems to be the primary goal using this is to compute the stability factor so that you can use that model to backtrack all the dispersion processes.

Would you think the other parameters from these sources we have been discussing like, say, possibly temperature or other things may also be used in addition to just the stability factor in order to estimate the risk?

DR. REISS: Actually, the model for the meteorological data for each site for each hour we get a wind speed, wind direction and a variability in that wind direction will go a long way to determining the result.

The temperature is included, the ambient temperature is included in the model. But since this is not a buoyant source, it actually has no impact on the dispersion calculations, at least.

But we do have temperature. If there is some way to eventually like model the flux rate as a function of temperature, which from our limited --

That's why we did that plot of the ambient temperature versus the flux rate. Because we have the ambient temperature for every day of all those data sets, historical data sets. But there doesn't appear to be any obvious way to use that.

So in addition to stability, the wind speed and wind direction are part of that data base, and the temperature is also part of the database, although it doesn't affect the calculations.

But if at some later date that could be incorporated into the model, then that data are also available from all of those data sources.

DR. WANG: Just to follow up, one small point. The ambient temperature and the soil temperature, there is a time lag. So soil temperature tends to follow behind air temperature from the heat transfer in the porous media, the theory tells that.

To follow up on the sources of meteorological data, have you looked into the Mer flux (ph) data, the basis, and the variability of those information? Have you considered that might be useful?

DR. REISS: I'm not familiar with it. Could you explain what that database is?

DR. WANG: I think DOE and some other agencies -- it is not a particular agency, but it is more on the collection of different groups. Mostly for others -- gas emissions at the lan ams (ph) for interface could be greenhouse gases and other source of information.

So that the data usually tends to be fairly detailed in terms of radiation (ph), wind speed, temperature and even soil temperature and all those tends to be available.

DR. REISS: It is worth looking into. But I think we have done a pretty good job of looking for meteorological data near where these things are -- these applications occur.

The FAWN data, I don't know if that's ever been used for dispersion modeling. ASOS is just coming on line, really, in terms of being used in dispersion modeling. So we went far and broad, I think, to get meteorological data that characterized the growing regions.

DR. WANG: The other thing is assessment on the buffer zones, the distribution, you did that for every day of the year. Would you think that's realistic for fumigation to occur any day of the year? Agronomic considerations is usually a main constraint.

DR. REISS: Absolutely. We can incorporate into the model if you knew for a particular fumigant or for a particular fumigant being applied to a particular crop, you could use either those monthly distributions or we could incorporate into the model a way to calculate the exact growing season.

The reason we did this case study analysis is that, with just assuming an equal probability of application on any given day of the year, is because we're looking at all different seasons in all different states and all different crops and potentially even different fumigants. It was just hard to generalize what those probabilities would be.

I would say that we're conservative in the sense that we're calculating buffer lengths during those January, February, November, December months, which are least likely for an application to occur.

DR. WANG: Because if you do this focus study in the most likely period of time that fumigation may occur, you likely could focus on some other sources of uncertainty so that it would reduce your requirements for computation of other needs. So that may be advantage.

My last question is I know we already pretty much beat that to death on the concentration, 24 hour concentration topic. But I'm still wondering shouldn't that be a time average, time weighted average in 24 hours?

Meaning that if you measure the concentration on the fixed interval every hour, now you can just take arithmetic mean. But usually, that may not be a same increment. So you may have to do a time weighted. Is that how you did it?

DR. REISS: In the PERFUM model, we calculate a concentration for every hour. So it is just a simple arithmetic average to get it. One of the advantages of the model is that in the ISC model you can't calculate the concentration from, say, 9 a.m. one day to 9 a.m. the following day.

You could output the hourly data and do it, but it is computationally difficult. By actually going and using the ISC code, we were able to get it. So we could calculate a 24 hour average from the start of the application to the following 24 hours.

DR. ROBERTS: Dr. Seiber.

DR. SEIBER: Your model has been run and calibrated and developed really with using a flat terrain without obstructions and no complexity, as I understood it.

So it is just a simple question. Do you have some plans to bring that in in future iterations of the model? And if so, how do you see that developing?

DR. REISS: I think it is going to be, really, the question of whether the terrain, downwind terrain is going to be a big factor.

The ultimate toxicity level that EPA chooses is going to be a major factor, because you are less likely to have influences from terrain if the buffer zones are 100 feet or 300 feet than you would if they are 1,000 or 1,500 feet.

Now, the ISC model can be run -- we're running it in a rural mode. We also can run it in an urban mode which accounts for some of the -- if you had more obstructions that were causing more turbulence, you would get lower concentrations. It can also explicitly account for terrain in topography.

Most of these things would generally more likely than not reduce concentrations instead of elevate them, although, there could be other circumstances.

It is just when you are looking at trying to run the model for five years and trying to generalize it to all different circumstances. The most sensible way we thought was just to run it with a flat terrain. But we could look at that from a sensitivity standpoint.

DR. SEIBER: This is somewhat related to that. A number of these coastal areas which, of course, is where the fumigant will be used, not only, but in major amounts, they have fog at different times of the year. They will have either the ocean fog or maybe ground fog at some times.

Have you considered that at all in how to deal with the complexity of fog?

DR. REISS: I'm not sure what impact the fog would have beyond what is already included in the historical meteorological data which accounts for the stability.

I don't know whether it is less likely that an application would occur during fog. That's one element we haven't dealt with about the likelihood of an application occurring given the meteorological conclusions.

We have used the historical meteorological data. Our assumption is that accounts for the variability that we would observe.

DR. SEIBER: I guess one place where it might be relevant, going back to a very earlier question, is on particulate matter because you have actually introduced another phase into the atmosphere then.

That's more a question for the toxicologist, whether that would change the exposure or the effects.

DR. REISS: We really are dealing with a gas. There is no real particulate exposure. Because, as I said, it is injected into the ground. Just by gravity, it is not possible for a particulate to escape from the field and emit downwind.

DR. SEIBER: Then finally, just very quickly. When you talked about having measured a value and no methyl iodide was being used in the vicinity, and you said there is probably no natural source of it, but there could be an analytical interference that is measured as though it was iodomethane, but it is not, in fact. Has that been explored?

DR. REISS: That has been the case, that there is some finite amount. It is not affecting our flux estimates just because the concentrations at the monitors downwind, as I said, are like 10,000, 100,000 fold higher. And that really dominates the estimation of the flux rate.

I can't speak expertly on the analytical methods, though.

DR. ROBERTS: Dr. Shokes followed by Winegar, Yates and lunch.

DR. SHOKES: One of the things you mentioned in your report in your uncertainties was the different factors in the soil that you really didn't have a good measure of. Yet, we see a tremendous effect, it looks to me like, of temperature when you put the fumigant in and you get a lot of emission.

Ideally, with a fumigant, the idea is to put it into soil and bring the lethal level up to some point so you can kill nematodes or weeds or pathogens or whatever for a given period of time.

In an ideal world, we would like to be able to see the efflux of that from the soil over a period of time in a little lower rate, I would imagine.

Has any work been done to determine the effects of things like, for example, I think soil and moisture might be a factor with a lot of fumigants. And perhaps not as much with iodomethane, but certainly with some of the others.

And is very much known about that, and could something like that be put in so that -- if we get to the point to where we regulate this, we may want to have a prescriptive application that determines when could we get the conditions to be the safest possible to have as efficacious as possible.

DR. REISS: That would certainly be desirable, yes, like you said both from an efficacy and an exposure standpoint.

It is very difficult to get, as I said, one data point, when these flux studies are pretty substantial operations. We now have seven data points and we probably should start to do some more analysis on the variability among the weather conditions to see if we can explain some of the variation we see in flux rates.

But as yet, we haven't found any significant correlations with any of those variables. It is not to say that those variables aren't impacting temperature, soil, moisture. It is just that there is a lot of different things that are impacting. You may need a much larger set before you get a real handle on that.

DR. BARRY: I was going to comment on detecting the effect of different factors on the back calculated flux. We only have one direct flux for methylbromide. But you are asking about soil and moisture and someone else asked about tarping and different soil types.

And we actually did for our 35 or so studies, 38 studies calculated emission ratios. They were 24 hour averages. And the only differences we could detect were tarped versus untarped and bed versus broadcast.

And I think the reason is that there is so much variability in the measurements that it's -- these are finer differences we're talking about. And you might need to have more studies or -- there is variability on the measurement.

It is difficult to separate out those finer points, I think. And we had soil type. We didn't have soil temperature for all the studies. But we could not detect soil type differences.

Like I say, those are the factors, tarp versus untarp, bed versus broadcast. And that was it because of the variability.

DR. ROBERTS: Dr. Winegar, then Dr. Yates.

DR. WINEGAR: I have several questions. First, chemistry, a little bit. Iodomethane, do you know what the vapor pressure is relative to, say, methylbromide, which is what I have more experience with?

DR. REISS: It has a lower vapor pressure than methylbromide.

Do you know the exact numbers, Jim? Jim Platt is a chemist with Arvesta.

MR. PLATT: Jim Platt, Arvesta Corporation, consultant now. Previous project manager for this thing.

For methylbromide, the vapor pressure at ambient temperature 25 degrees is about 1600 millimeters, I believe, and methyl iodide is about 450 at the same temperature -- millimeters of mercury. So it is about one-fourth.

DR. ROBERTS: Thank you.

DR. WINEGAR: I don't have my calculator with me. What is 120 micrograms per cubic meter in terms of parts per billion, which is what I'm used to thinking in?

DR. REISS: Do you know it, Jim?

MR. PLATT: Jim Platt again. When the correction factors are put in there for -- I believe it is .02 parts per million, it translates to the 120 micrograms per meter cubed.

DR. WINEGAR: 20 PPB.

I imagine you have done this. But looking at slide 21, your structure of PERFUM versus ISC, I presume you have run some test cases of running -- you said you modified the Fortran code that incorporated the ISC code into PERFUM.

DR. REISS: That's correct.

DR. WINEGAR: You have run test cases devoid of any of the additional stuff?

DR. REISS: I have. In fact -- yes, I have run the ISC model and run PERFUM and compared just the output I get from the ISC model to make sure that they are identical.

We also developed -- because the ISC model is so large, I couldn't run it in debug mode in my computer. So I developed basically a shell ISC so I could run the whole model in debug mode.

DR. WINEGAR: On slide 27 you talk about the linearity between flux and concentration in ISC. How about the linearity between application rate, the relationship between application rate and flux?

It has to do with absorption and volatility, those kind of things. Are you dealing with only one application rate? How will that be affected with different application methods?

DR. REISS: We have done tests for multiple application rates. We -- I believe the product, if it gets registered, we'll register a lower application rate than some of the studies that were done.

With methylbromide, they found a linearity between flux and application rate. We don't have enough variability in application rate to do that test. So we're relying on that, the analogy between methylbromide and just general physicochemical considerations -- think that that's linear.

The range we're looking at right now is not that large. The lowest application rates may be in the 125 pounds per acre range and the maximum might be about 175 pounds per acre.

DR. WINEGAR: So you don't think the lower volatility of iodomethane is going to be an issue with these flux estimates in the calibration of the indirect flux that goes into the model?

DR. REISS: I can't think of a reason why they should violate that linearity assumption.

DR. WINEGAR: Last, you have given several plots of the flux variation over time. Some of those look pretty smooth. There have been some discussion about the diurnal variation with the peaks, up and downs. But others look pretty smooth. Can you comment on why some are smooth and some do exhibit that diurnal?

DR. REISS: I think most of them saw some diurnal variability. You are dealing with a lot of experimental variability, particularly, when you get out to the three, four, five seven days.

I think there is just some experimental variability that you would expect there. But we have seen a relatively consistent diurnal profile. But it is not on all days. I assure you that's a diurnal variability or some other process that we can't explain.

DR. WINEGAR: Have you tried plotting your hourly model output compared to these -- I know you are dealing mostly with the 24 hour and that's what the toxicology people like. But just curious about how your model predicts on an hour to hour basis versus these experimental curves.

DR. REISS: The model is essentially calibrated to those experimental curves. So it predicts it quite well.

DR. WINEGAR: Thank you.

DR. ROBERTS: Thank you. Dr. Yates.

DR. YATES: First, just a follow up on something that was said before. I think that there probably are some background, I mean, some natural sources of methyl iodide. For example, brassica will produce methyl iodide if there is iodide in the soil.

And I also think that in marine environments I think it is possible to produce, well, all methyl halides, actually. I don't know that that would really have much of an effect on flux measurements, especially if you are pretty far from marine environments.

I think it is a potential source in some places. It might be something to kind of keep in mind.

The next thing has to do with your figure of 59, I guess, the ambient temperature. It seems to me that -- you show a plot here for the average temperature versus the flux. And I was wondering if you could comment on whether you think it is -- that this temperature dependent should be a factor when you are looking at application methods.

I could see where maybe temperature dependence for, say, flat fume, where you have tarp, since we know that the tarp permeability is strongly affected by temperature, I could see where you would see some kind of temperature dependence.

But when you are starting to compare flat fume to drip, would you expect to see the same kind of dependence?

DR. REISS: I think we have too little data to really know that. For example, with raised bed, I think the range was 56 to 61 percent between the three studies we did. That's just not enough range to start to look at temperature variations.

There is a little bit more range between the other two methods, but there is only two studies. Is that what you are asking? Is there a temperature dependence if you look at just one application method?

DR. YATES: Right.

DR. REISS: I don't think we have enough data to really look at that.

DR. YATES: Also, you talked about having six or seven studies. It's my understanding that if you were to use this model to develop buffer zones, you would probably pick the two studies that go with the flat fume if you are trying to predict a buffer zone for a flat fume.

And you would use the three for the raised beds if you were looking at a raised bed study. Right? So really, you would have two, three and two studies available. Right?

DR. REISS: I think we may have three, three and three by the time it is all done.

DR. YATES: But you are taking the appropriate emission --

DR. REISS: Yes. We will. Like methylbromide, the buffer zones are a function of the application method. So it is -- we haven't worked out how that will happen, but that's quite a good possibility.

DR. YATES: Right. So it will become very difficult in a way, then, to use this methodology for new techniques, say, to reduce emissions, because you would have to go out and run a number of studies, maybe like --

Given that two or three is not enough to capture maybe all the variability, you would have to run, say, 10 studies, maybe, for -- say that you were going to look at a virtually permeable film or if you were going to look at some surface amendment to act as a reactive barrier or something of that form.

DR. REISS: If you had a new method and you wanted to use the model, you would have to have some basis to know what the flux rate was, of course.

I don't know that you would need 10. It would depend if you did one or two or three and then look at the variability that you have before you decide how many you are actually going to need.

DR. YATES: Well, the other alternative, I'm sure has its own problems, but would be to look at some kind of a soil based emission model where you are no longer trying to back calculate the flux, but you actually have a model which captures the processes that you think are important.

And there is actually a scientist, ARS scientist, in Saint Paul, Minnesota, that has come up with a boundary condition that couples what is occurring in the soil to what is occurring in the atmosphere. He has atmospheric stability terms and resistance terms in the near surface.

And the data that you get from the weather stations could give you the atmospheric parameters that would go into this boundary condition. And the soil information, a lot of it could be determined empirically without having to go out and collect experimental data.

I mean, I don't want to make it sound like regression type empiricism. The only real difficult parameter to obtain might be soil degradation. But I think there are probably ways to correlate that with organic material or other soil processes. And then you would be able to simulate the flux into the atmosphere.

If you had a new application method or a new emission control method you would be able to pretty much do it without having to run a lot of experiments, which we all know are expensive and time consuming.

DR. REISS: That would be desirable. If you had a way you could couple the meteorology and the flux rates, that would be great, because you could incorporate that into the model.

Obviously, from a regulatory standpoint, people are worried about uncertainty. We have spent a lot of our discussion today talking about uncertainty. And the question would be, with an analytical model like that, what would the uncertainty be and would it be greater than the measurement uncertainty that you have out here.

So if something comes on line like that that could be used and had a reasonable low uncertainty, then I think, yes, that would be great.

DR. ROBERTS: Let's take a break for lunch.

People probably need an hour or so. Let's try and get back together here. Let's try and start at 1:45 sharp.

(Thereupon, a luncheon recess was taken.)

DR. ROBERTS: Let's begin with Dr. Majewski who had some questions.

DR. MAJEWSKI: I'm still trying to get a handle on how you calculate the actual fluxes from the field data.

In the field data or the field experiments, you have three different methods, three application depths, at least three different tarp thicknesses and types, four hour and 12 hour measurement periods, and you are taking these data and generating hourly flux values --

DR. REISS: Period flux values. By period. By whatever the measurement period in the study was. So if it was four hours or 12 hours, we just have a flux estimate for that period.

It is expressed as an hourly value, but it is the same for all the hours of that period.

DR. MAJEWSKI: My understanding from reading the background data is that the modeled flux values are based only on the field flux data.

DR. REISS: That's right.

DR. MAJEWSKI: And that you give a higher weight to the early morning hours when you expect the highest flux values and then it decreases throughout the day.

But the field flux studies were done different times during the day. And as you showed, you had maximum fluxes occurring throughout the day depending on when they started the application. And it's just unclear to me how -- I'm no modeler, bear with me, but it's just unclear to me how you can have confidence in the modeled data or modeled results from all those variables in the field experiments.

DR. REISS: Well, let me try to answer that.

Regardless of the start time of the application, we saw a relatively similar amount of material coming off the field. So the actual amount of material didn't seem to be affected significantly by the start time.

Now, what was affected was this so-called diurnal profile. At what time of day did those emissions come off. And what you would find is in a study where the flux occurred, more of the flux occurred during the evening or nighttime period, you would have higher concentrations than you would for a study where the application was earlier in the day.

And the model I think successfully accounts for that phenomenon. By modeling explicitly the diurnal profile, the model can use a different flux value for every hour. So it explicitly accounts for that diurnal profile in the model to, I think, successfully simulate what actually was observed in those field studies.

Now, how it gets regulated in terms of choosing a buffer zone when you have these different buffer zone estimates depending on the start time of the application, that's another issue. And I don't presume to have that answered yet.

But that will partly be a policy and partly be a science decision that we have to choose. To the extent that you can be assured that if you set a permit condition that the application occurs by a certain hour, you would have to be assured that you could enforce that and that's a reasonable condition.

But the model can -- all I put forth is that the model can account for those sorts of variabilities.

DR. MAJEWSKI: Another thing I'm having troubles with is the -- I have done field studies. And I know that the meteorology on the field can be very different from the meteorology at these CIMIS stations, because you are doing a field scale study and you have all your micrometeorology or micrometeorological parameters going into effect.

It just seems to me taking wind speed at 10 meters and comparing it to what is happening down close to the surface is -- plus, the data you are using in the model is many kilometers away in most cases, there just seems to be a disconnect there.

DR. REISS: Well, it's a common challenge in dispersion modeling. You really have on site data for the particular application you are looking at. But I think we have dealt with that by -- we analyzed the total of 15 different stations and looked at the variability and the results you got from those different stations and chose stations that represented that overall variability.

Of course, if someone was concerned about it, they could look at more data if they like. The model isn't specific to these four stations that we have chosen.

So I think that -- I think that we do have a good handle on what the variation of meteorology could be out in the growing areas. Now, that's not to say that for a particular application and particular location that CIMIS station X or Y is going to be the best station to represent that.

But we're not trying to represent a particular application. We're trying to characterize the variability you would observe across all the applications that are occurring, because we're going to have to set these buffer zones from a regulatory standpoint prior to any of the applications occurring.

And let me just also say about the two and 10 meter data. The CIMIS data is at two meters. The NWS data, six meters. The other data, ASOS and FAWN are at 10 meters.

Typically, for dispersion modeling, people have preferred six to 10 meter data because it is more largely representative of a greater surrounding area.

Now, but we did model with the two meter data as well. We included two meter data in the analysis. And we also in two of the field studies we conducted we included measurements at both meter and 10 meter height.

I have some graphs. I don't know if it's necessary to show them, but we calculated the flux estimates with both meteorological measurements made at two and 10 meters. And they are virtually identical. They are very similar estimates. So I think we have accounted for that as well.

DR. MAJEWSKI: One last question.

Correct me if I'm wrong, but you did one comparison between the aerodynamic and the indirect method. And DPR has done how many?

DR. BARRY: We have done one. The Ross study that was published.

DR. MAJEWSKI: So that's two.

DR. REISS: That's right.

DR. MAJEWSKI: You are putting an awful lot of faith in two studies to say that this back calculation is the way to go.

I mean, I know these studies are expensive and time consuming. But it just seems to me that you are putting a lot of faith in these comparisons saying that the aerodynamic method is what you are comparing things to and using that as the starting point and you have got basically two data points. It seems a little sparse.

DR. REISS: We're not using the aerodynamic method as a gold standard. It has its uncertainties as well. It was an independent measurement, is what it is, is what I would describe it. So it was reassuring, albeit, from one data point that we got the same result using two independent measurements.

Another advantage of using the indirect flux method for this type of analysis is we essentially calibrate our model to the flux estimates that we derive.

Another point I would make is this product, if it gets registered and continues to be used, more data will be collected to further characterize these sorts of issues.

DR. ROBERTS: Dr. Portier, then Dr. Hanna, then Dr. Yates.

DR. PORTIER: Ken Portier here. On slide 22, we were talking over here trying to figure out. I think you have a mistake on the equation here. Probably, the flux, what is it, flux measurement, the second one doesn't belong in there. Right?

DR. REISS: You are right. It should be the coefficient of variance.

DR. PORTIER: The question is the standard error there, is that the standard error of the flux adjusted estimate or is that the standard error of the M coefficient from your regression?

DR. REISS: It is the -- I calculated a coefficient of variance based on the M coefficient. So it is -- when I apply it in the model, it is a standard error of that adjusted flux estimate, yes.

So if the flux estimate was 100 and the standard error -- this coefficient variation was 20 percent, then that would be 80 plus or minus, or 80 to 120.

DR. PORTIER: I guess they are both the same because you are just using this (inaudible) relationship. So we were just trying to figure out if that was the correct --

DR. REISS: You are right.

DR. PORTIER: It should be the predicted -- it should be the standard error associated with the flux adjusted prediction.

DR. REISS: It is that. I converted everything to coefficients of variance just for that reason. So I kind of nondimensionalized everything so I could apply it in the model.

DR. PORTIER: So if we could go to slide 48. If you take that equation, what you are essentially saying is that, in your simulations, at each stage, when you are simulating, because you really are simulating it to stage, because that's all you have data for --

DR. REISS: That's correct.

DR. PORTIER: -- you have a uncertainty on each one of these points.

DR. REISS: That's correct.

DR. PORTIER: So on simulating a day, you are going to go through and select at random some value for each of these dates. Right? You are going to simulate a flux for each stage.

DR. REISS: For each period, yes.

DR. PORTIER: Each period. Now, it is possible, unlikely, but possible, that you can get high values for each one of these for that whole day which would produce fairly high percentage flux rates for the whole day, which means you could get way over 100 percent at the end of a period of time.

So there is no constraint on this that if it is high flux at one point it has to be low flux at some other point so that the total mass of gassing for a day is somehow fixed. Correct?

DR. REISS: I don't have a mass constraint on it right at the moment. I have checked it. It is my observation that for these particular data it doesn't. It is not a problem.

But it probably would be worthwhile just to generalize the model to put a mass constraint so you don't emit more than 100 percent of the mass.

DR. PORTIER: One of the saving graces here is that you stopped the whole thing at 24 hours. So there should be left over mass.

DR. REISS: Right.

DR. PORTIER: You are just not worried about how much I left over for tomorrow because tomorrow never comes.

DR. REISS: It would be a lot more complicated if you had to extend it plus 24 hours to do that mass conservation. That's correct.

DR. PORTIER: The last question is you mentioned something about detection limits in the data, the real data, the measured data. How was that handled in the regressions?

DR. REISS: I used zero values. I also did a sensitivity analysis. Because the detection limit is on the order of 10,000 fold less than the highest measured values, it makes no difference in the regression.

If you did a log regression, you would have to choose some detection limit to do that regression. And the answer would be heavily dependent on what you chose.

But for the linear regression it is not a factor.

DR. PORTIER: A lot of the regression is going to be heavily dependent on those zeros as well. Because if I remember what you said, for every simulation, about half of the values are zero because they are upwind of the plume. Right?

DR. REISS: That's correct.

DR. PORTIER: So they are really anchoring that regression for every one of your scenarios.

DR. REISS: And they correctly anchor it through zero zero. That's what you would expect. It may reduce the variability, which is something I'm concerned about, you know, just having four pairs of zeros, which is one possible idea is to only do the regression with the larger values. You get a little bit higher standard error that way. And it may be a standard error that is more representative of the larger concentrations.

DR. PORTIER: It doesn't look like it would change the regression all that much.

DR. REISS: No, it doesn't.

DR. PORTIER: But it really makes it bothersome when you do a regular regression and the intercept term is still significant.

The fact that you have so many zeros, the regression wants to go to zero. And then occasionally you find the intercept term nonzero, that makes me worry that maybe there is nonlinearities going on in here that are not being picked up or lack of fit of the ISC model to the data that you actually measured in the field.

DR. REISS: It is hard to say. I have not seen nonlinearities. I have looked at a lot of these plots. They certainly look linear. You do occasionally get a positive intercept. Occasionally get a negative intercept.

I think part of the reason is that -- the ISC model, it is known to predict well what the maximum concentration might be, but it doesn't necessarily predict the location of that concentration as well.

So I think that's part of the variability you see in these results. It may be why that intercept term is sometimes statistically significant.

I don't think it is often really large, I mean, compared to constraining it through zero. Typically, when we constrain the regression through zero, it might increase by -- it usually goes up, but it is maybe another 5 or 10 percent.

DR. PORTIER: We would like you to explain again what you meant by ordering. You had the two regressions on --

DR. REISS: Sorting.

DR. PORTIER: Yes, the sorting.

DR. REISS: With the DPR method, if you don't get a satisfied fit with just doing it, your first linear regression, what you do is you just take the data from highest to lowest independently on the X and the Y axis. Just order them independently from highest to lowest and then refit the ordered data.

And the theory behind that is, again, what I just talked about a moment ago, is that the model predicts the maximum concentration quite well, but it may not predict the location of that concentration as well.

So removing that spatial component is one idea that you could use to get a better fit.

DR. PORTIER: That's kind of equivalent to shuffling the locations. I mean, you have the fitted model and you are shuffling the concentrations at the different locations until you get the ones that seem to fit the best.

DR. REISS: Yes. And it is generally how the people validate the model. I mean, they are looking to see that the model predicts those maximum concentrations well, but not necessarily the locations of those concentrations, which is a little too much to ask from the model.

DR. PORTIER: So you assign the maximum observed location to the maximum model fit.

DR. REISS: Maximum measure -- that's correct.

DR. PORTIER: So it is no wonder that the R square goes up.

DR. REISS: Yes. It is no wonder that the R square goes up. That's correct.

DR. ROBERTS: Dr. Hanna.

DR. HANNA: My question to Dr. Reiss is again related to the uncertainty. Suppose we have a perfect input, flux, emissions, everything. We still expect to find uncertainty in the result, either the concentration or the buffer zone or the exposure, everything, as the result of the different methodologies and schemes being employed in the ICSCT model and the consequent models and so on.

My question is, the methodology you are proposing, does it address this in particular? For example, the horizontal dispersion and the vertical dispersion are being calculated in the ISCST. So unless we have measure of the uncertainty or an estimate of the uncertainty in this parameter, which can be found, and maybe you can do Monte Carlo, so we will not be able to get an estimate of the resulting uncertainty into the concentration.

So really my question is really have you looked at uncertainty within the model parameterization or algorithms used?

DR. REISS: I haven't tried to modify the model in any way like that. I mean, if uncertainty bounds could be developed on the dispersion coefficients, that would be one way I could easily incorporate that into the model, since I have the code incorporated into the model.

And that might be an interesting idea, to put that into -- to incorporate that as a Monte Carlo element into the model. And that might get at some of the uncertainty in dispersion coefficients.

We are calculating for five years of data for over 1800 values. So you are getting some of the uncertainty that way. But yeah, I think that's a reasonable idea.

DR. HANNA: There are a number of studies actually that can give some estimate of the uncertainty in the dispersion coefficients or parameters like the sigma Z and Y in the ISCST compared to different kind of observations or measurement or so.

DR. REISS: Thank you.

DR. ROBERTS: Dr. Yates.

DR. YATES: I'm referring to figure 18. If I understand this correctly, if you are on the red line, then you are -- basically, the red line is a stable result. It is after you do all your simulations, this is the line where you are outside -- if you are outside that line, you are outside the exposure area that's been set?

DR. REISS: That's correct.

DR. YATES: I guess I know one question for people who fumigate, like farmers who fumigate their fields, is that the size of the buffer zone kind of impacts them in terms of what kind of profit they can make on their farms.

This kind of suggests that maybe there doesn't need to be a very large buffer zone on one side of the field and maybe down in that corner -- so this technology in principle would be able to identify a noncircular buffer zone?

DR. REISS: Yes, it could. And it does, essentially, for every day in the five year historical data set. The question would be really a risk mitigation issue. If you were confident that the predominant wind direction was in a particular direction for a particular site, then I think you could justify having buffer zones that were different in different directions.

The problem you are looking at -- you are running the model before the application. You don't know the meteorological conditions. I mean, you could forecast to some extent, but you don't know for sure what the meteorological conditions are going to be during the application. That's one of the challenges in doing that sort of analysis.

DR. YATES: Clearly, you would have to have some base buffer zone like 50 feet or 100 feet or something like that. But I mean, I guess if the five years worth of data is sufficient to capture the variability in the meteorological data, then this should indicate -- if a 95 percent risk threshold or whatever you said is adequate, then theoretically -- I mean, in a sense, you are overly safe over on this side.

DR. REISS: Absolutely. If you had a particular site that you were interested in and you were willing to set a buffer zone for that particular site, then you could have a situation where you could essentially calculate the 95th percentile in each direction around the field and have this sort of oblong buffer zone.

It would just be a question of whether that's a feasible risk management strategy.

DR. YATES: I don't have much experience with the micrometerological or the meteorological models. Do you think that there is sufficient safety in all this that

that's possible, I mean, from your expertise?

DR. REISS: It is hard to say universally true. There could be a lot of micrometerological factors that can affect particularly the wind direction in a particular area.

You would really have to evaluate that on a case-by-case basis to know that you really have a handle on what the predominant wind direction is at a particular location.

DR. ROBERTS: I think Mr. Dawson, do you want to respond to that question as well?

MR. DAWSON: I was just going to say that one of the things that we need to be, we, as the agency, need to be cognizant of as we go through this process is to get a clear understanding of exactly what this means, what it represents, and I guess ultimately how we use that and some kind of risk mitigation action or whatever you want to call it.

There is going to be a lot more process around that. So I just want to make clear that we have come to no sort of decision about how we might end up ultimately using these results, especially when you consider we're looking at things like national level labelling and how the implementation and all the other issues that go with that.

That's something we need to think about as we go through. We're still definitely grappling with that.

DR. YATES: I wasn't trying to put anybody on the spot. I'm just curious, because it does sort of make sense that if you feel that the meteorological data, if you feel that you know it well enough, and you see certain patterns, that a buffer zone should reflect those patterns at least in some way. But the other side of it is as someone who might be sitting near that buffer zone, I might have a whole different idea about it than if I'm quite a far distance away.

Anyway, on to another question. In some of the data I have seen when you look at comparing the model results, this would be in trying to fit the flux, there will be places where the model has a lot of zero values but sometimes you will get a measurement that's not zero. I was wondering if you have any thoughts on what might cause that.

DR. REISS: The model, the ISC model doesn't account for diffusion from like a concentration gradient. It is only basically moving the gas in the direction of the wind.

Perhaps it is possible that you have some diffusive transport that would result in low concentrations in an upwind direction. That's my only possible guess as to what is going on there.

DR. YATES: Do you think with the meteorological information, you know, that you get a mean wind direction and then there is also like a variance or something on it, is it possible that there might be some -- for a short period of time there might be a drastically different wind direction that somehow -- when you look at the mean direction of the variance, you don't get that component going in a different direction?

DR. REISS: That's certainly possible. Particularly, when you have light winds, you have the so-called light and variable winds. You have quite a lot of meteorological variation.

The dispersion coefficients are designed to account for some of that variation by just using one wind direction for the hour. It disperses it at some angle around that to account for that variation you would get on a sub hourly basis. But I think you are right, especially with calm winds.

When you have calm winds, it is very stable and you can have concentrations built up. You have a highly variable wind direction. In fact, there is some data to show that the direction is even more variable than you are actually measuring just because you are below the sensor threshold.

DR. ROBERTS: Any other questions, Dr. Ou.

DR. OU: One of your slides indicated the future of methyl iodide for that rate including chloropicrin, as much as 75 percent.

If chloropicrin is included, do you think the buffer zone needs to be adjusted to include in the chloropicrin?

DR. REISS: Yes. What is likely to happen, I can't again speak for how EPA will ultimately regulate it, but one possibility would be that there would be sort of a rate limiting buffer zone or you would calculate the buffer zone for both materials and you would probably use the larger buffer zone of the two.

I don't know specifically, the toxicologist will have to answer it, but I don't know if there is any synergistic or additive effects between the two. They have relatively different toxicology.

DR. OU: Also, that the methyl iodide is much, much more volatile than the chloropicrin. Chloropicrin is like water. If they are mixed together and applied to soil, I assume the methylbromide (inaudible) would be (inaudible) by the chloropicrin.

May need to adjust the flux rate, including the chloropicrin.

DR. REISS: You think the mixture of chloropicrin that you add could affect the mass that's emitted?

DR. OU: Yes.

DR. REISS: It is not an issue I have looked into.

DR. OU: You add much as 75 percent of chloropicrin. You are (inaudible) going outside the flux rate of the methylbromide -- not methylbromide, methyl iodide -- methylbromide as well.

DR. REISS: We are accounting for the mass. But we calculate the flux rate based on the mass of the material that's in the actual formulation, of course. But whether there is an actual impact on the flux rate, depending on the chloropicrin you add, that's something we may need to look into.

DR. ROBERTS: Dr. Wang.

DR. WANG: In the margin of exposure assessment, you have like a sub model, a subroutine.

Would phytotoxicity being considered in there or just human exposure? Because in some locations there may be other plants across growing that can be a big concern.

DR. REISS: The model is purely to estimate the air concentrations. So yes, it is human exposure. If there are other plants in the area -- I mean, the model generates concentrations. And also, when you calculate the margin of exposure, you give it right now a value a toxicity threshold that is applicable to iodomethane.

I haven't considered it, but I suppose if you had an equivalent value for a nearby plant, there would be no reason why you couldn't use it for that purpose as well.

DR. ROBERTS: If there are no other questions from the panel, I think we need to move on with the public comments.

Let me take this juncture to thank Dr. Reiss, not only for his presentation, but for his willingness to engage in the panel in answering our many questions. It has really been very helpful for us to understand the model and the case study. I think that will be very useful as we discuss it later on.

Let's now move to public comments. The SAP always welcomes comments from the public on the issues that we are addressing. We have two people that have indicated an interest in addressing the panel.

And the first person on the list is Ms. Shelley Davis from the Farmworker Justice Fund.

Welcome. For the record could you identify yourself, please.

MS. DAVIS: Good afternoon. My name is Shelley Davis, and I'm the co-executive director of the Farmworker Justice Fund. And I submitted written comments this morning on behalf of the Farmworker Justice Fund and the California Rural Legal Assistance Foundation.

I hope you all have a chance to read the complete written comments. They are not very extensive. I'm just going to briefly highlight a few points that we made more in greater detail in the written document.

From our perspective, it is important to analyze this model to determine whether it would yield adequate buffer zones to protect all bystanders in the area, especially in the area of maximum concentration.

And to that end, we have a number of concerns. One of our key concerns, really, is when a model, as this one does, calculates -- uses as a key assumption the idea that concentration can be equal in all directions.

We're concerned that the number of low concentrations will bring down the 95th percentile and so it would not be as protective as a model that looked at the maximum concentration area.

What we're also concerned, that 95th percentile is just not enough with all due respect to the idea that the curve is not that steep. There is a significant difference between 95th percentile and 99.9.

And in other areas, for example in the dietary exposure area, EPA does look at 99.9. And that is -- we would want you to evaluate the adequacy of this model at that level of exposure.

A couple other points that I want to highlight. In evaluating this model, the sort of key variables include weather, the flux rate and the consideration of how conservative it is. So we have a few comments in each of these areas.

Starting with weather. I think in general the idea of using actual weather data sounds like a good one. But we're concerned that the way that the weather data has been used is not sufficiently protective.

First of all, on the quality of the data. The National Weather Service data is acknowledged to be of far greater quality than the other data sets that are used.

And so one of our concerns is the use of data other than the National Weather Service.

Another key concern, really, is related to the necessity of focusing on worst case weather conditions, which we have a good handle on from California experience, because in the last five years or so, there have been a number of mass poisonings in California due to fumigants.

And one key component appears to have been worse case weather, which is generally high stability and low wind speed. And so one of the concerns we have with the weather data that was used is how they treat calm hours since calm weather appears to be a key component in actual poisoning incidents.

And with the ISC model that's incorporated, calm hours are not included. And that too has been cited in the past by California agencies as underestimating the concentrations.

So the whole effect of calm hours is something we also would like you to look at and have a great concern about.

Just in sort of the practical of it, there was a comparison provided of Bakersfield data, which was not National Weather Service data, and buffer zones produced from Fresno data, which was the National Weather Service data. And the Fresno data produced a far larger buffer zones.

A comment on the issue of flux rate. We commend the registrant for doing a number of studies. But we're concerned that again they didn't maybe focus on the worst case scenario.

And most of the flux studies that were done were done in coastal areas where the temperatures are more moderate. Of the two that were done in the Central Valley where it's hotter, they weren't done in the hottest time of the year. For example, the Manteca study was done in September.

So we're concerned that they don't reflect the flux when it is hottest. And this has been mentioned also -- is a time when frequently many of these fumigations actually occur, during the warm months. So that appears to be another limitation.

Now I guess I want to make a few comments about how conservative or not the model is and what it produces. There was the idea, the concept that this model produces conservative results because it is not all that likely that someone would be at the perimeter of the buffer zone for 24 hours.

And I guess this is another area where I feel like the real world provides a different view. The fact is that people who live at the edge of the buffer zones could easily be 24 hours in that area. Especially, the very young, the very old, and the disabled who would tend to be at home 24 hours a day. And in the summer months, school children also may well be home 24 hours a day.

There was another suggestion that it is protective because the indoor air is different from the outdoor air and less concentration inside.

We cite a study which shows actually that the indoor and outdoor air levels are quite comparable.

But again, focusing on the people who are likely to be hurt in such an event, low income, rural residents frequently people leave their windows open as a form of insulation, especially in the warm months.

So the likelihood is that they would have the same exposure indoor or outdoor over a 24 hour period. So we don't view that notion as at all a conservative assumption.

Another point that actually got raised in the end of the discussion, we also would like to raise in our discussion, is the idea that the model needs to take better account of multiple applications and multiple chemicals.

In rural areas, it is very common for fields to be quite close together, even if owned by different parties. And so a distance apart of 50 feet or 100 feet is not at all uncommon. So the possibility of multiple applications is not at all a rare event.

The other thing is in this model where you are looking at a 24 hour time period for concentration, it is quite possible that over the course of that time period the wind will come from one direction for some of the hours and another for another of the hours to get you a concentration from fields even if they are not contiguous.

So we think that has to be taken a whole lot more seriously than it appears to have been. And similarly, multiple chemicals, and as one of the panelists mentioned, that iodomethane itself is being combined with chloropicrin, so the combined effects of these different chemicals would need to be taken into account.

And just another point on the time of the day, because that appeared to have been a variable that had a big effect on the buffer zone, in the practical, in California, frequently these applications are made in the evening when the temperatures are cooler. And so you could much more easily be in this worst case weather scenario of high stability and low wind.

So these are a number of our concerns. And we hope that you will carefully consider our comments. Thank you very much.

DR. ROBERTS: Thank you, Ms. Davis. I would like to give the panel the opportunity to ask any questions about your comments if they them. Let me see if anyone has any questions. I don't see any. Thank you very much for your comments. I appreciate it.

The second individual that has requested the opportunity to address the panel is Dr. James Platt from Arvesta. Welcome, Dr. Platt.

DR. PLATT: I'm James Platt, former project manager for iodomethane with Arvesta, now a consultant. And I'm here with some other Arvesta people today.

I just wanted to comment on a few things that seemed to be perhaps unanswered during the earlier discussion.

There seemed to be an interest in some of the chemistry of iodomethane that was not part of the presentation. So I thought I would just mention briefly that iodomethane is actually a liquid, boils at 42 degrees C, 108 (ph) degrees fahrenheit. As we mentioned, the vapor pressure is about 400 millimeters at ambient temperature. The specific gravity is 2.3. It is a very dense material. The water solubility is about one and a half percent.

One of the issues that we addressed early in the development of this chemical was its sensitivity to ultraviolet radiation. It breaks down very rapidly.

And part of the process here was to get a ruling from EPA on whether it was a threat to ozone, what its ozone depletion potential was. And that was stated it was developed at 0.0015 where 1.0 is the worst case that things are compared to.

Soil half lives in our soil dissipation studies were in the range of three to four days. So that's an important thing.

Just to be sure that -- perhaps I didn't understand the questions, I wanted to make a comment. In terms of our sampling for these studies, there were no data gaps. And I got the impression perhaps someone thought there was. The sampling started when the application was being done, and then when we got to midnight we switched to a 12 hour cycle.

So it is possible in the first day when we were doing three hour intervals, that that would take us to, say, 19 hours. And then the remaining piece had to be taken from the next segment.

So there were no data gaps. And we always got the front end where the highest potential flux rates were.

And the 100 percent evolution that was material from -- that was mentioned at Manteca, that was entirely consistent with the soil dissipation studies and with the laboratory studies on aerobic soil metabolism. That there was very little retention in soil and it rapidly evolves.

Another point was the fact that most of the sampling data, except for the direct flux, samples are taken at 1.5 meters. That's a regulatory requirement, that these sampling points represent the approximate breathing zone of typical workers.

So it wasn't part of a research program to evaluate vertical heights beyond what we did with modeling in the direct flux.

And then also from a regulatory standpoint, to be conservative, all of our studies were done at the maximum application rate. So studies were not done as a research program to look at flux versus application rate. But again, that was a regulatory requirement and that's where we focused our efforts.

Again, not only do they represent -- in terms of the areas that we picked from a regulatory standpoint, we picked the highest potential agricultural uses. We picked the typical growing seasons and the conditions where they are going to be grown.

So just to be clear on what we're doing and how we got to where we are today. If there are any questions, I would be happy to answer those.

DR. ROBERTS: Dr. Yates.

DR. YATES: Two questions, I guess. The first is I'm a little curious about -- you said that the soil half life was one and a half days.

MR. PLATT: No, I said the water solubility was 1.5 percent. Soil half life is in the three to four day range.

DR. YATES: Was that soil dissipation or is that the transformation of methyl iodide in soil?

MR. PLATT: Those were actual field data, so they were soil dissipation studies. It is a combination of degradation by multiple paths plus volatilization.

DR. YATES: Because some of the work that we have done shows that the soil half life as far as just transformation would be about double that of methylbromide.

Figure, for at least the soils that we work with, the soil degradation half life would be about 20 to 30 days where for methylbromide it is 10 to 15 to 20 days. So I just want to be clear. Because that's the lowest --

At first I thought you were saying that was the transformation half life and it seemed low. But for dissipation that's fine, yes.

The second question, do you have any kind of like, say, a transformation half life for photodegradation in the troposphere?

MR. PLATT: Actually, we do. It is 1.5 to four days. And that's the atmospheric lifetime. Not a half life. But that's the one that was -- that's not our data, but that's reported by the Montreal protocol related working people.

DR. YATES: Thank you.

DR. ROBERTS: Dr. Winegar, then Dr. Maxwell and then Dr. Ou.

DR. WINEGAR: My question is actually similar to the last one in regards to the chemistry of iodomethane in the soil. It is not clear to me. You say that the half life is two to three days for dissipation.

So that's basically the flux, is what you are talking about. Is that right or --

MR. PLATT: No, I'm talking about a soil dissipation study where we're measuring soil samples and we're measuring different depths, we're calculating disappearance over time in a field study.

In California and in Florida, we see a half life of three to four days.

DR. WINEGAR: I'm trying to understand what the mechanism of dissipation is that you are referring to.

MR. PLATT: In that case, it is a combination of everything that's going on, because we know that from the flux studies there is volatilization going on. We also know there is hydrolysis. We know there is microbial breakdown. So that's the net effect we got for those two sites.

DR. WINEGAR: What do you know in terms of the relative fraction of volatilization versus loss through biological transformation or hydrolysis or that kind of thing, non volatilization mechanisms of dissipation?

MR. PLATT: I don't have a lot of information on that with me right now. We have done the -- we have done laboratory studies on hydrolysis at various pHs and temperatures. But in terms of the numbers I can give you today are in the soil half life. Others I would have to respond in some other way.

DR. ROBERTS: Dr. Seiber, then Dr. Maxwell, then Dr. Ou.

DR. SEIBER: You said the chemical breaks down rapidly. You didn't say initially how fast that was. But I gather you mean it was 1.5, 4 days is the reported atmospheric half life.

MR. PLATT: Unless I said something other than what I had written down, I said the water solubility -- oh, was 1.5 percent. But I do have data here from the Montreal protocol studies that put the atmospheric lifetime, yes, at 1.5 to four days.

DR. SEIBER: One of the things ISCST fails to do is take into account either deposition or chemical breakdown in the atmosphere. I'm not sure that it is impossible to include it, but normally it is not included.

Have you looked at that?

DR. REISS: The model can take into account both of those factors. When you have, say, a life time of a day and a half, what you are actually looking at when you have these field studies or in a field situation, you are interested in the concentrations that are 500 to 1,000 feet from the field, it takes seconds to get there.

It is not that we're not accounting for it. It is just that it is a negligible factor with the small distance between the field and the receptor site we're interested in.

DR. SEIBER: Right. But again, that number we probably ought to find out more what condition it was run under. Because if it was high so ozone conditions or something different about that particular atmosphere around the field, those numbers tend to slide quite a bit, those half lives in the atmosphere depending on --

DR. REISS: Like I said, the half life would have to be on the order of minutes to matter. It would have to be as quick as a few minutes to make a difference in the model.

DR. SEIBER: I hear what you are saying. I kind of agree with you. But still I don't think we should just, you know, neglect it completely. Particularly, if your model is to be used with many fumigants.

DR. REISS: The model itself can accommodate that. It builds in the ISC, which has both the deposition rate and a first order of decay rate. So a user of PERFUM could add both of those variables into the model.

DR. ROBERTS: Dr. Maxwell.

DR. MAXWELL: Dave Maxwell, National Park Service.

Are there any criteria for applying the chemicals? For example, are there any circumstances when the iodomethane should not be applied? Any atmospheric conditions where you say we shouldn't apply this right now?

MR. PLATT: We're now putting together our labels. And in our draft labels, we have talked about conditions to avoid would be atmospheric conversion. And that's kind of standard language for fumigants. But I'm not aware that we have developed any specific conditions other than that.

We have talked about possible areas about minimum wind speeds or maximum, but we haven't developed them yet.

DR. ROBERTS: Dr. Ou.

DR. OU: I have one question. Does intense degradation occur for methyl iodide when you repeat apply the methyl iodide to soil?

MR. PLATT: I'm sorry. I don't think I quite understood your question.

DR. OU: For some fumigant, not -- some pesticide, including fumigant like one sodium, MITC, you apply soil one time or more than one time. Then degradation rate will increase. The degradation just amend (ph) the biological. My question is does methyl iodide can cause, enhance degradation when repeat applied to soil.

MR. PLATT: I'm sorry. That's outside my area of expertise.

DR. OU: Because if (ph) enhanced, can be very severe (ph). How that can be done to only few hours?

MR. PLATT: I'm not sure I have gotten the essence of the question. Maybe someone else could --

DR. ROBERTS: Dr. Wang.

DR. WANG: I think we completed a study recently on that topic. It is looking at the accelerated degradation of fumigants when a field has a history of fumigation with that particular chemical. Since you probably preferentially are selecting some of the microbes, will become more efficient, degrade that particular compound.

I think the question I believe he was trying to ask if for methyl iodide will there be microbes will also enhance the degradation if you repeatedly use that compound at the same location.

MR. PLATT: I'm certainly not aware of that. That seems to be outside the scope of the meeting today. But we could respond to that if there is someone who would like to follow up.

DR. ROBERTS: Dr. Wang.

DR. WANG: Appears the specific gravity is 2.3 and is applied in the soil. That means there might be gravity driven. I wonder if you monitored any soil gas concentrations during your experiments besides the error concentrations in the ambient.

MR. PLATT: No, we didn't. We measured what we could collect in soil samples, and then above ground monitoring. But we didn't do any internal gas monitoring.

DR. ROBERTS: Dr. Platt, I would like to thank you for stepping forward and adding your expertise in answering some questions that we had about the chemistry of this material and how it behaves in the environment. I appreciate that.

There were only two people that initially had expressed an interest in addressing the panel. But I would like to make that opportunity available to anyone else in the audience, would like to make comments to the panel. This would be the only opportunity to do so.

After the public comment session closes and we proceed in the deliberation of questions, there won't be another opportunity for public comment.

Last chance. If anyone in the audience wants to make a comment. I don't see any. Let's move on then to the questions.

MR. DAWSON: The background information presented to the SAP panel by the PERFUM developers provides both user guidance and a technical overview of the system. Please comment on the detail and clarity of this document.

Are the descriptions of the specific model components scientifically sound? Do the algorithms in the annotated code perform the functions as defined in this document?

Please discuss any difficulties encountered with respect to loading the software and evaluating the system, including the presented case study.

DR. ROBERTS: Question 1, which is really two or three questions, let's ask Dr. Spicer to lead off the discussion of this one.

DR. SPICER: I have briefly had a chance to review the user guidance document and the technical overview.

Just in general -- I take it I'm to respond at this point to the questions to my best ability?

DR. ROBERTS: Yes, if you would. Then we'll ask other discussants if they have anything to add to your comments. I will ask you to try and speak into the microphone.

DR. SPICER: The document, of course, we have received rather late in the process. But I did have a chance to review it. One of the things that it did seem like it was a preliminary document in the sense that there were additional studies that were alluded to in the document that were not included.

And I think that it's evident from the discussion today that you are still in the process of conducting field tests and those sorts of things, which is understandable.

As far as the detail and clarity of the document are concerned, there were some things that were clear to me, some things that were not. The things that were not were things such as the direct method of calculation for the flux.

That may be something that is more familiar to someone else in the field. But that was discussed, but it was not clear what specifically how that information was derived from the experimental programs.

To be honest, I have not had a chance to look at the algorithms in the code and to see whether they are correct. I did try and load the software, and I was able to get the software off the disk. But when I tried to run it according to the read me file on the CD, I was unable to do it with a few minutes' effort.

I might have been able to have done that with additional effort, but I simply was not able to do that.

There are specific questions that came up, but I don't know whether those are apropos to deal with at this point in time or not just in terms of -- like for example, the table on atmospheric stability does not mention difficulties that you have associated with the fact that the hour before sunrise and sunset and those sorts of things are automatically destability and detail such as that. But I don't know whether that's appropriate to be --

DR. ROBERTS: I think if there are some areas that were particularly unclear, if you could go ahead and sort of highlight those, that would be useful.

DR. SPICER: I think the other area that in general was unclear, and your presentation today did help in that regard, was this idea of estimating the flux calculations.

For example, in your overheads, there was one overhead that included where the sensors were located during the process of the flux calculation. But that was not included in the report, at least I don't believe it was. And so that process was clearer in the presentation today.

Those are the main comments that I have at this point.

DR. ROBERTS: Fine. That's great. Dr. Portier, let's see what you think.

DR. PORTIER: This is Ken Portier.

The first issue is descriptions of the specific model components being scientifically sound. By this, I understand that the description has to be clear enough that a user could replicate what was done.

If so, then I think the answer is yes, the descriptions are sound. The only problem encountered was with the technical documentation for the ISCST 3 model from the EPA web site that when I downloaded that data, I couldn't get some of the fonts to get my PDF file to come up.

It wasn't the PERFUM problem. It was the EPA side. Sorry about that. But I'm trying to be complete on this.

Some of the description discussed in the presentation by Dr. Reiss should be incorporated into the documentation. Specific sections that could use strengthening include description of flux rate estimation process with a clear description of the amount of data actually used in the process.

I think that compliments what Dr. Spicer was saying.

Second, we need a clear discussion of how those exceedance probabilities for each location on the grid are computed. In my discussion, I had to ask for that clarification. I think it is because it just wasn't clear enough in the documentation how those values were computed.

The second question deals with whether the algorithms in the annotated code performed the functions as defined. I think the answer there is yes. In fact, it is difficult to find something to say about this.

There are a few lines of codes where tabs are used to align the code with column 7. I look very carefully at this stuff. And this could cause problems with attempts to compile the code in other systems other than the Lahey compiler.

Finally, I should mention that the code actually looks like it uses Fortran 77 formatting conventions, not Fortran 95 or anything, Fortran 90.

As such, it is going to be quite inefficient as compared with modern coding standards. With minor effort, such as changing how the do loops (ph) are coded, it is quite possible to greatly improve the processing speed, I think, of this application.

I would not be surprised if you could increase it fivefold, simply bypassing it through an optimizing compiler. I attempted to do that. Of course, if you can do this, you can run more simulations. If you can run more simulations, we'll get a better understanding of what is going on.

Were there difficulties encountered in loading the software and evaluating the system presented. I didn't load the software and run it, but I did try to compile the fixed format fortran files found on the CD rom submitted to the committee.

I attempted to use the compact visual Fortran compiler, which is the one I have available to me. For the most part, the code compiled with the exception that the main program, the PERFUM.4 program, it uses a max and mod functions which are very slightly formatted different between the compact compiler and the Lahey compiler. It takes about 30 seconds to fix that.

Actually, this thing could be compiled in a different compiler. The nice thing about the compact compiler, it has the optimizer function. I could actually start looking at where you could optimize this code to really increase the processing speed.

The other thing about using a more recent compiler is that you could add a visual interface. You shouldn't have to open up a DOS command window to run the application. It probably should pop up its own little application window.

Again, that's an hour's worth of work for a programmer to create that little interface. And then I think that would make it easier for the users to use in the future.

Finally, one thing I really liked was your standard of using one item per line on the input file and allowing for descriptions in that first part of each of those lines. I think that's really great. It makes it easy to document the scenarios that that file is attempting to run.

And for those of you who have done model and attempted a lot of scenarios, it is very easy to lose track of what you are doing unless you can document the meta data that goes with the file. And the format that you have is real simple and I really like that.

DR. ROBERTS: Thank you, Dr. Portier.

Dr. Wang.

DR. WANG: Yes. The first question on the description of the specific model component scientifically sound, I will say yes, also. The overall approach I think (inaudible). It is a design of the framework integrating the ISC model into the performance. It's a logical approach to create a more probability assessment.

Algorithms in the code performance, again, that might be expedited by adding functions to that so you can select the duration that you can do the simulation rather than using all the possible days and scenarios. That way will be much more targeted and also the computation time will be much smaller. But overall, I will say it performs the function for what it's designed for.

As far as loading the program and evaluation, I work with many models and I will say this is acceptable. But for those who has no experience of working with computer codes and models, probably it is very difficult.

And it depends on your target, who is going to be using this. And it is really a question of prior experience before they can use this code. That prior experience will be needed.

DR. ROBERTS: Dr. Yates.

DR. YATES: I also tried to load the model. The MOE model for some reason I wasn't able to get running. I think -- I guess there is a control TXT file. Is that right? Yes. And it took me a while.

But it seemed like maybe there was a file name discrepancy and something down lower. I don't have it in front of me, so I can't tell what it is. But there was some file that it was looking for on input that had a different name than anything in the directory.

And then what I intended to do, although I didn't have time, was to later on go back change that and try running it. But I didn't have time doing that. So I assumed that once that was done it would work.

The other, the PERFUM model worked fine. I ran the simulation that was the test case. And it went through without any flaws. The input file was easy to read. I agree with Ken, what he was saying about the ease of reading and having all that information in the input file.

I didn't have to go to the user manual to try to understand what things were, which I thought was pretty nice. It would have been nice to have a user interface, a simple one at least. Going to DOS -- I used to like DOS, but I don't anymore, but it's a small thing.

One thing I thought for the documentation -- that you probably should provide a more complete description of the field sites. There wasn't very much information on like soil types, organic matter content, things that a soil scientist would find useful in trying to interpret the things that you observed at each of the field sites, average temperature.

If you knew the water content or at least, maybe not in numerical form, but some description of how the field was prepared that might give an indication of whether it was really dry or had some moisture to it.

I thought that if with methyl iodide photo degradation is a possibility, it might be worth running an example where you could show how you could include that. Not so much that it makes a difference for risk assessment in determining the buffer zones in this case, but just, if this model was used for something else, it might help someone to help see how to do that.

And then I thought that as far as a little more discussion about the causes or some of the uncertainty in all this could be included in the documentation. I thought you did a very good job this morning talking about it.

As a matter of fact, your presentation this morning helped quite a bit in answering some of the questions that I had from the documentation. So if you could kind of merge the two, I think you would have a really good document.

DR. ROBERTS: We have heard some suggestions for areas to increase clarity and perhaps increase the speed and functioning of the program.

Let me ask other members of the panel if they have anything to add, anything they want to weigh in on agreeing or disagreeing with? Let's start off with Dr. Seiber and then go to Dr. Baker.

DR. SEIBER: I thought it was a good description. I enjoyed reading it. It was brought up earlier this morning, and I think it is just more of a suggestion for the future. One of the first things you look for is references in the back to see if it had, in fact, any part of it been peer reviewed.

I understand from the comments of Dr. Reiss this morning that that is being thought about. I strongly encourage that that be continued.

Parts of the model, however, have been extensively peer reviewed, like the ISC component. That maybe could be brought up a little stronger. There were references in the back to that particular model. So that certainly strengthens the confidence.

I would only add to what Dr. Yates said, that in the description of the fields, I think the terrain maybe could be specified, not only the terrain, but surrounding trees or buildings or whatever, structures of one type or another,that might have been nearby, particularly in relationship to where the samples were taken.

Finally, just a general comment. We read this document. We begin to think that all fields are square. But, in fact, they come in all different sizes and shapes.

So I thought it might have been good to have included a description of what might happen with a truly irregular sized field of types that you might encounter in agriculture and how it might perform or be made to perform with that type of field.

DR. ROBERTS: Thank you. Dr. Baker then Winegar and then Dr. Hanna.

DR. BAKER: I wanted to follow up on comments made about the file names. I was able to load the software and run it. I forget where I encountered the name problems, but I ran PERFUM in the course mode and PERFUM MOE in the course mode.

I think it was PERFUM MOE that was looking for a file in Bakersfield AS, but I think it was named Bakersfield 95 or something like that.

DR. REISS: Yes, that was an error I made. I sent an e-mail I think on Friday. It may not have got out. I apologize for that inconsistency.

DR. BAKER: Following the warnings, though, I was able to find it relatively quickly.

Then I went and tried the find mode. I shouldn't have. For me, it took me I think roughly an hour per year. So it ran for five hours on the find mode.

I don't know if that's -- you were saying you got quicker times.

DR. REISS: Yes, it is going to depend on your system.

DR. BAKER: Then to follow up on comments -- several of the things you mentioned today were useful and should be included.

In particular, the update on how you perturb the flux. You got rid of the 2000 variables. So that needs to get incorporated. I'm not sure if I caught on, but there was updates like that that you had today that would be useful.

DR. ROBERTS: Thank you. Dr. Winegar?

DR. WINEGAR: I didn't get far enough in the loading process to try and run a test case. But I'm looking through the documentation in regards to the output and any kind of graphic type of capabilities it has.

Unless I missed something in there, but it will be useful, I think, for users to, if not incorporate it into the program, at least directions on how to incorporate the output into a graphical program so that it can be viewed visually.

Huge tables of numbers tend to lose their meaning. So some type of graphical output like you are suggesting would be useful.

DR. REISS: Can I comment on that?

DR. ROBERTS: Sure.

DR. REISS: I would love to do that. I think one of the first steps is getting clear that this is a valid scientific approach before that investment might be made to make a user friendly interface and some graphical components.

I would comment on the user population. It is less than a dozen, I would say. It may be less than that. So we're not talking about a large user population for this model. So that does enter into our thinking in terms of how user friendly and how much we want to invest in making it that way. But it is possible that more could be done.

DR. WINEGAR: At least some way so people know how to get it into surfer or some kind of things so you can see contour plots, that kind of thing.

DR. REISS: Sure.

DR. ROBERTS: Dr. Hanna?

DR. HANNA: I also like the document. I think it is well written. It is written to inform rather than through information results.

I agree that maybe if you include the extent of the uncertainty analysis as you presented today, that might be helpful.

Also, I guess my main comment is the ISCST3 model. Since this is a central component of the whole project, it might be worth even one flow diagram of the ISCST3 component, the I/Os.

And also mentioning how -- especially the ISCST with the calm wind condition. It puts the bound at one meter per second and uses, I guess, the wind direction from the previous hours or previous meteorological observation, I guess. Is that what's --

DR. REISS: That's what it does when you don't use the calms processor. If you use the calms processor, which is the regulatory default mode, it actually skips and it calculates the average for that 24 hour period. It doesn't ignore the calms.

DR. HANNA: Right. That might be good to include because the calm is an essential part of this. Thank you.

DR. REISS: It is, yes. Sure.

DR. ROBERTS: Any other comments from panel members on this question? Before we move on to the next question, let me ask the agency folks if the panel's responses or suggestions were clear?

MR. DAWSON: Yes, thank you.

DR. ROBERTS: Great. Let's go ahead and take -- since we got a little bit of a late start after lunch, let's go ahead and take question two. Then we'll go to a break.

MR. DAWSON: Question 2: In section 2.3: Development of the PERFUM Modeling System of the background document, a series of detailed individual processes and components included in PERFUM are presented.

The key processes include (1) incorporation of ISCST3 into PERFUM, (2) probabilistic treatment of flux rates; and (3) development of a receptor grid. Please comment on these proposed processes, the nature of the components included in PERFUM, and the data needed to generate an analysis using PERFUM.

Are there any other potential critical sources of data or methodologies that should be considered?

DR. ROBERTS: Dr. Majewski, could you lead off discussion by giving us your thoughts on this question?

DR. MAJEWSKI: Sure.

Not coming from a modeling background, I read the section and it made sense. All your arguments about incorporating the ISCST3 model into the PERFUM seemed to be the way to go in terms of simplifying the data processing and outlook time.

One question I had is that -- let's move on. With the different application methods changing, actually I think we discussed this earlier, but I have it written here, so I will ask it, the different sealing methods and the application methods seem to change with time.

How does that affect the output, the flux source term, and how does the model deal with that? And it appeared to me that it wouldn't have that big of an effect. Is that a correct assumption?

DR. REISS: Yes. We got relatively comparable results among the different application methods, I think. The model, I mean as we ran it for this case study, whatever you get for the flux rate for whatever the conditions for that study are, that's what goes into the model.

DR. MAJEWSKI: As far as the probabilistic --

DR. ROBERTS: Dr. Majewski, I'm sorry, could you speak up a little bit? I think the mics are not picking up very well.

DR. MAJEWSKI: As far as the probabilistic treatment of fluxes, your use of the standard error from the flux studies to estimate the uncertainty seems to be valid. Again, I'm not a statistician. So I have to defer to my colleagues to comment more in depth on that.

Then the receptor grid, I like the idea of being able to run the model in the course mode to get an idea of the outcome first and then run it in the 99.9 or 99 percentile to fine tune it. It seems appropriate.

The only -- my guess is that it is not a problem, but the only thing I noticed was, what Dr. Seiber mentioned, was that all the examples are for square plots. That's probably good for an introductory document, but I think maybe for the final thing you might want to put an odd shaped field.

In the data needed to generate an analysis using the PERFUM model, there are five studies done in California and one in Florida. Is this compound going to be used primarily in California or what is the distribution, the percentage?

DR. REISS: I don't know if I can give a percentage. But I think the predominant use is going to be in California and Florida would be the next highest usage area.

It is not currently used, so giving a percentage is hard to say. But I think those are going to be the predominant use areas. As I said earlier, if this product gets registered, Arvesta is committed to continuing to try to characterize some of this variability.

DR. MAJEWSKI: So then additional field studies are in the works if this --

DR. REISS: Yes. There are additional ones already being planned. And then there could be additional studies with different application methods and whatnot as, you know, if a registration is achieved.

DR. MAJEWSKI: Now I have a comment on the background concentrations. And you mentioned two things.

One, that right now there is no background concentration from the use of iodomethane, but there is possible other sources. But in either case, these concentrations would be relatively insignificant compared to what is coming off the fields.

In an area where iodomethane is used -- begins being used in an area, it is conceivable that this background concentration will increase with time, which also brings up the point of residues coming off previously treated fields or interfering concentrations from other fields. And I think that may need to be looked into a little more.

And one question I have is that this is a field based study looking at the emissions from a single field. And generally, the fumigants are used in a wide area, a large area.

So how are you going to distinguish the emissions from a single field and know that your boundary measurements are accurate or do they need to be adjusted due to influences of other fields and applications in the area?

DR. REISS: I'll try to answer that. I think, unlike methylbromide, it has a very short atmosphere half time. I think Jim just quoted it as one and a half to four days. So that is one mitigating factor in any kind of buildup of concentrations.

I suspect, and I haven't done an analysis, we can use -- not really PERFUM, but we can use the ISC model, I think, to look at this in a little more detail.

My suspicion is that background concentration is going to be pretty low compared to the toxicity threshold that we're worried about.

DR. MAJEWSKI: Moving on to other potential sources of data or methodologies to consider. It didn't seem that the CIMIS data locations were all that plentiful -- or the National Weather sites -- you had seven or eight of them?

DR. REISS: There are many CIMIS stations in California. I don't know the exact number, but there are dozens in California. But we chose --

DR. MAJEWSKI: In that figure you showed with the met station locations, they seemed to be almost in a semi circular form in the area.

DR. REISS: We chose four stations, the four stations that California DPR used for their methylbromide analysis. The data were already processed.

We could certainly look at other CIMIS data. There is a plethora of different stations that you could look at and also for the ASOS.

For National Weather Service, there is a very limited number of stations. There is maybe seven or eight in all of California and some of those aren't in the growing areas or anywhere near growing areas, like San Francisco Airport, which wouldn't make any sense to use.

And in Florida, the same, there is a limited number of National Weather Service stations. But there is a plethora of data out there. We have tried, you know -- it took a monumental effort to get 15 of these files created. It is quite an effort to process the data.

But we developed the software at this point to do so for all the different systems. So it is possible we could look at other stations, but I think we got a pretty good handle on the variability that is out there. But there are other data.

DR. MAJEWSKI: I was also thinking of -- the California Air Resources Board has monitoring sites all over the place. If you can tap into that data if you need it --

DR. REISS: They do. I think they tend to be mostly in urban areas. They are developed for ambient air pollution. But they may have some other rural sites.

DR. MAJEWSKI: Actually, that brings up another point. You ran the IST model in the rural mode where you're considering it flat terrain.

Yet, you showed a picture where there was a housing development right next to it. And presumably, these footprints or buffer zones are for people. So why aren't you using an urban --

DR. REISS: The urban -- generally -- say you have -- the buffer zone is at the perimeter of a housing development. The air is going to travel across a relatively flat surface before it gets there. And then it might get to a point where the dispersion is going to increase because of all that turbulence created by the houses.

So I think -- we have to choose one or the other in the model. The conservative choice is to use a rural mode. That's what we have done in this case. But it is a conservative element of the model.

DR. MAJEWSKI: Then as another potential source of data, this one would be for validating your model or testing it. I know the Pesticide Action Network Group has a drift catcher program where they are giving air samplers to citizens and they go out and take air samples at their homes or whatever.

That may be a potential source of getting downwind data or something like that.

DR. ROBERTS: Thank you. Dr. Baker is next. Before we go to Dr. Baker, let me just point out that we're in an acoustically challenging environment.

It is very difficult for them to adjust the gain on the microphone so that the people in the back can hear. I realize it is getting on in the afternoon and energy levels are starting to flag, but let me just exhort everyone around the table to try and speak loudly and forcefully into the microphone so that the folks in the audience can hear.

Dr. Baker?

DR. BAKER: Having used ISC from the DOS prompt several times, I know the -- the output from ISC is inflexible and it often doesn't satisfy the needs of the exposure community. And I've tried to put a bug in the ear of people on the AERMIC to consider that in AERMOD.

I would applaud -- for the first question, I would applaud use of ISCST3 within PERFUM in extracting the type of data. Slicing and dicing per hours and different types of output isn't that easy to obtain. And I think you did a good job.

I think the exposure community could be more well served if they were included up front in the development, but that might be water under the bridge.

The probabilistic treatment of the flux, I think it is appropriate. One issue that came up was can somebody choose an option of not perturbing the flux if they just wanted to study, say, the meteorological variability. Is there an option that you can choose?

DR. REISS: Yes. You just set the coefficients of variance to zero and it will run without perturbing the flux.

DR. BAKER: Right. Then, again, the documentation we had was for the -- with the Excel file information, and I need to look at the update. That's a good option to have.

Back to number one, I missed it, the ISC within PERFUM, I read some things about AERMOD. Did I read or I just thought I read that you did an ISC3 in an AERMOD model comparison for these area sources?

DR. REISS: I didn't do it, but it has been done by EPA recently as part of their evaluation for AERMOD.

DR. BAKER: The development of the receptor grid, I thought it was well explained and scientifically sound, performed in the GIS platform and then brought in.

I guess that if you move away from flat terrain, that would complicate the development of the receptor grid and the flag pole receptors. So it's not easy to incorporate generically, but there is EPA guidance on how to do that as long as that is suitably referenced.

You just mentioned in the last discussion that you had a meteorological preprocessor that allowed you to take information from the stations.

Referencing that, you say the user community is small, maybe smaller than five, but still you might want to make that available to the user community if they have other stations.

DR. REISS: Yes, it would be a goal. It is just it would have taken a little more effort and time I didn't have. But eventually, yes, if people are interested in using the FAWN or the ASOS data, we could possibly make that available.

DR. BAKER: I guess the emissions preprocessor's a little bit more complex. And it requires the field data and some decisionmaking. But again, as long as that's referenced, that these preprocessors are available and then the community knows about them.

DR. REISS: You really just need Microsoft Excel to apply that sort of method. The trick is the long field study you have to do.

DR. ROBERTS: Thank you. Dr. Bartlett?

DR. BARTLETT: I think a lot of this was taken care of earlier on this section on Number 2, probabilistic treatment of the flux rates, that the discussion we had earlier could be in this section and be much more useful.

It was a little confusing to me on the purpose and why of doing that. And I think what you did explain is that, if I don't have it wrong, is that when you have tried different random number generators, it didn't really make much difference in the actual results.

Now, with the indication that you gave to us of some of the graphs, though, where we have information where there is noise in it, which I assume some of this noise is coming from the air term that you are putting in, which is now I guess a Z as opposed to T, and I wasn't sure if that affected spatial as well when you had the illustration, I forget what slide it was, where you had the red.

DR. REISS: Is this the buffer lengths?

DR. BARTLETT: Yes. With the buffer -- is any of the spatial also affected by the air term?

DR. REISS: Yes, it would. Generally, the air term won't change the 50th percentile of the buffer length distribution, but it will change the upper percentiles.

So both in terms of just using higher flux rates and also possibly combining a higher flux rate with a more worst case meteorological situation, you just have a better chance of that when you have more higher flux rates.

DR. BARTLETT: And it might affect the orientation as well of the location?

DR. REISS: Yes, it could certainly affect the orientation as well.

DR. BARTLETT: What still puzzles me about it is to the -- a standard error a lot of times does some random process that we don't quite understand. But there are processes that we do understand. And that's somewhat within soil emission the different factors that we discussed earlier, like soil moisture and other factors about soil, soil temperature and other factors.

So it is not clear to me whether if including that in the model or in the testing or the evaluation of the model, whether it makes sense to do that.

DR. REISS: At this point with the data set we have and the analysis we have done, I don't see a way to incorporate temperature and soil moisture content.

We only have seven data points. If start to try to analyze all these potential variables like organic matter content, soil temperature, ambient temperature, et cetera, you really don't have enough data at this point to develop a model of that process.

If that's eventually possible or there are some -- you could combine that with some more phenomenologically correct model like Scott was referring to earlier, Dr. Yates, that would be great. It would be an improvement on the predictions. But right now I don't see that that's possible.

DR. BARTLETT: What is useful for me was the discussion of the methylbromide studies, because I'm less familiar with the volatiles, that a lot of these parameters that we're used to affecting other compounds may not be so.

And the fact that we had the discussion earlier on physical chemical properties from the audience, it would be very useful to have that information in the report so we can understand better in this section why you are choosing which processes you are choosing.

I very much like introducing probabilistic approaches, because that's closer to the real world even though it may not be at the right time and place the way we can do that.

One quick thing, I guess on the receptor grid, I guess it would be problematic with approach to do a traditional square grid, which people are much -- it is easy to produce a square grid.

But I was wondering -- then you will have distances that are varying distances in your receptor grids. But with the interpolation that you are doing, maybe that would be more traditional as far as looking at dispersion in a squared grid and easier to generate on a

GIS for those of us who take a long time to do simple things in GIS.

DR. REISS: Actually, we have already run it this past week for an irregular sized grid.

There is no reason why the method can't be used. We just define -- in the GIS program, we can just define all those receptor points along the rings at the right distances and the model had no -- we didn't have to modify the model in any way to run it.

It is something we can look into more in the future. The impact of it would be if you had the wind going across the smaller distance, you would have a less of an impact. If you had the wind, the predominant wind going across the longer distance of the field, you might get a larger concentration.

It is a potential mitigation option even to at least to give advice to growers as to, you know, what sort of orientation to apply. But it is something we can account for in PERFUM, and we'll develop a few more of those receptor grids as we go forward.

DR. BARTLETT: It could be a nested grid too.

And then you might get some more efficiency in running it as well as closer endpoints, closer to, and a larger grid farther out. Then you might get some computational efficiencies.

DR. REISS: That is how it works, actually. The grid sizes are about 10 meters apart for the first one hundred meters then they start to get progressively --

DR. BARTLETT: I was thinking efficiencies in the sense in the corners you have high density that's redundant in a lot of ways. But it's not a significant point.

As far as the weather data, I think I put that off to when we talk -- that Dr. Majewski was -- to later when we talked about the other weather data.

So I guess -- the other thing in this section, it is a section you talk about seasons. And there is a bias that's introduced here that's not necessarily upward or downward. It really depends on the region.

That our experience in -- we have done a lot of emission modeling or working with emission modeling with ORTECH, which is another -- other chemicals, other pesticides have been modeled for emissions.

I don't know what is closest to the fumigants. There has been a lot of work recently also on lindane, but none of this is as volatile as what you are working with. I don't know if there are things that could be learned from that or mentioned with that.

But one thing we have noticed a lot is extreme variability between season, which also popped up, came out in your analysis by month. By doing yearly averages, it's good to know what kind of bias might be coming out of that.

The other thing, I guess, about probabilistic approaches is when we're talking about impacts and health is typical impacts on who happens to be where. And I think the comment from the audience was particularly important is, if you have worse conditions like an inversion or something like that, then the 99th percentile is important.

I guess this may go further ahead in a future section, but the maximum, there is a virtue of having -- focusing on not diminishing the importance of the maximum

24 hour length.

Because then in those conditions, if the farmer is aware of this when they are applying it, what kind of conditions that may result in these maximums and certain distances like a school that happens to be at the corner of the farm or something like that, then their probabilities have changed dramatically.

So I think as far as the other types of scenarios you have --

DR. REISS: In terms of what probability to choose for regulation, it is an issue I don't want to really comment on.

I will try to just deal with the scientific computation of those probabilities at this point. And, you know, how it is actually used to calculate buffer zones is really a policy decision that goes beyond just science as to how conservative to be.

DR. BARTLETT: I have one technical question, I guess, on crosswind. I believe you said you eliminate that in interpolation if that situation arises.

But sometimes in the real world that's what results in higher concentrations in certain areas. That's a real effect as well. I don't know -- if it's only within the interpolated space, it is not that meaningful.

DR. REISS: The problem -- a problem occurred because of the geometry of the calculation. You occasionally had -- for a given spoke, the concentration didn't decline for the first period or the first couple spokes. And it just caused -- basically, the interpolation algorithm fails if you do that.

So I just had to eliminate those points. I'm certainly not -- we're still going out beyond the field to correctly calculate the distance until it reaches the toxicity threshold. That was purely a matter of just making the interpolation algorithm work correctly.

DR. BARTLETT: In regional modeling we overlap and then combine. You get new centers of high concentration, which seems to be a real phenomena. I guess the last comment -- I guess I will stop there.

DR. ROBERTS: Dr. Hanna, do you have comments to add?

DR. HANNA: I wonder if we consider, especially regarding the integrity of the meteorological data, a possibility, if you want to, for a broader use of the PERFUM is to use meteorological data from another meteorological model.

I examined five or so areas really for areas where the data does not exist, for example, or very low quality as we have seen in some of this.

DR. REISS: I certainly haven't built it at this point to incorporate a wind field like MM5. Although I don't think the spatial resolution of a wind field from like MM5 would -- you would have the same answer for -- among the space you have for agricultural field, there wouldn't be any variation in those winds fields in MM5. I don't think the resolution would get down that far.

You can use any data that you can convert to an ISC format. If it was predictions from MM5, you would just need to convert that to ISC format.

DR. HANNA: Right. And MM5 can go to four kilometer in some of the nested applications. So that might be close enough to represent the meteorological conditions in certain areas.

DR. REISS: Sure.

DR. ROBERTS: Thank you. Dr. Wang?

DR. WANG: In the report, you said that you used the five year meteorological data. And you used the five years for a different reason than the EPA's Office of Air using five year for permitting purposes. But you mostly tried to provide a more probabilistic approach.

So if the reason is different, than why use a five year? If you use a 10 year or 30 year, likely the means of variation will be different. You go back to history.

DR. REISS: I used five years in one level for the same reason that EPA uses five years. They found in their meteorological analysis and their guidance that using five years in meteorological data characterizes the historic variability in meteorology.

What I meant to say when I made that statement is that when people run a permitting application for a continuously emitting industrial source, and they run it through five years of meteorological data, they are interested -- they are actually calculating real estimates for each and every hour because that source is emitting for all that period.

We're using it in a different way just in the -- we're just getting individual estimates of the 24 hour concentration. We're doing that 825 times to develop that time series. That's all I meant.

But in terms of characterizing variability, the same argument -- I'm making the same point that EPA is making in terms of using five years.

DR. WANG: Another comment on the very last question we are supposed to ask. On the methodologies that you likely may use to improve the flux estimates, I guess it was brought up earlier, is using some of the soil space, the emission model's more deterministic predictions, that will integrate all the factors we have been debating, the soil moisture, the temperature, even the bulk densities, soil type, organic matter, degradation. Those can all become an input.

Even though you don't have direct measurements, likely you may have a very reasonable guess, I suppose, to put that in the model and provide another prediction of the source trend to fluxes or time.

And at least that can be provided as a comparison to your current approach, probably.

DR. REISS: I'm very interested in these soil models. Like I have said, we have only a limited amount of data to sort of characterize what the impact of all these factors are.

And in developing a model to be used for regulatory purposes, I think we're reluctant to rely on something that may be purely theoretical without field data, actual field data to back it up.

But that's probably the future and I can't honestly say I know what the state of those models are and how accurate they are. But if something could be done that was accurate and would meet the regulatory burden, then I would be all for incorporating something like that.

DR. ROBERTS: Let me ask other panel members if they have any comments to add on this question.

Dr. Heeringa?

DR. HEERINGA: I want to be sure that we're clear in our report on the interpretation of the perturbation of the estimated flux rates.

In slide number 43, I don't know if we can bring that up, slide 22, I think in your presentation, and I haven't cross-referenced with it the report to see, but I think we recognize -- I think there is a typo there. You have standard error instead of CV in the equation. But I want to be sure.

If we go now to slide 43, the sigma error there, that is the, essentially, the standard error on a prediction of a future value.

As I look at this CV, that CV makes sense as a standard error on the expected value of the flux rate conditional, which is a different item. And I wonder if that sigma includes not only the variance associated with estimating the regression, but also the residual variance associated with predicting a future value off of that regression.

In other words, in linear regression where you have the prediction problem, one of developing confidence bounds for the expected values, one of them of developing a projection bounds for a future value, the projection bounds are broader than the confidence bounds on the expected value.

I'm thinking that this is essentially a random draw from the expected normal distribution for the expected value, taking into account the variance on the regression coefficient, but it does not incorporate the residual variance for predicting a future value from that regression coefficient. Just mention that here. We'll try to provide formula, just for clarification.

DR. REISS: I think if you -- I may not completely understand your question. But I think at least when you constrain the intercept through zero, those would be the same. Is that correct?

DR. HEERINGA: No, they shouldn't be. It's just a different concept of whether you are trying to develop error bounds for your predicted regression -- your expected value regression line or projection bounds for a future value that you predict from that regression.

There is an additional error term, I think, that has to be added for the latter.

DR. SMALL: I think you are right. If you think about the prediction error for a value, though, it would be for an ambient concentration value predicted as a function of an aerial emission rate of a flux rate.

And that's really not what they want. What they do want is the uncertainty in the emission rate or the flux rate, which is the slope.

So in a sense, what they have done there is correct statistically as terms of characterizing the uncertainty in the flux rate, which is the slope of the regression.

The bigger issue, which we'll get into when we address question 3, is whether or not that's -- the coefficient of variation that comes out of that, which as we see are typically on the order of 10 to 30 percent, really reflects the site to site, period to period uncertainties and variabilities that are really out there or if it just reflects the emission rate at that site for those set of tests.

So that is a much bigger issue.

DR. HEERINGA: Thank you for that clarification.

But it is the issue of whether in fact you are trying to deal with projecting a value from that regression to a larger population from which this sample is drawn here.

DR. SMALL: Correct.

DR. ROBERTS: Dr. Portier, I believe, would like to follow up.

DR. PORTIER: No.

DR. ROBERTS: Cover something different.

DR. PORTIER: Different question. I wanted to talk a little bit about the projection grid.

When you stop and think about the computational cost that goes on, I would say probably 70 percent of your computation occurs in or immediately around the field. And yet your interpolation for your boundaries are much further out.

It seems like you have a sparse set of points where you are trying to interpolate and develop that pink curve and you have a dense set of points right close to the field.

I'm wondering if some kind of adaptive system, which would start with a course grid and then allow you to fine tune that grid in a second task to actually get really nice, if that's your objective, might really speed up the computational thing.

If you are trying to put stuff into a GIS, maybe -- I tend to agree, if you were trying to actually develop the whole probability surface, you probably got the right grid right now.

But if all you are trying to do is estimate that threshold boundary, you are wasting a lot of your computational area simulating what is happening close in when what you want to do is what is happening out at that --

DR. REISS: You still get -- there are still distances or slices of the field where it is close in.

Actually, the first run I did of this we didn't have as dense a grid close into the field for that reason.

And I just found that there were too many errors in the interpolation algorithm. I have a fairly good hold on these kinds of mathematics, but I'm sure there are other more computationally efficient things. Maybe a two dimensional interpolation might even improve the computation.

I'm all willing to hear ideas on how you can improve the computational efficiency.

Can I make one other comment about the point you made in the -- as I said, I'm more than willing to hear ideas on computational efficiency. But I would mention that at least 75 percent of the computation time is the ISC model.

If you really want to reduce the computation efficiency, you need to reduce the number of receptor grids as opposed to the latter issue about the loops.

I think the biggest bang for the buck is getting a sparser receptor grid to simulate the calculations we have here.

DR. PORTIER: I want to see you tackle both of them.

DR. ROBERTS: Other comments or suggestions from the panel members? Yes, Dr. Seiber?

DR. SEIBER: The documentation provided I think gives a very good description of how ISC is coupled with PERFUM. I thought that was very nice. And of course they have -- the back calculation of flux and ISC have been used separately and now you put them together in very logical combination. I thought that was very well described.

On the other hand, even though ISC has been used for many years and successfully in these kinds of, not only fumigants, but in some cases for other pesticides and I think for fumigation chambers as well as fields, there is a longstanding use.

But we probably shouldn't forget some of the differences in an agricultural field where you typically start spraying over on this side of the field and in a ribbon manner you may go through and fumigate, and, of course, you are emitting over here while the tractor is still moving over there.

That only happens during the application. But I have always wondered myself, and maybe you have an answer for this, maybe it should be brought up, is that a complicating factor? It would be nice if the field was all treated at once and then you go out and put your samplers up and it all emits at once, but it doesn't happen that way.

DR. REISS: For a couple of the field studies, I didn't describe it in the report, but for a couple of the field studies where it took a long time to do the application, we essentially broke them into slivers in accordance with the time it took to do the application.

It is a simplification, obviously, when we run PERFUM to put it altogether or just assume it is all applied at once. You are certainly capable of doing that in the model. Whether or not it would be a worst case or best case situation would just depend on the wind direction relative to where you were applying.

It is hard to make choices about how to exactly apply that. You could look at it in terms of sensitivity analysis, but for calculating the fluxes we did take that into account.

DR. SEIBER: So a simulation could be run taking that into account and show that really it doesn't matter that much in the overall result if it doesn't.

The second comment on are there any other potential critical sources of data. Again, come back to the fact that the wind and -- particularly, the wind can vary so dramatically from one field to another when you get into more of the complex terrain situations, and some of these fields are in fairly narrow valleys, for example, where you could have a Ventura effect that's probably not taken into account by the nearest meteorology.

I don't know how to get around that except -- I'll just go ahead and say it. There may be cases where you need to buy or rent a met station and take it out to the field. It is just not -- that weather pattern just isn't simulated by your nearest recording station.

DR. REISS: That's possible. I think the key question is, since we're not predicting the concentrations for a particular field, is whether the weather stations that are out there are accurate enough to capture the overall variability that's out there.

You know, the idea of looking at a specific field if somebody was very interested in doing that, then that's an interesting idea. And you could certainly refine the estimates.

As Dr. Yates pointed out, maybe you could justify not having a buffer zone in a direction that you know is not going to have a lot of impact.

DR. ROBERTS: Anything else on question 2?

Let me ask the agency then if the responses from the panel to question two were clear? Do you need any clarifications on some comments or suggestions?

MR. DAWSON: No, they are clear. Thank you.

DR. ROBERTS: Before we move on to question three, which will be the last one we'll tackle today, I think the panel could probably benefit from a 10 minute break. And I think the audience could benefit from a 10 minute break too.

Let's take a short break. Try and reconvene about 10 minutes after 4. We will tackle number three, which will be the last one today.

(Thereupon, a brief break was taken.)

DR. ROBERTS: Pose question three to the panel.

MR. DAWSON: Question 3, The determination of appropriate flux and emission rates is critical to the proper use of the PERFUM model as these values define the source of fumigants in the air that can lead to exposures.

Upon its review of how flux rates can be calculated, the agency has identified a number of questions it would like the panel to consider.

In PERFUM, flux rates were treated as a probabilistic variable with an uncertainty developed from the statistical bounds of the flux calculation. For each measurement period, a standard error is generated that reflects the measurement uncertainty of the flux rate.

PERFUM then perturbs the concentration estimates within each period by the standard error using Monte Carlo methods to simulate the uncertainty in the flux estimates. What, if any, refinements are needed for this process including the manner in which the flux values were calculated for each monitoring period to generate the standard error estimates?

How appropriate is it to use a flux or emission factor from a single monitoring study (or small number of studies) and apply it to different situations such as for the same crop in a different region of the country?

Please comment on PERFUM's capability to adequately consider multiple, linked application events as well as single source scenarios.

Does PERFUM appropriately address situations where data are missing?

In the back calculation approach used for estimating emissions rates, the regression of measured versus modeled values can be forced through the origin or not. Which approach does the panel prefer and what are the implications of each approach?

DR. ROBERTS: Okay. There are lots of questions in this question.

Before we get started, let me remind panel members, please, bring the microphone in close and speak directly into it as you make your comments. Let's start with Dr. Yates.

DR. YATES: This one has a lot of -- there is a lot of substance to it. In some ways it's -- I think maybe the -- well, I guess where I would start is that I think that the idea that the PERFUM uses a probabilistic treatment overall is better than if it would have all been deterministic, just as kind of a background statement.

Because capturing variability, even as you will see in a minute in some of my comments, is probably not all the variability that's present. It is still better than just assuming some single average value.

There are a number of assumptions that go into how they determine the flux. And if you can get to the point where you can accept the assumptions, you know everything is really fine.

However, it seems like there are possibilities to improve the risk assessment by adding additional sources of variability, which I have got some slides I'll be showing in a couple minutes that I hope will bring the ideas across clearly.

One thing I like is that the -- just the approach as a whole, is the fact that the buffer zones are affected by meteorology. And that the -- as I think we talked about earlier, that the shape wouldn't necessarily have to be circular.

So anyway, now to get to the questions that are at hand, I guess the first one was what refinements -- where is it, flux values. Okay.

I can't seem to see the question in here. There are so many of them. What refinements are needed for this process including the manner in which flux values are calculated for each monitoring period to generate the standard error of estimates?

It seems to me first off that the standard error the way it is being currently done captures some of the variability. In essence, it is the variability that is expressed as what I would call discrepancies between the data and the model. And that's good.

And I think that in terms of if you are just looking to having a flux value that would allow you to use the model and the risk assessment for that particular field, it seems like it is okay. Because as Dr. Reiss said, it is sort of calibrated for that particular field study.

Now, the problem I see is when you start trying to apply that elsewhere. And let me have the first slide.

We have looked at a number of studies of methylbromide. This slide here shows a cumulative probability versus total emissions for -- I forget how many methylbromide studies are shown here, but, basically, these are all direct measurements of methylbromide total emissions.

The studies that are included here would be Yagi's studies, Williams', who also worked with -- Yagi also worked with Ralph Cicerone. Williams worked with Ralph Cicerone. Mike Majewski and Jim Seiber's study and some studies of ours, which are the ones that have the white bars.

Now, if you look at how much variation there is across studies, you can see that the low end total emissions are somewhere around, I think that's about 30 percent. And they run all the way up to the high that we have seen, which is over 80 percent.

When I was looking at this, this actually was part of a study that we were trying to look at what the effect on global emissions would be if VIFs were used instead of high density polyethylene. Because these field studies were all shallow injection, tarped, flat fume studies.

So in the process, my objective for the paper was I looked at fitting a log normal distribution to this data where -- I didn't want to fit through the data, so I put kind of bounding distributions on there.

So basically, it looks like the mean value would be 50 percent and there is a lot of spread between these studies.

So if one of these studies were used to obtain the emissions and then used for risk assessment, I think you would get quite a different outcome than if you sampled from one of the let's say a 50 -- a mean distribution of 50 percent with this kind of spread.

If you look at the bars up here, this is the variability we observed in the experiment. We had a number of methods for estimating the emissions and so we were seeing like -- I don't know, that's about 10 or 15 percent variation in one study.

But you start looking at over the study, and the variation is quite a bit larger. And I think this is typical. The data I see, and it is shown in some of these reports, it seems to be all over the map.

And as a matter of fact, there are times when you can see things -- for example, if you look at in the report on page 92, table 6.1, there is emission ratios for methylbromide from the CDPR analysis that have three broadcast columns.

The broadcast high barrier, they have a very high barrier and a VIF. If you look at the mean emissions for these things, you will see that applications with a permeable film have a mean of .253. If you have a less permeable film, the mean goes up to .5, which right away you start saying physically something seems a little wrong here. VIFs, the mean drops to .3. But it is still higher than when you have a permeable film.

So there is -- the variation that you are seeing in experiments is tremendous. Even to the point where when you use a VIF, which is very good barrier, I will actually show a slide which shows the permeability of some films, you can have higher emissions from an experiment than you would when you basically have a barrier that lets a lot of gas go through.

So this kind of variation seems like it might be something that should be somehow included into the risk assessment model.

Can you get the next slide? Now, all that was on cumulative emissions. What the model uses are period emissions. This slide shows -- I guess you can't see the scale. There is days. That's one day, two day, three day. It goes out to eight days.

Here we have the flux density. These are period values of two to four hours. Three methods. The same basic data set was used. This was all done on the field. We had a sampling mass in the field. We had anemometers. We got temperature gradients so we could do the aerodynamic flux method.

But that same profile of concentrations on the field and the anemometer wind speeds that we collected on the field, we used a method called theoretical profile shape. And it integrated horizontal flux method.

Three separate ways to analyze the data. If you look at this, there is no correspondence between the flux at a particular time for each of the three different methods. And yet the cumulative emissions are almost the same for each three. So there is a real problem when you are trying to get period -- what I like to call instantaneous flux. It is very difficult to get numbers that have meaning when you start spanning across different locations, different methods for estimating them, different times.

Can I have the next slide? We had one other -- unfortunately, this has some other information on it as well, but we also used flux chambers to measure the flux.

The points here are the flux chamber value. And if you compare it to the ones before, the scale on the previous slides were 300. This is 120. You will see that there is, again, a drastic difference when you use different flux methods.

So it would be nice if the method that is used for obtaining the period flux measurements contained variability that you would be experiencing when you are using it in a region as opposed to just at a particular field.

Let me just run through the rest of the slides and then I'll finish up on my comments.

But let's skip that one. Basically, all that was showing was if you have different amounts of organic material in the soil you will get different emissions, which is fairly obvious.

I'm showing this slide for Randy Segawa. He was asking about what the permeability of high density polyethylene film would be for methyl iodide. It is shown here. This is for methylbromide. I think the value at 25, I think is 4.3. And at 25 for methyl iodide it is 1.0.

This basically means if you use high density polyethylene, you can expect to find that the emissions from flat fume with a high density polyethylene barrier will be more than what you would experience with methylbromide, assuming that the film is a controlling factor, which in our experiments and in our modeling that we have done, we have found that the film does tend to control the emissions.

You can see also there is a temperature effect. A very strong one. This is going across a whole number of chemicals, but even here there is -- I think if you go up 10 degrees you get about a 1.7 factor in the permeability.

So as you have daily cycles in the temperature, the temperature to the film is extremely hot. It is basically like a greenhouse effect.

And you get temperatures say on the order of 70 degrees C. And that -- we don't even have permeability measurements at that high temperature. So the film really does have a lot to do with the emissions.

Can I see the next slide? I don't want to talk about that yet.

So I think that it's kind of important to include the spatial and temporal variability that occurs in flux measurements.

The difficulty, of course, is how is that going to be done these experiments are very expensive, very time consuming. And yet to get those -- I mean, there was like 10 or 11 studies there, full studies. And that might be cost prohibitive.

Another question was whether a single study -- I think that pretty much has been answered. It is hard to believe a single study would be appropriate to generalize to a regional or maybe a state scale.

Most of this I have already covered in talking about the slides. I think that it's good that you separate the analysis based on the fumigation type, where you have flat fume and you get flux values for the flat fume versus the raised bed and the drip. Although I did see some things in the data that you presented that kind of struck me as a little bit odd in just that -- I think it seemed to me that the drip had kind of a kind of a high emission rate, which a lot of people are saying that emissions tend to be lower with drip, which could be due to the partitioning of the chemical into the liquid. You put a lot more liquid into the soil, there is more partitioning to it.

But anyway, that's good in the sense that the emission values that you are using are more appropriate for the analysis.

What was the other question? Oh, about linking it. It seems to me that the approach -- I don't see any problem with being able to link applications assuming that the emission data is appropriate for each of the fields that are put in there.

And if you use some kind of a probabilistic form, well, then, it wouldn't even be an issue there. You could just sample from that probability distribution and just assume that the -- if the two fields would fit on that probability curve.

Then as far as with missing data, I'm not sure how to respond to that. In my reading of it, I didn't see where missing data -- except for maybe with the meteorological, which I'm not really the best person to be answering that question.

There was some discussion in the text about when you are missing data how to go about filling it in. You know it probably would be better for someone else to talk about that.

Then as far as the back calculation approach for estimating emission rates, this idea of using the regression I think is fine, especially when you have very small intercept.

But I wonder if it wouldn't be -- I think you could bypass this whole thing if you, instead of setting -- in the model setting a default emission rate and then using the regression to try to figure out a way to scale it to what you observe in the measurements. It would seem like you could just at each location that you have a receptor you could find the emission rate that causes a match between measured and the model.

So what you would end up getting is a series of emission rates. You could have like -- for every receptor you would have the field emission rate that gives the match.

So say that you have 10 receptors, you get 10 emission rates, you take the average. That should be the same as what you would get from your slope. Then you never really have to worry about an offset because it is not really pertinent. You also have a range in emission rates which gives you in a sense a standard error or some kind of an error measurement.

The only difficulty I can see in doing something like this would be those situations where at a receptor you have a measured concentration but the model can't give you anything but a zero.

And in a sense, that's -- I mean, that's going to cause you -- that's what is causing the problem with the offset anyway, probably.

And it would seem that if you just looked at where the zone where the model -- where the plume is, you could exclude those points that are outside of it.

Otherwise, I don't see any real -- I can't think of a preferred way in terms of whether you should go through the origin or not. I think depending on what you think the cause is for that intercept -- If you look at it as a background concentration, then it seems like all the data should have that little background amount scaled out or subtracted out so it goes through zero.

If it is from some other factor, then it probably should be ignored. But who knows what the truth is.

So then I guess the last thing I would like to just at least show something would be this idea of using a soil based model. I will try to run through this quickly as I know we're running out of time.

Can I have the slides again?

This is some work that actually Dr. Wang has worked with us when he worked in our group a few years back. What we're doing is using a fairly complex numerical model that describes water, heat and soil transport, the chemical transport would be methyl iodide in this case.

And historically, there have been -- actually, historically, there was one way in which this atmosphere soil boundary condition was characterized. And that's the one that says -- it is the boundary condition right here where the flux at the soil surface is equal to a mass transfer coefficient times a difference between the gas phase concentration in the soil and the atmospheric concentration.

This mass transfer coefficient has been parameterized by setting it equal to the gas phase diffusion coefficient in air and some boundary layer thickness.

This boundary layer thickness is something that is kind of arbitrary. That doesn't really have very good physical meaning.

If you have barometric pressure changes, you can actually have air move through from the soil into the atmosphere or vice versa in which case a stagnant boundary layer doesn't really even make physical sense.

But this is still used in this particular model, and provides one way to estimate the emissions into the atmosphere. I have a slide which will show for allowing this to be temperature dependent or keeping this a constant, and compares it to some measured data that we collected in a methylbromide field experiment looking at flat fume shallow injection.

A new, newer, I guess I should say, boundary condition, this was developed by John Baker at Saint Paul, is shown here where you have micro meteorological information. You have the stability of the atmosphere. There is wind information, Reynolds number, Schmidt number (ph).

So basically, to use this boundary condition, if you have micro meteorological information, you can actually let what is occurring in the atmosphere control what is happening at the surface.

Can I have that next slide? So for that first boundary condition, which is all soil base, you don't really have any knowledge of what is happening in the atmosphere.

For a constant mass transfer coefficient, you get a very simple, common flux curve that you would see if you were looking at isothermal conditions.

When you have allowed the temperature at the surface to be controlled by solar heating, you start seeing cyclic behavior. Some of this you actually see in the documents that we were given. This is kind of high at the beginning, decaying curve with cycles in it.

But you still see there is a fairly large mismatch between measurements and simulations. Now, if you allow atmospheric conditions to control what is happening, can I see the next slide, you can get a much better agreement of what is occurring.

You see this kind of sharp changes in behavior that you often see when you start looking at flux data that's taken at a higher frequency. So this would be an alternative. It is time consuming, there is some information that I know right now would not be available.

You couldn't go and plug this into PERFUM's tomorrow and get it to work. But it might be a direction that should be headed towards since it seems like the -- there is just some -- it seems like this idea of transferring from one location to another is not going to be very easy to do using an indirect approach. You have to go out to a lot of fields.

When you start looking at things like emission reduction strategies, it's going to be very difficult to get all the data you need in field studies. But in here all you have to do is change how you handle the boundary condition and then maybe run a study or two to verify that it makes some sense.

But anyway, there was some discussion earlier talking about whether this would work or not. I thought I would just put it up there so people could see that, at least in this case -- actually, I have done this for two situations, this study and looking at trialate (ph), which is a herbicide of lower volatility. It worked for that as well. That's only two studies which doesn't prove that it is right.

So I think that might cover everything. I think those are all my comments.

DR. ROBERTS: If you think of something else, we'll open it up again.

Dr. Bartlett I believe is next.

DR. BARTLETT: I think I talked earlier about the first question a bit on the flux values and the standard error estimates and it has been covered a bit already.

Then I guess I feel particularly wary about the limited amount of studies and the ability to generalize.

And I believe you say -- and I totally appreciate how much the cost and effort to do each study. But there is quite a bit of variation in weather and time, which you see just in your monthly data itself.

So that generates maybe another way of looking at variation too. But on the other hand, your approach tries to capture that.

But there are significant regional differences in weather. And unfortunately, we can talk about later on the weather section, but there are other weather data sources that might give us some idea of what some of those variations might be and do. So I guess I will delay it to the weather question.

As far as missing data, it seems like you are using standard approaches as far as missing weather data. But of course you wouldn't do that if you were doing a study. During the study period, you wouldn't use missing data because some problems can happen there.

And the issue of going through the origin, I kind of agree it is either there is another factor at work or nonlinearity. It is not unusual to have nonlinearity as you approach zero.

I mean, I think it is very unusual to have linearity when you are close to zero. And if you have a lot of data points close to zero, that's a problem.

I mean, there are other ways to deal with detection limit problems than conventional, which is one-half zero and full and truncated data. There are ways to project, come up with other data points, the censor data techniques to develop that. I don't know if it's worthwhile to do that.

I have somewhat reluctance when you force it to zero, but you already have quite a bit of differences on your slopes from the different studies.

And there are, of course, ways to put the studies together and use the data as you get more.

As far as -- so I pretty much agree with a lot of the discussion of Dr. Yates. I'm much more familiar with the physical process soil emission modeling. It would be nice to know how this would compare with that. But maybe -- and I'm less familiar with volatiles, but it seems like a lot can be learned about that.

I guess that gets into the issue of generalization again. If you are going to different crops and canopy, I'm not sure if fumigation is applied to situations where you have crops already growing or is that always pre-planting?

DR. REISS: I believe it is mostly pre-planting. I think they are also going to look at other applications. I don't know if there is anybody from Arvesta that wants to step in. But everything we have looked at to date has been pre-planted.

DR. BARTLETT: I guess kind of a general thing that I have been thinking about that applies to a lot of this is when the comment came up about mass balance. And I think what I'm trying to synthesize what is happening here now that I know more about this substance and methylbromide is that quite a bit of it is emitted within the first week or so.

And it gets to me, again, the question of the persistence of the substance in people's bodies that you may be focusing on the first 24 hours but you may have a lot less variation if you look at exposure of, let's say, a house in the boundary area for a week or something like that, because the variation seems to wash out after that a little bit.

DR. REISS: Can I answer that?

DR. BARTLETT: Sure.

DR. REISS: For methylbromide, they have looked at that in a different way than these source of models. DPR has calculated data or measured data in schools, for example, I think to get longer term averages.

I don't know what the case will be with methyl iodide. It is a much less persistent compound.

But you really can't deal with those sort of recirculation issues very easily in a model. You could, but not in this type of model. So you would have to deal with that in another way.

So it is not a goal of the model to get at that long term or weekly concentration. But it is not necessarily something that's going to be ignored in the risk assessment process. It is just not being dealt with right now.

DR. BARTLETT: I think with each one of your field studies you have the first 24 hours but you go on beyond that. Right?

DR. REISS: That's correct.

DR. BARTLETT: So it would be of interest to look at what would you do if you looked at the longer period and you have more consistency -- does that account for a lot of the variance, I guess it is not clear to me, the variance between them?

And maybe the cumulative emission curve that we just saw there might bring your data together more. And you are just seeing -- and then as far as exposure, it does make sense if people are living and working on the periphery to look at that as a cumulative exposure over that time period. I'm not sure if this is -- once it gets in your body how long does it stay there? Do we have some idea of that?

MR. DAWSON: Our toxicologist is sitting over here. It's relatively short lived. Again, as I said this morning, I think if it is capable of looking at -- averaging over the several days based on what the data show, we potentially could be interested in that as part of what we would do in a risk assessment.

Don't forget, it is not only this particular case, but we're looking at all the different fumigants. They all have different toxicology profiles. So we would be interested in varying durations.

It is going to be dependent on the data. As we look across the different chemicals, you see a very different kind of emission profile. So we're definitely interested in that kind of capability.

DR. BARTLETT: I guess it is the opposite point

I made before when I was concerned about the one two hour peak as far as acute exposure and then as far as something that might build up within as a cumulative over a few days.

But I'm glad to hear you are looking at both. But that's something the model could generate as well. Right now you are doing 24 hours.

But in the field studies, I think it is essential that you continue to do the field studies in that way, but maybe presenting the data in cumulative emission.

DR. REISS: In terms of the people or the distances right surrounding the field, the model could and hopefully eventually will look at shorter and longer term durations to deal with other fumigants and other issues.

DR. ROBERTS: Dr. Majewski?

DR. MAJEWSKI: I think it is hard to add much more than what Dr. Yates already presented. I agree with most everything he said. All my questions, in fact, have been answered previously. But I would like to reiterate that or go through the questions as an exercise.

What refinements are needed? Basically, I think as the model progresses more field data will help refine the model.

I don't think that you can take results from one area and apply it to a completely different area or state like taking Central Valley data and applying it to Minnesota. I think that would be inappropriate use. You may be able to do that in various areas of the Central Valley.

I think that's about all I can add. Like all models, I think it needs more field validation, basically.

DR. ROBERTS: Dr. Small?

DR. SMALL: I'll sort of reiterate and follow up a little bit on Dr. Yates' discussion of the mechanistic models. I think you are probably going to need some type of a dual strategy in which you are working on the mechanistic models as a long term objective.

But it is probably the case that that's not going to be ready for short term use, so that you are still going to have to have some more empirical approach.

I think given that, the indirect method is a reasonable place to start. I think it is a good approach for estimating the emissions at a particular site for a particular observation period.

I think when you start applying it to different conditions and different sites, though, I think you recognize that the type of variabilities that you are representing aren't probably representative of those.

And that gets back to the chart that we saw before.

I think the model breaks down in a couple of different ways. One of them is that the standard error method that you have for uncertain emissions, if you apply them to individual time periods and you sample those independently, you get still a further smoothing.

If you have, say, four time periods in a day or six time periods in a day, you start off with a coefficient of variation that's 20 percent, you are going to be dividing that by the square root of four or the square root of six in terms of your daily variability.

So I think that's part of the reason why you got those results where those upper tail distributions seem to be pretty flat and close in. Because I think you are doing a number of things that tend to be variance reduction procedures.

By assuming the independence, I think, if for some reason the actual emissions due to variability or site conditions are higher during one period, they are probably going to be higher during the next.

Now, once you get into sort of larger scale variations, normal distribution or even a T distribution, which is symmetric, really isn't going to fly because you are going to start generating negative values.

You are not really going to be able to characterize coefficients of variation of 50 percent or 100 percent with a normal distribution or a T distribution.

If you start using a log normal distribution, you might get into mass balance issues where you start generating more than one hundred percent of your application.

So I think one possible hybrid approach might be to first consider the total mass emitted either during the entire period or during the first day, and use a distribution that would constrain that between zero and one. The logical choice for that is a beta distribution.

If you go back to the distribution that Dr. Yates fit with a log normal, if you want to constrain that, that's one approach. But if you want to constrain that between zero and one, you could use a beta distribution. His particular plot actually looked like a mixture of two beta distributions, because it wasn't really that smooth. But that's one approach.

Having that then as sort of characterizing your uncertainty in the total amount that is emitted, either over the entire period or over the first day.

If you want to work with a daily averaging period, then you might be able to take representative profiles that sort of start off a little higher in the beginning, have some diurnal variation to them and a random component, and somehow constrain them so that you end up with a sampled or simulated value of the fraction that was applied.

I'm kind of working out here as I go kind of an approach that might allow you to capture a larger amount of variability but still be consistent with the mass balance constraint. You don't want to emit more than you apply.

You might have caught in my questioning earlier that I was not enamored with the approach of using the second day's emissions, applying them to the omitted first day emissions because you got started later on.

I think that once you get into the second day, as you have shown, a significant portion of the mass has already been lost. So you would expect the emissions on the second day to be significantly lower during that time period than on the first day even though they are at the same time of the day.

DR. REISS: I may not have been clear about that.

We didn't miss any periods in the field studies. We didn't miss any periods during the field studies.

It is purely a matter of numbering the hours. Say if you had 19 hours in the first -- you know, it said that we had 19 hours in the first day. That was 19 hours from the start of the application and the start of the measurements to just the beginning of the measurements for the next day.

So when I'm using that first 24 hours, it is the first 24 hours after the application. So it isn't -- we're not missing anything.

DR. SMALL: Good. I think a little clarification would help on that issue.

I had some thoughts too on the intercept term. I think in your documentation you would be a little clearer as to what a nonzero intercept might represent physically, particularly on the background issue. I think that would help. A statistician would actually be baffled at the approach that you have taken here, which is one in which if you get a statistically an intercept which is statistically significantly different than zero, then you reestimate it and force it to be zero.

Because a statistician would say if it is statistically significantly different than zero that shows that it really is significantly different than zero and you ought to allow it to be. In contrast to the case, if it is not statistically significantly different than zero, then you could argue let's force it to go through zero.

So you have taken an opposite approach that a statistician would take. I think whatever you end up doing with that, you need to motivate a little bit more with the physical reasons why you may have background concentrations, particularly if you have done multiple tests in an area at that site or at other sites.

DR. REISS: The physical reason is you can't use the intercept term to calculate the adjusted flux rate.

So you have the possibility of underestimating the flux rate if you have a positive intercept, which is typically the case if you have an intercept.

DR. SMALL: Unless you argue that it is from other sources other than from that field.

DR. REISS: I think that's highly unlikely. It could be from diffusion processes that are not accounted for in the model. I think that's more likely the reason.

So I think the reason that it is done is just not underestimate the flux rate. Make sure you predict those --

DR. SMALL: It's conservative.

DR. REISS: Yes, it is conservative and you make sure you predict those high values. That's the idea.

DR. PORTIER: Could I have a clarification question on that before you go on, if you don't mind?

DR. SMALL: Yes. Then I just have one minor last thing. Go ahead.

DR. PORTIER: When you do the regression and the intercept is significantly nonzero, is it normally positive or negative?

DR. REISS: It is typically positive.

DR. PORTIER: You are in good shape.

DR. SMALL: That's common. That's very common. There is a number of reasons why that could happen. Even if your errors in your measurements are non normal, you are never going to get negative concentrations. You don't report negative concentrations. You will sometimes report higher concentrations at those very low levels.

So you still have kind of a log normal error structure in your measurement rather than normal.

DR. PORTIER: That only happens because you get a lot of zeros. You can still have a negative intercept with a couple of positive zero values if you didn't have all those zeros at zero.

DR. REISS: It is possible. It is just much more likely --

DR. PORTIER: But if it were negative, then your statement wasn't true. By forcing it to zero, you would tend to underestimate the flux rate. So as long as you are on the positive side you are always okay.

DR. SMALL: I just have one last comment.

In a document, it brings up the issue of the log transformation before the regression.

That's inappropriate because the linear model is the mass balance model. If you want to be -- consider with just the physical superposition mass balance approach, you have to stick with the linear equation.

Now, if the errors are big, they may be non normal because you don't want negative concentrations. That's a much more sophisticated statistical model. You have to use numerical methods, numerical maximum likelihood estimation techniques to do that. I don't know that it is worth it, given the sort of bigger picture that we have laid out here, which is that you are really not all necessarily all that interested in skinning the last bit of statistical accuracy for that study at that site but rather considering the variation from site to site that could occur.

And I think this approach, in the long term, using a mechanistic model perhaps in the short term considering sampling your percent of your application that volatilizes and then distributing it in some reasonable way would be a good approach for that.

DR. REISS: Thank you.

DR. ROBERTS: Dr. Wang?

DR. WANG: I would like to comment on PERFUM's capability to address multiple and linked application events versus the single source scenarios.

I think it is a big plus to be able to use this model to look at multiple events or applications at the same time. And that should probably be explored a bit further beyond the example you presented.

It is true that wind direction -- one moment you may only have one wind direction in close proximity. But there could be -- that could change. And also from a probabilistic approach, that direction may not be just in one direction. It could be in -- covers a large region, angle in the angular orientation.

That means it is going to translate to the risk factors if you have closely related fields that's fumigated, and that maybe you want to expand on those areas.

Also, the distribution of those fields that maybe fumigated in a fairly close time period and how they going to be very uniformly distributed or very far apart like the example you presented.

They may be somewhat random or they may not be very random since the logistics of soil fumigation tends to optimize those operations.

So those things may need to be considered when you try to -- when you come up with some additional examples to help address those scenarios.

DR. ROBERTS: Are there other panel members that would like to contribute comments? Dr. Winegar?

DR. WINEGAR: I would like to make a couple comments about -- first of all, using a single emission flux factor for a single study to apply to different regions of the country.

That kind of reminds me of the use of AP42 kind of emission factors where you look in a book and you have some type of emission source and you apply it to that source across the board.

In my experience in doing source testing for a fair number of different types of sources and trying to reconcile that with emission factors, it doesn't match up very frequently. And so -- I mean, you have heard other people comment about the same thing. So I'm just concerned about that.

DR. REISS: It is a good comment. When we have seven different studies, we have applied it for all the studies, and the range in the amount that was emitted during the first 24 hours is between 35 and 60 percent, that's a variability that we're concerned about and concerned about whether there is more variability there.

However, when you look at like AP42 factors, they can vary by orders of magnitude, the uncertainties on those. Just by mass balance considerations we're certainly doing better than order of magnitude. Whether it is possible that there is 70 or 80 percent example out there, that's possible.

But just by mass balance limitations it constrains the uncertainty in that direction.

DR. WINEGAR: And also from just a pragmatic point of view, questions will always be raised, well, you used it here versus -- and the study was there, was that any good.

So my other comment is in regards multiple application events. You stated that you thought it was pretty improbable for more than one event to be done -- application to be performed within a certain area.

I got to thinking about some monitoring that I have performed for the alliance of the methylbromide industry a couple of years ago on behest of DPR where we did ambient monitoring for methylbromide in a couple locations. One of them was around Santa Maria.

There was one situation where we had -- there was a large field to the northwest of the center of town where there was multiple fumigations that had occurred. It was a large field, so they did it sections at a time. But they were like one day after the other.

Downwind, approximately a mile at least, much more than any of the buffer zones would have predicted, there was one of the highest concentrations that we measured. And so I think that scenario can occur in those kind of situations.

DR. REISS: Yes, I agree it can occur.

DR. WINEGAR: That was an example of how it really could be impacted.

DR. REISS: The ideal circumstance would be some sort of Monte Carlo model where you would randomly choose the probability. You would have a probability for there being a multiple application, how close it was to the source you were interested in and the direction from the source you were interested in.

I would like to develop a model like that to look at this problem, but I can't. I don't know how to assign any of those probabilities. I don't know what data I can use to assign any of those probabilities.

I think we're going to have to look at more in a worse case situation. There are for methylbromide risk mitigation measures that restrict the intervals between applications when you have multiple fields and also the distance between applications, I believe, also, for different growers, for example.

Those mitigation options are obviously going to be on the table to deal with this issue.

DR. ROBERTS: Dr. Seiber?

DR. SEIBER: I would kind of chime in on the same point. I think you have probably the elements to do multiple sources. We did it in the Salinas Valley during a period of peak methylbromide fumigation way back in 1995 and worked up the data after that.

We used ISC model and we also compared it with CALPUF just to get kind of a sense of which model would work better in that multiple source situation. We had eleven fixed monitoring sites. And we had roughly 20 applications that were done within about a week or so period.

So we basically said, well, if this field was treated four days ago and based on Yates' emission factors and ours and some of the others up there, here is kind of a boundary of what that field could be doing today, four days later.

And then we had a receptor at a fixed site. So you kind of -- what you basically do is add the contributions of multiple sites.

So it wasn't a perfect study. It had all kinds of limitation. Mainly a resource issue, but I think the approach it looked like it actually could work. And we were able to get a margin of exposure for the people in north Salinas, south Salinas depending on where they were relative to the monitoring sites.

So again, there is all kinds of flaws in it. But I think it is just a tour de force. If you can spend enough time on it, you can probably get it done.

In that case I had a graduate student that basically got burned up trying to calculate all the numbers. But at least he finally got it done.

DR. REISS: I think with multiple applications there is two issues really. The one which I have tried to deal with at least in the prototype scenarios we gave is how would another field affect the buffer zone for this particular field.

I think the question you are looking at is more the regional concentrations.

DR. SEIBER: Right, exactly.

DR. REISS: I think one of the other models you might hear from may look at that issue. But we didn't design this model to look at that issue. It is really totally different from a modeling perspective, a totally different scenario.

DR. ROBERTS: Dr. Spicer?

DR. SPICER: One of the comments I had was with regard to this plot of the measured concentration versus predicted concentration for these flux rates and this idea of the non zero intercept.

I think the point was made earlier that if you had -- if you did not include the non zero values in the intercept that, in essence, your slope was going to be increased and therefore the flux estimates were going to be increased.

And I agree with that from just simply looking at the slope of the line. But I think the problem is that those non zero intercepts are really a failure of the dispersion model at that point. Because, in essence, what the concentrations are telling you is that the cloud is somewhere that's not predicted.

DR. REISS: I think it could be, and I'm speculating here, diffusive, some diffusive transfer, minor amount of diffusive transfer which causes those very small concentrations in the upwind direction.

DR. SPICER: Let's suppose you estimated the stability class incorrectly. Then the value of sigma Z is changed, then the dispersion estimates are off.

So there are all kinds of reasons why that would be the case. But it focuses on that dispersion question. The point is that if there is concentration where the model predicts it, then no flux, ever how large in the model, will predict that mass there simply because the distribution coefficients are incorrect.

Now, what that means, though, is you have now missed some mass that's gone from the plot going downwind and not being predicted by the model. So there is some mass going downwind that is not included in the field that can be predicted by the model.

Because now what I'm doing -- what you are doing is you are fitting a straight line through the points that you have measured that the model tells you do exist.

Now by doing that, you correctly modeled the maximum concentration that the model sees. Therefore, you have the mass right that's inside the plume. But now there is an area outside the plume predicted by the model that you don't have. And therefore your flux estimates are too low.

DR. REISS: Well, I mean the flux estimate predicts the concentration in all directions. So you give it a flux estimate and the model will predict the concentration at all of the points around there.

DR. SPICER: No, I'm talking about getting the flux estimate from this plot. The point is that you are comparing them -- the maximum concentration that you measure versus the maximum concentration that you predict.

And you are lining those two things up. You are saying to get that maximum concentration that I measure, I have to multiply the flux by .43 or .62 or whatever to get the right flux ratio based on my basis.

But that model prediction incorporates a certain amount of mass by virtue of the Gaussian dispersion model. The point is you have now measured mass outside that distribution that's not being accounted for.

Therefore, your flux estimates are too low as opposed to being too high like it seems like everyone is thinking at this point.

DR. REISS: I'm not sure I agree with that. I think particularly when you constrain the intercept through zero you are deriving the flux that statistically best explains the data.

It is the one that minimizes that -- you have got the flux rate that minimizes the residuals between the predicted and the observed values. I can't see that that would be an underestimate in any way.

DR. SPICER: But the model values are always going to predict a certain amount of error regardless of what the slope is. Because the model values that are at zero predicted concentration are simply going to add an increment to the mean square error when you actually do the minimization of the mean square error to get the least squares fit.

The point is that because those values are zero measured concentration, they are essentially thrown out as far as having any influence on the predictions concerned.

DR. REISS: No. Certainly, they are incorporated into linear regression and they are part of the residuals that you calculate.

DR. SPICER: But they will be a part of the residual that cannot change because the predicted values are always going to be zero, though.

DR. REISS: The residual could change, because if the predicted -- well, you are right. If you constrain it through zero, it will always be zero.

DR. SPICER: Therefore, if you leave those points out of the slope determination, it won't make any difference as long as the model predicted concentration is zero.

DR. REISS: I'm not sure I have my mind wrapped around your question. I may need to think about it a little more. But I think from a risk assessment standpoint, the most important thing we're concerned about is that we're predicting that maximum concentration correctly.

That's what is going to be the risk driver. And whatever method we're going to use, we're going to optimize it to make sure we predict that maximum concentration accurately and we don't underpredict it.

DR. SPICER: There is no question about that. The point I'm trying to make is that your ultimate goal is not the maximum concentration. It is the flux. It is that slope of the line that you are using to say gives you these calibrated fluxes which I believe may actually be underpredicting the actual flux instead of overpredicting, which of course is the direction that you don't want to have.

If you were in a situation where you are always overpredicting the flux, overestimating it, then it would be a conservative approach as far as the modeling is concerned.

But I'm afraid that by virtue of the fact that you have cloud where you don't have predicted cloud to be, you are missing some mass. Since you are missing some mass, you have to be missing some flux.

DR. REISS: You could be missing some mass in that upwind direction, but by virtue of doing a linear regression, you are getting some more -- you are getting that mass back in another direction by slightly overpredicting.

When you look at what is causing that intercept, it is extremely small concentrations compared to the maximum concentration that you are seeing in the field.

Whatever you do, whether it is a mass balance or a minimization of the mean square errors, those handful of maximum concentrations, three or four, are going to dominate whatever calculation you do.

DR. SPICER: I guess that's the whole point, is that I don't disagree in that regard. But what this does tell you is that it appears that what you are relying on is the atmospheric dispersion to tell you something about the flux even though it is very close to the source.

And so the net result is that any sort of uncertainties in that atmospheric dispersion modeling right there is going to be an uncertainty that's reflected back into the flux rates.

And that's an issue that I think at this point in time no one knows which way it can go, because all you have are these concentration measurements at one single level.

If you had vertical concentration measurements somewhere, presumably, along the center line would be the best approach of course, but that's not always easy to do, but if you had those then you would have a much better estimate then of what the flux could be.

DR. REISS: We'll take that under consideration as we design more studies. Thanks.

DR. ROBERTS: Dr. Baker?

DR. BAKER: The emission fluxes are determined by the back calculation, which are based on point measurements and you were talking about consideration for future field studies.

Is there any way to enhance or modify the program? Are there line methods that might be appropriate --

DR. REISS: Line methods?

DR. BAKER: -- suitable lasers where you get a line average concentration or something like that and you can vary that with height?

DR. REISS: No, I'm not -- I mean, if we were to do a variation by height, we would just likely set up different samplers at different heights. That would be the way to do it.

We are going to look at in a future a field study we have planned that's coming up, we're going to have an arc of monitors at a farther distance from the field to provide some further validation for how well it predicts concentrations farther down.

And that would, at least indirectly, get at the vertical dispersion coefficient as well.

DR. BAKER: Are they going to be vertically arranged?

DR. REISS: The plan right now is not to vertically arrange them. By virtue of the fact you are assuming some vertical dispersion and you have measurements that are significantly far apart vertically, horizontally, you would get some indication about bias that way.

DR. BAKER: There was a series of tests done several years ago with nitrogen tetroxide where they looked at different sampling arcs. I think it was 800 meters and 150 meters or something like that.

The point is that the only thing that allowed you to sort out the distribution coefficients and those sorts of things was the fact that you did have the vertical measurements in addition to the horizontal ones.

DR. REISS: We'll take a look at it. I would note that this is a great data set in terms of looking at really even validating air models.

There are not a lot of data sets out there where you have on-site meteorological measurements and concentrations you can plug into your model. So it is something we look forward to putting out in the literature.

DR. ROBERTS: Anymore comments on the issue of flux measurements? Let me ask the agency if the feedback and responses to this question were clear or whether you would like some clarification?

MR. DAWSON: Clear and thank you for a very informative look at this issue.

DR. ROBERTS: With that, then, let's go ahead and adjourn this session for today.

We will reconvene tomorrow morning at 8:30. There is plenty more discussion to go. I think we have five more questions.

I will look forward to seeing everyone at 8:30. I would, however, like to ask the panel members to meet in a very short session in our meeting room here so we can discuss the issue of writing up the minutes.

With that, unless anyone needs to bring anything up, let's go ahead and adjourn for today and reconvene tomorrow morning at 8:30.

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[Whereupon, at 5:20 p.m., the

meeting recessed.]

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I, Frances M. Freeman, Stenotype Reporter, do hereby certify that the foregoing proceedings were reported by me in stenotypy, transcribed under my direction and are a verbatim record of the proceedings had.

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FRANCES M. FREEMAN