FIFRA SCIENTIFIC ADVISORY PANEL (SAP)

OPEN MEETING

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FUMIGANT BYSTANDER EXPOSUER MODEL REVIEW:
THE FUMIGANT EXPOSURE MODELING SYSTEM
(FEMS) USING METAM SODIUM AS A CASE STUDY

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VOLUME II OF II

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P R O C E E D I N G S

DR. HEERINGA: Welcome to the FIFRA Scientific Advisory Panel on the topic of fumigant bystander exposure model review. In this two-day session, we're focusing on the FEMS model using metam-sodium as a case study.

I'm Steve Heeringa and I'm the SAP Chair for this two-day meeting. I am a biostatistician at the University of Michigan Institute for Social Research and my area of specialty is not closely related to these topics, but more in the area of design of research for population-based studies.

If we could have the other members of the panel introduce themselves, give their name and affiliation and maybe just a short description of their area of specialty.

DR. PORTIER: I'm Ken Portier, a statistician at the University of Florida Institute of Food and Agricultural Sciences. I work in the area of applied statistics probabilistic risk assessment.

DR. HANNA: I'm Adel Hanna, I'm with the University of Carolina, Research Professor, and my area is air quality and dermatological (ph) modeling analysis.

DR. SHOKES: I'm Fred Shokes with Virginia Tech, Tidewater Agricultural Research and Extension Center. I'm the Director and also a Plant Pathologist.

DR. SEIBER: I'm Jim Seiber with the USDA Agricultural Research Service at the Albany, California location and previously with the University of California, Davis, and University of Nevada, Reno, and my research interest included pesticide environmental fate processes including atmospheric environmental fate.

DR. WANG: I'm Dong Wang, I'm an Associate Professor of (inaudible) Physics at the University of Minnesota, the Twin Cities. My specialty is in fate and transport of fumigants and other pesticides and (inaudible) physics and remote sensing.

DR. WINEGAR: I'm Eric Winegar, Principal of Applied Measurement Science. I do monitoring and measurement study as analytical chemistry and exposure assessments.

DR. OU: I am Li Ou, scientist with the University of Florida. I'm a Soil Microbiologist, my special area is the (inaudible) of organic chemical in soil and the biodegradation.

DR. MAJEWSKI: I'm Michael Majewski, I'm a Research Chemist with the US Geological Survey. My background is in -- I've developed and conducted field studies on measuring post-application volatilization from treated fields and I also investigate the atmospheric transport and fate of organic chemicals.

DR. BAKER: Dan baker with Shell Global Solutions in Houston. I work on emissions modeling and air quality modeling.

DR. BARTLETT: Paul Bartlett, CBNS, Queens College, City University of New York, and I model semi-volatiles environmental fate and atmospheric transport, and also make measurements in monitoring.

DR. SPICER: I'm Tom Spicer, Professor and Head of Chemical Engineering at the University of Arkansas. I work in consequence analysis and atmospheric dispersion of chemicals.

DR. YATES: I'm Scott Yates, Interim Research

Leader, the Soil Physics & Pesticide Research Unity, that's USDA ARS facility in Riverside, California. My area of research is transport and fate of pesticides in soils and volatilization into the atmosphere.

DR. HEERINGA: Thank you very much members of the panel. And at this point in time I would like to introduce the designated Federal official for today's meeting, it's Mr. Paul Lewis of the EPA.

MR. LEWIS: Thank you, Dr. Heeringa, and again welcome to panel members and to the public for participating at his this final day of our meeting reviewing fumigant bystander exposure models. Very briefly, as designated Federal official for this meeting I want to remind everyone that this meeting operates under the auspices of the Federal Advisory Committee Act and my role as a designated Federal official is to serve as liaison between the Agency and the panel to ensure that we are operating under those guidelines with the Federal Advisory Committee Act.

I want to thank everyone again for agreeing to participate in today's meeting and again looking

forward to a very interesting and challenging discussion during the course of today's meeting. Thank you.

DR. HEERINGA: Just a few administrative notes before we begin, in the interest of the individual who has to record the proceedings of this meeting I would like to make sure that everyone when you do take your turn to speak that you state your name again, even though I may call on you if you would just state it so that it makes it a little easier for her to pick up as she's doing her job. Also I guess with regard to timing today our agenda has us going through the day into the afternoon, I know that many of you including those of you in the audience may have flights out of here and Friday afternoon, the horses head back to the barn pretty fast, so I want to make sure we pace this correctly and make sure that we get adequate coverage of each of the questions, but we'll try to move it along so that we do end on time and people can make their flights. But again, we don't intend to short-change any of the discussion, but we'll keep things moving so that we do stay with our

schedule today.

At this point in time I would like to turn to Mr. Dawson of the EPA, Jeffrey Dawson, for any opening remarks that he might have before we return to the directive questions in this session.

MR. DAWSON: Thank you, Dr. Heeringa. My name is Jeff Dawson, I'm with the EPA. We really had no clarifying comments per se from yesterday, so we look forward to stimulating discussion continuing on this model today and we would like to thank the panel for all their hard work and very extensive look into this system, thank you.

DR. HEERINGA: Thank you very much, Mr. Dawson. At this point in time I think we are -- we covered questions 1 and 2 at the end of yesterday's session and worked a turn to question number 3, so if I could ask Mr. Dawson to read question number 3 for the panel.

MR. DAWSON: This is a three-slide question, so it's going to be a while. The determination of appropriate flux and emission rates is critical to the proper use of the FEMS model as these values define the source of fumigants in the air that can lead to exposures. There are different methods of determining flux and emission rates from empirical data including direct measurements and was referred to as the "indirect" or "back-calculation" method. Direct measurement of flux is not that common and the available data because of the difficulties and expense associated with generating these types of data. The "indirect" method is most commonly used and involves fitting monitoring data with ISC to determine flux and emission rates. 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.

A. The emission fitting procedures used in FEMS are based on least squares analyses of log-transformed, dispersion modeling and fielding monitoring data. What, if any refinements are needed for this process?

B. Is it appropriate to log transform these types of data for back-calculation purposes and to use a least-squares regression analysis which implicitly assumes that the fitted line passes through the origin?

C. How appropriate is it to use a flux or emission factor from a single monitoring study -- excuse me -- (or small number of studies) and apply it to different situations such as for the same crop in a different region of the country?

D. Does the panel believe that FEMS could adequately consider multiple, linked application events as well as single source scenarios?

E. Does FEMS appropriately address situations where data are missing (for example is the data filling procedure appropriate)?

F. Should there be a threshold r2 value below which a regression of measured versus modeled air concentrations should not be used in flux rate determinations?

G. What are possible alternative approaches?

DR. HEERINGA: Thank you very much, Mr. Dawson. And our lead discussant for our panel on this question is Dr. Spicer.

DR. SPICER: This is Tom Spicer, University of Arkansas. With regard to questions A and B I essentially lumped my comments together on those. The issue of whether your treat the data in a log-transformed fashion or whether you deal simple with raw concentrations has been, both those cases have been made, the report dealing specifically with the log-transform the comments made previously to the panel that dealing with raw concentrations actually is more effective in dealing with the material balance issues is taken as well, so that debate I will leave to my colleagues who are more learned in statistics than I, however, there are some observations.

The first is that the log-transformation will tend to emphasize the lower concentrations especially since the 0 concentrations have been set to the lower detectable limit over 2. That's particularly evident when you start looking at some the scatter plots that there are large numbers of these values, and so this question -- and this may have already been dealt with, I'm not certain -- but the question is what effect might one have if one simply leaves off the 0 values of concentration in looking at these. My guess is that it won't make much difference, but it may make some.

The second thing is to consider this idea of changing the flux basis to see if (inaudible) of this log-transformation is significant, that was discussed by Dr. Baker and myself. Essentially the idea is, and I believe that this has been agreed to, that you simply change from one microgram per square meter per second to a different value and see the if the scaling values change significantly.

Once again, my guess is there may be some difference, but maybe not a significant difference and therefore this log-transformation versus raw concentration may be not be an issue for this particular set of data, that's not to say that it's not an issue in other sets of data. The other question that's involved in this is with regard to the origin and once again whether the origin should be forced through 0 or not. The comments I made previously are that I believe that the measured concentrations that are non-0 being predicted is 0 by the model are simply problems associated with the model predictions.

So that I believe is the issue there and that gets into this question of whether the model is actually predicting values that it should, and I will talk more about that in a minute. With regard to the first A and B, there's also this question of concentration measurements, the measurements in the report were based on four-hour samples and it's been indicated that six-hour samples are being considered. Six-hour samples would smooth the concentrations even more and throw out the variability that is evident in the meteorological conditions and measurements.

If you take that argument to the extreme and take six hours to twelve hours to twenty-four then indeed you would smooth out the concentrations as well, but you also would be throwing away the information associated with the high flux rates and therefore quite likely not predicting the exclusion zones of the buffer zones properly. And so I believe personally that that's moving in the wrong direction even though it would make the measurements easier to deal with I don't believe that it's a valid way of actually trying to access what the hazards might be.

The second point is that it seems that the present practice is to move to taking concentration measurements at 150 meter samples, that's probably 150 meters from the center of the field, from the edge of the field, and so that makes the situation even better. At 150 meters one might expect the vertical distributions to be less sensitive to this issue of height and that's fair enough, the only problem is that when you go out to that long distance that instead -- you do gain the information from the -- not having to worry about the vertical distribution, but the problem that you have with a limited number of samples is of course you can have situations where you have only two or three samplers getting data and the net result is then that you're actually losing information in that direction. It seems that unfortunately there is this trade-off and it's also a trade-off of course associated with cost, you can increase the number of samples and get better information, but that always does cost more.

It's been pointed out that this indirect method is essentially a way of calibration of the model to the fluxes and that's possible and we discussed this as well if the atmospheric stability were a continuous function, in reality of course the atmospheric stability has the effect of a step-function on the dispersion predictions. Even if you have destability meteorology determined in two sequential periods in some of this test data that does not mean that you will have the same values of sigma Y and sigma Z, the dispersion coefficients in the field.

In other words, even though the atmospheric stability may be the same the dispersion coefficients may be different in reality and of course the model does not reflect that, and so that's part of the issue with why this is not really a calibration to the model fluxes. If the model would indeed have a way of predicting the dispersion coefficients in a continuos fashion then quite possibly this would be improved, but as it is that simply doesn't seem to work very well.

There's also this possibility of misdiagnosing the stability changes especially during the weather conditions that we're trying to monitor.

One of the most difficult situations here is exactly at the end of the day when the sun is going down and the stability is changing rapidly as reported out in the presentation I believe that you looked at that sunrise situation in going from stable to unstable cases, but the same thing could apply in terms of going from the daytime stability to the nighttime stability categories, and I've taken some figures out of the report if I could have those at this point.

This is figure 19 from the report and what I've done here is simply taken the figure and in the blue box at the top there I've simply outlined the seven largest concentrations. As you can see the fit to the data does seem to have -- does seem to not reflect those values at all and yet those are the highest measured concentrations. Now the model would tell you that everything being equal the highest concentrations will be for the lowest stability classes, okay, that's what the model would tell you.

If you look at the next slide it's quite easy to identify those seven concentrations because once again they're the largest ones, you see that they

occur at the lowest wind speeds, once again consistent with this idea of being the lowest stability classes, okay. But if you look at the next line you can see that they actually end up being categorized as D and E stability as opposed to the F stability.

So my point is that the meteorological information that we have may simply be missing the fact that we're in these transition zones and giving the model even incorrect information, so we got two issues, not just the fact that the model has the step-change problems, but also the fact that we may not be getting accurate information because of these transition periods, and that's just simply a consequence of the fact that we're trying to make measurements and characterize the atmosphere in a very difficult time in which to do so. Thank you.

DR. HEERINGA: Thank you, Dr. Spicer. Mr. Sullivan, you have a comment?

MR. SULLIVAN: I would like to comment. I agree with you in the sense that during that transitionary period the step functions can be incorrect, I mean it can say D and it could be E, possibly F. We do have sigma W data from these field studies and it would be possible to interpret that and consider it you know through even AERMOD to see if that would show a situation where it would be getting into a stable regime and missed by Pascal Gifford (ph). The study it showed was chemigation intermittent sealing and I agree those maxes could occur right near the transition time.

The emissions situation there was such that the nighttime, as the nighttime was coming on of course the seals were getting put on at that point in time too, so the emissions were being greatly reduced, but that aside it is possible during that transition that there were artifacts that could have been detected with the sigma W data, so that can be looked at.

DR. SPICER: And once again these are the sorts of differences between the model predictions and the observations that I believe may lead to these issues of whether the line should pass through the origin or not when fitting the measured concentration at the predicted concentration. As far as question C is concerned, there's a simple issue associated with that and that is that this methodology seems to limit the application to the hour within sunrise simply by using a single study where that was the prescriptive method. I mean obviously in a practical sense although you'd want to make this application near sunrise it may not happen all the time, so to me it's not necessarily a direct consequence that sunrise would be the worst time for application, it may be that there are other complicating factors that would cause a delayed application to actually have a high buffer -- resquare a higher buffer zone.

It was pointed out yesterday that the time smoothing is not mass-conservative, of course there are still lingering questions with this compound with regard to the effect of degradation and the simple issue of how much is the total amount that's omitted as compared to the application of the chemical. So those are open questions and so of course the conservative approach would be to assume that none of the material is degraded, that could be extremely conservative but then although that may be going to the extreme in one direction, it may the fact that you only have for example 21 percent of the compound recovered in the downwind flux in the material may indicate that you're not getting the flux measurements correct, so those are still just open questions.

The report makes the point that the temperature is important as far as this processed is concerned and therefore the Bakersfield meteorology is conservative and then relies on using a single use of the Bakersfield site as a characterization of a release, but the obvious question then to me is why not have replicates of releases at the Bakersfield site. That of course then would force you into the question of saying how do I deal with multiple sets of data, flux data, when the model takes single data input into the program, and so that's the question that seems to be unresolved is how do you actually treat multiple data sets. And that question was deemed mute by the report, but I would suggest that that is simply not the case.

The other point is that of course, and it has been discussed, that use of the Bakersfield data in

other locations may not be beneficial because it may be overly conservative. There is a constraint apparently in FEMS that the mass, that the total mass be not omitted in the first four hours once the variability of the source is taken into account. In other words, once they take the data from the field, the flux data from the field, do the smoothing to it and then generate the probability distributions from it and you get these nice curves with a two and a half percent to ninety-seven and a half percent that the ninety-seven and a half percent value can actually dictate that the total mass is at omitted in the first four hours, or actually more than the total mass is omitted in the first four hours. The code actually says we're going to limited that to the total mass being omitted in the first four hours.

Obviously that's a difficulty because we know from the data that the mass is, total mass is omitted over the first four days and so there does seem to be an issue there about this maybe being too conservative. But the problem is that because of the fact that these are probabilities and this is

predicting the ninety-seven and a half percent level you also have to consider the two and a half percent level, now just see that the pans (ph) reasoning would dictate that when you actually draw the two and a half percent level that you will be significantly and physically underpredicting the amount of material that goes downwind in the first four hours exactly when it's most critical as far as predicting the buffer zones are concerned, so therefore that's an issue that seems to me is extremely important.

Furthermore, if you actually draw that two and a half percent level or some below level that's unphysically low then that would mean since there is no constraint on the total mass that you would be predicting quite possibly that a very small percentage of the mass goes down field since the first period, the first four-hour period is the most important as far as the emission of the mass, and so therefore you may be significantly and unphysically underpredicting the buffer zone that's required.

Dr. Yates has pointed out yesterday that these single-rogue values when you have what might

seem do be an outlier is an influence when you're looking at a single set of emission data and that's a point that's well taken. Of course that situation becomes worse when you move from the four-hour averages to the six-hour averages. If you have a bad data point then there is less collaboration when you move to the longer averaging times.

And Dr. Seiber has pointed out that validation, I believe yesterday afternoon, that validation methods are extremely important for this, so there is definite need for looking at alternative methods or at least some sort of validation method even though the indirect method may be the primary source of the information.

With regard to question D, whether this would take into account multiple sources or not I believe that this seems possible given the methodology and the fact that essentially two tools that are already -- have some sort, some level of acceptance by EPA, it may be although it seems possible to take these multiple sources into account it may not be easily done and furthermore it may be easily done

incorrectly.

With regard to the threshold or squared levels I will leave that to my colleagues that are more familiar with statistics than I. Well I guess I did want to say something about question E, the effect of messing data. It was pointed out in the report there was one data set that was complete and what I would suggest as far as evaluating that is concerned is taking the complete data set, running it through the process, and then selectively leaving out information from that data set that's complete to test what the influence of those holes may be. And I believe I've discussed possible alternative approaches as well, so that concludes my comments.

DR. HEERINGA: Thank you very much, Dr. Spicer. And the next discussant is Dr. Wang.

DR. WANG: Let's see what I can fill the gaps that has been talked about. The first one as far as on any refinements that can be pro added to the process of going through the log-transformation dispersion modeling and then field data, I think a

good suggestion was made yesterday by Dr. Yates, basically that a flux rate likely can be predicted from concentration measurements over time at one single monitoring location since for the field experiments you are setting up monitoring locations around the fields, so you likely can derive set, a whole set of the fluxes and then that would provide you an estimation of the variation in terms of fluxes using this indirect methods.

The other thing that has been touched on about the time smoothing by changing the duration of sampling, it's not a good idea if you want to know, it's a mechanics in the emission process since you are going to miss a lot of data, especially if the application is made before the transition of stability effects either early in the morning or late in the afternoon, so the highest peak will have to go through the transition period for the uncertain stability, so you may miss some of the important processes.

And the other factor I think you may consider in terms of deciding the duration in your measurement processes would be the exposure times for the

potential receptors, if it's either humans or other receptors, and again those probably need to rely back on the toxicology studies if it's one- or four-hour exposure appears to be pretty lethal then you may not to be able to use your measurement time longer than that.

On the second question B, whether log-transformation is appropriate and other related issues, I think it's probably a good idea to log-transform, in the measurements we do see the concentration data can go from zero to very large values, so it will probably fit into a log-type of distribution, but the suggestion I would like to make is that there are large data bases from DPR mentioned before and from your studies and others and the literature, so you probably can pull out those large data sets and do some histograms and do some normality test, and if you're not you may do a log-transform or it may not be log, it may be other distributions that you can use to make the transformation to meet the normality requirements before you can do the further analysis.

Question C whether a single study can be used in other situations and in other regions, I would say it is probably not a good idea, but the main reason is that it's a micrometeorological properties is going to be different as a fact from one situation to another, it's just how different, especially in those key parameters we have talked about in air temperature, the wind speed and direction, so those -- if there's a way that you could incorporate in the model to consider those local effects then maybe you can, but how to do that is yet to be seen.

Usually without an experimental validation to commence the local stakeholders it's probably hard for most people to say here's a model, use that to help you decide something, most people probably won't accept that.

On question D, does the FEMS model adequately consider multiple linked application events as well as the single source, I think whether FEMS model can do that it depends largely on the capability of the C model since it's basically an extension of that, dispersion component depends on the C model, since I'm not that familiar with the C model, but it does appear to have the capability and I would think that's probably a much more important factor to include. In most cases, at least in the practical commercial standpoint is probably rarely you apply a small field, usually for commercial applicator to come to a region they apply to a cluster of different locations so that may be a higher priority than just looking at the one single field for your modeling purposes.

And question E, I'm not exactly sure if missing data is an issue in here, so since you're -- either you'll miss concentration measurements you probably can use the remaining data to your dispersion estimation so that may not me applicable in this case.

Question F, you were asking about the r2 values whether to use a threshold of standard model to assess your model predictions, I think overall it's a good idea because if you are to apply the model to different situations, recollect different scenarios, how do you know, how do you compare one from another, so it's probably a good idea to have some sort of a standard size r2 say .9 or if it's that's that much

(inaudible) probably you have to say .75 or something, it may be arbitrary but still it provides some comparison between locations to bring to the same level of competence you may say.

And especially while you're doing different locations there also may be differences in the application methods, different kind of service (inaudible). So with a common value in the requirements for good fit that probably a good idea to have.

The last question you were asking about the possible alternatives for this, again, Dr. Seiber mentioned yesterday that we, likely you could use -- do some validation, of course a direct field measurements is ideal, but it is again, it's costly, it's expensive, so laborious to do, but some predictions with (inaudible) space models and that's not necessarily requires any measurements, actually a lot of the (inaudible) data the fumigant transport parameters are available, I mean you just plug it in and give you something that may not be exactly the same, it but probably has quite a bit of confidence in

that predictions in terms of the fluxes that you're going to be able to estimate from this black box indirect method.

And along the same line I think another alternative to do this is probably use the dispersion model forward rather than doing it backward, meaning you use this type of solace (ph) model to predict the fluxes and then plug into some sort of a dispersion model, it may not be (inaudible) anymore since you already know the source strengths, so I'm not sure about air model can do that since you basically using the fluxes input in this case and using (inaudible) as a way to drive the defusion process, so rather than going backward. In that case you don't really need the air concentration outside anymore since usually you are predicting the flux coming out of the source you know the (inaudible) conditions so you can project which way they are going, how far they are going, so that's a direct approach, that's all, thanks.

DR. HEERINGA: Thank you, Dr. Wang. Next discussant is Dr. Winegar.

DR. WINEGAR: Erik Winegar. Question 3 in general the big picture question appears to be essentially asking about the indirect method of flux determination and fitting that in with the model, et cetera, and I've stated previously my opinion that the indirect method is probably about the best thing we have in our toolbox right now to get this kind of information, but there are some questions that are lingering in my mind particularly was we are looking into this a little bit more in depth.

Some of things relating to the fitting of the equations, et cetera, raise some certainly in my mind about the procedure particularly as we look at the scatter of the data and the quality of the fit between the two parameters, the correlation coefficient or I guess is actually coefficient of determination technically, is that right, are squared. When I look at a couple of those plots I see that they're on the order of .5 and .6 in the report and I don't know if there's any others that you didn't reproduce.

I keep coming back do my experience and training in the chemistry lab and where we routinely look at r2, people start throwing things out if it's below .995. And I recall my p-chem professor saying you're not doing p-chem if you don't have an r2 greater than .9. I know we're not doing p-chem here.

MR. SPEAKER: Well usually in the air quality of business you have an r2 of .5 you're really happy. The atmosphere has uncertainty and I guess our thought was that by incorporating that uncertainty we can you know do the best job in terms of representing exposure, but it is pretty hard to get r2's consistently much high than that when using measured data and modeled data in the real atmosphere, it's kind of a turbulent stochastic process that usually works out that way whether it be air quality of meteorological research that that's pretty typical.

DR. WINEGAR: Well I can appreciate that, it's a touch situation. I was wondering if anybody had a feel for, particularly maybe EPR folks, and all of these methobromide (ph) in direct flux studies what range of correlation we are looking at you saw in those studies in general, you quoted the 30 mid-odd (ph) studies.

DR. HEERINGA: Dr. Barry?

DR. BARRY: Terry Barry, DPR. It was

everything from about .5 to as good as .95. We had some really good fits and then when we had some not-so-great fits and when we had the not-so-great fits is when we had to start figuring out what to do next, and part of it is low wind conditions and meander and things like that. But in general anywhere from .5 to .95, and most of them were .75 / .8 if the design was well designed and the weather behaved.

DR. WINEGAR: I mean my personal gut feeling is that you start I to get below .75 it's -- I get uncomfortable and maybe it's just I'm not exposed as much as others are working in this of seeing, you know dealing with the lower r2 values, et cetera.

MR. SULLIVAN: It also would depend upon the averaging time that's being used, and this is four-hour averaging. As you go to longer averaging you probably wouldn't get your r2's get any better too, so it's that trade-off between getting the coverage as Dr. Spicer appropriately pointed out versus the r2, there is a balance there you try to meet.

DR. WINEGAR: Indeed and I appreciated Dr.

Spicer's comments, there's some very cogent, incisive thought that went into his comments I thought in regards to these averaging periods and how that relates to variability and such. It seems to me there should be additional investigation into that whole question so that we can maybe try to minimize some of the these sources of variability and understand a little bit better.

The mention has been made in regards to conducting additional studies to try and I don't know compare and calibrate the indirect method by comparison for example of the direct flux method, the radiant methods and things like that, I mean it's easy to say why dont you do this and that, it's tougher to get it down, et cetera, but nevertheless it seems like the stakes are fairly high here in terms of future use of this, so I would repeat that suggestion that I think some additional validation work should be done particularly when I agree with Dr. Spicer's comments in regards to that there's -- I just got a feeling that you know I think it's a good idea to start out at

fair level of conservatism, it seems that the some of the gaps in the knowledge and understanding are leaning a little bit to an over-conservatism as has been stated.

Again I agree with the comments in regards to the chemistry and degradation issues of metam as it goes into the soil and converts MITC and then so it's kind of a pretty big article of faith that you're saying X pounds is applied and Y pounds are coming out or Y fraction, whatever, is being admitted, that's kind of a black box that while I have a fairly high tolerance for black boxes in many situations, this one I'm feeling uneasy about and so I think a little more study in that regard would be useful to try and eliminate some of these vague areas.

In regards to some of the specific questions, I think lot of my comments have kind of addressed my concern. The log-transform issue I mean I'm still -- some of the previous discussion about that the log-transform is not appropriate is still kind of ringing in my mind and I haven't really heard a good resolution of that issue, but I do recall seeing in

the literature and statistics test all over the place that log-transforms and the appropriate back transform to original units is appropriate, so I don't know about this argument that it's not a physical thing that -- physically appropriate thing to do, so I'm tending towards feeling just fine.

MR. SULLIVAN: I could add that I did do some homework last night and I went back to that scatter plot that I created and just as a very crude review of mass balance I added up all the micrograms per cubic meter for the measured data did the same -- that's 240 numbers, 24 periods times -- more than that -- 24 periods at 15 sites, I did the same thing for the DPR approach which exactly, almost exactly matched the measured data, for the log-normal fit it was 15 percent lower, but that was based upon using the best-fit values and askewed distribution, so if anything if there's a mass balance it may be a little bit high, but probably is not too much off, so that was encouraging, we can do that for the other three GLP studies and see, but so far that would appear to suggest that it's not under estimating mass, but I'm

open to any ideas and additional testing that could be done to confirm that this assumption if you run it through the system and do random sampling is it matching what we're seeing, any statistical tests that could be done to better confirm that I would certainly happy to run those and assess what that looks like, but so far it looks like mass balance doesn't appear to be a problem in that one data set.

DR. WINEGAR: That's good. I guess my last comment in regard to the log-transform is that I tend to rely also on the advice of statistics, texts, et cetera, I talked about looking at the distribution of your data and dealing with it appropriately and it's a log-normal distribution we should apply the appropriate statistical analyses to that distribution.

Question C in regards to the number of monitoring studies, I have expressed previously my concern about the application of studies in just one area to other geographic sites. There was a lot of discussion in the previous model in regards to the use of a single meteorological or a number of meteorological data sets and applied across the board

and the same argument can be made here in regards to the flux measurements. Again my concern about the chemistry, the metam-sodium MITC chemistry in different types of soils coupled with the meteorological aspects of different areas of the country, I have some concern about a wholesale application of one particular type, plus as I look in your studies you have, each of your studies is a different type of application method which of course has a different flux characteristic and so it seems like a pretty large extrapolation from those relatively few studies to very diverse geographical sites.

MR. SULLIVAN: If I could offer some clarification, in terms of metam-sodium conversion issues, the conversion rates and amounts in that kind of a condition with a hot soil such as Karen (ph) County low carbon content and so forth, the conversion is quite rapid so in that situation you would expect within an hour or something like that to have converted 90 percent or so, and the lab studies that have been done on that show about 90 percent

conversion is what you tend to get from the metam-sodium, so as you went to other regions with more organic matter in the soil, cooler temperatures, especially much cooler temperatures, that conversion was slow released, you would expect lower impact, so your point is well taken, it is always comfortable to have additional studies, the approach was to do rapid conversion -- you would have maximum effects and then it's up to the manufacturer if they want to do additional testing in heavier soils where there would be less of an issue with off-gassing they could do that, but that was the rationale for that part.

In terms of the variability of meteorological data, I agree with you, one meteorological station cannot represent an entire region and I think that the most appropriate way to deal with that would be to put in multiple met-data sets, multiple five-year data sets into the run, and again you get 50 years of real data to represent the valleys, the coastal areas, and whatever you wanted to represent for that region and then cover the full-range of variability, because again the field studies were designed just to get the

emission rates, but to characterize the variability and uncertainty in emission rate we need to open that up to not try to find a representative station for a region, but rather put in five or ten data sets to represent that region and then I think you could make the case fairly well that we have reasonably represented what you expect to see in that entire region, whether it be the San Wakeen (ph) Valley, whether it be Florida, whatever region you want to pick, I think that would cover the variability that way.

DR. WINEGAR: If you recall and I think it's attachment 6 lists a whole number of studies that have been about MITC and different types of flux measurements, most of them don't appear to be the full-modeling combination flux measurements; is that correct?

MR. SULLIVAN: Most of them aren't -- now some of them are, DPR and ARB have conducted some studies that were listed there that you know could be used and I guess have been used to develop flux values, some have not been, but the issue that that is a fairly large data base, in some cases that represents nighttime applications, the ones I'm describing here represent daytime applications, but that data base certainly is available for historical purposes, some of those studies still would be applicable to different situations today.

For example, at study done in `93 by DPR on nighttime off-gassing, it does provide data that would characterize that, the data we have presented here characterizes the daytime conditions.

DR. WINEGAR: It seems like it would be a worthwhile exercise to extract from that data set in combination with the DPR to apply this whole system to see if maybe that would address some of these issues and the validation realm.

I think the remainder of the specific questions I've pretty much addressed in my other comments.

DR. HEERINGA: Thank you, Dr. Winegar. Dr. Bartlett.

DR. BARTLETT: Yes, I found the previous

comments very, very elucidating and some of your responses, let me see if I can rethink this and go back to ABCs. Let's see, on the issue of the general issue of log-transforms, I believe it probably does fit the data better and my experience using least square that log-transforms myself I found to be a better fit for this type of situation, so that makes sense.

I think, I don't know how -- I agree that it would be useful to look at other data sets as was suggested before to see if that's consistent because in some sense we have one sampling situation and we may be -- you could go too far in trying to fit everything to this one sample and this goes back to this one experiment station, and that there may be some outliers or some certain data points that are leading you to this situation that may be unique to where you happen to put the sampling stations, how the plume happened to move, all those types of things, but in general I feel comfortable with the log-transforms.

As far as the method itself I understand that that's probably the best field tested technique that we have at this time. It is one of the things that keeps coming up is with experience with soil model ing and also even with meteorology I found with topographic and terrain features that a multi-variant analysis can improve understanding of dispersion because in some sense you have when you're back calculating you're not using a dispersion model per se using this from what I understand, maybe I misunderstand, but by using this technique you're not -- or am I wrong?

MR. SULLIVAN: We are doing the least squares based upon normalized modeling with ISC, so we are normalizing to one microgram per (inaudible) per second and then doing the least square analysis using that as the X term and the measured data as the Y term.

DR. BARTLETT: So the ISC dispersion model is used for the back calculations with, in conjunction with the least squares, okay.

MR. SULLIVAN: Right.

DR. BARTLETT: I guess what puzzles me about this method using that is that it seems like it would be behave better in certain situations of dispersion

you get a higher r2 then in other situations in actual emissions and behavior maybe this method performs better under certain scenarios than others that's from -- I haven't seen the other data sets but it seems like if you explored those other data sets you'd find meteorological conditions and the movement of the plume that might explain the influence of -- I mean you would expect certain zeros to be outside of the plume and other movements and other situations to have wide variations of measurements which would affect the technique of a linear regression, but that's just my impression of that.

So getting back that leads to the r2 so also working more on meteorology I'm more happy with .5 and if I see a .95 I would suspect somebody's cheating. When you overfit a method there's something else that goes on, so actually going back to I guess point C is that it is to me that since you're trying to get at the variation of emissions to some extent which there's a measurement uncertainty and there's also real variation in emissions that we have get yet to satisfactorily model with soil models in the real

world that having another similar study would get a better idea of emission even if it is a different situation, then the question, I mean different meteorological conditions which in some sense you can control for, but then the question comes up with the desire to have different conditions, what soil conditions and other conditions that we know that influence emissions to some extent which actually in this technique of intermittent sealant may be less applicable, and this is one of the difficulties I think of using soil modeling that has been done so far is because you're using a different method and how you would adjust your soil modeling to deal with that is problematic to me, so at the first instance I think I agree with Dr. Wang that we would want to look at soil modeling in those factors.

And in any case in these experimental data sets collecting that data is tremendously important because as more studies go down the road and more applications and even if some of the measurements may not be ideal we might get some idea of what those factors that may change, deal with some of the those

variations.

MR. SULLIVAN: What's interesting is the more recent studies, the second intermittent study, the chemigation study, we did have Dr. Hussein Ajwa who's a soil scientist participate with us in that study and one since then, and he has collected the soil information that would describe how the soil conditions do change during the intermittent sealing process, so the data is there, the model that Dr. Wang discussed could be used in conjunction with that chemigation data set and you could see how well it performs and what the limitations would be.

DR. BARTLETT: So can we go to the next set of questions?

The multiple linked application events, it does seem to me that this model can be used for that purpose which I think is important. The issue of missing data I'm going to cover in question 4 when we work with -- I'm not sure if -- I assume this is meteorological data, or is this also an input data, other input data that's --

MR. SULLIVAN: The thrust of the question was on the emission side. On the meteorological side we don't have much filling to do on the on-site program and also use the National Weather Service, the only feeling we really have to do is for mixing height which is in a sense a parameter anyway, so I think it's the emissions that's most critical and Dr. Portier's comments yesterday about the despondent interpretation (ph), that's something we should check.

DR. BARTLETT: Okay. Yeah that's enough.

DR. HEERINGA: Thank you, Dr. Bartlett. I think there may be some additional questions or comments from members of the panel, I know that Dr. Portier.

DR. PORTIER: I always have comments. Ken Portier. You know when you invite an academic on a panel you have to be prepared for a lecture or something. Just as I read the question I started thinking about you know what exactly are you asking, and on the first one you're asking about refinements to the least squares process, is the least squares process is, it really doesn't need refinement, it stands by itself, but you have to think about what it does, it's a very simple procedure that allows you to match the expectations from a model to observations and minimize the square differences and you get an estimate. The only problem comes when you want to do something with that estimate, you want to make an inference on it and that's when the second part comes in, when we start worrying about the log-transformation because to be able to say something about the slope parameter that you get to put confidence bounds, we have to make an assumption about the residual variability and that's where his presentation that showed the QQ plots that looked more normal on the log-transformation than on the unlogged transformation is more persuasive to a statistician because it says I can make this normality assumption under the log-transformations and I can put some confidence bounds on that parameter and then proceed to do the uncertainty analysis in that.

Now that begs the question of the mass balance issue and transformation and mass balance, but I think that just needs to be worked out, I think that's just a little bit of mathematics that we have to work through to figure out how to determine the mass balance issue under the log-transformation. The question of how appropriate is it to use flux emission factors for a single monitoring study, again here you start thinking about appropriate versus acceptable, you know you asked for appropriate and I think we've answered acceptable. Acceptable means if we do enough studies after a while we see patterns and we can decide that this model works in a lot of general situations.

Appropriate to me would say in our understanding of the mechanics and the chemistry and everything about gassing and flux movement can we move a model from one location to another location, in the strictest sense of the definition the answer there is no, it's not appropriate, but it's probably going to be acceptable for the uses we want to use these models for within some kind of regional of characterization of soils and agricultural use and terrain and everything else, so I guess we've been answering the question of acceptability and not really answering the appropriateness, but I think maybe a chemist and the soil physicist would tell you, no, it's not appropriate to move the models because we know there's going to be big differences in separate locations.

Under the multiple-length applications, my only issue here is I'm still not clear how that's actually implemented in the program, okay, I think it's a documentation issue with this model as well as with the previous model, it's kind of mentioned as an aside but I'd really like to see a more worked-out scenario, and I think from the panel point of view we probably would want to see that as more focus of a discussion at some later point because there's a lot of details in there and the details are the parts that kills us, generic sounds good, now show me how you're actually going to do it.

It's like how are you going to lay down the response grid and how are you going to collect data from this and a lot of issues there. I think that I'm going to pass on the data filling procedure because I really want to -- I think we need to talk about that when we talk about things like calms and everything else in the next question because that's kind of where that kind of data comes in, is there wind, is there not wind, did I not measure wind, all of that comes together in that issue. I think I'll stop there -- wait, no, one last thing.

On the r2, this something I deal with all the time, scientists always say you know what should be the right level of r2, r2 is an index of how good something fits and then we have a statistical test over here that tells us whether it's significant or not and we kind of tend to confuse the two issues. We want to say well it's .5 significant, the significance of a .5 r2 depends on how much data you've collected and the underlying variability, not on the value of the index, so in one situation .5 is not statistically significant and I don't look at it.

Another situation, if we had 20,000 observations r2 would be fantastic, right, incredibly significant, so you need to be thinking in terms of is the regression statistically significant which means there is some indication of true pattern here and then r2 tells me a little bit more about how uncertain am I about that pattern, so a .5 tells me that I'm not that certain about the pattern, there's a general direction, but it's probably not well constrained, but if you told me it was .5 with a P value with an M statistic of .0001 then I would say well there's something certainly happening here.

MR. SULLIVAN: Question, Dr. Portier, would the standard air be a criteria rather than the r2 for making that determination?

DR. PORTIER: Well, if you put the standard air in the r2 and the sample size together then I can tell you how significant it's going to be, those are kind of the three components I need. I think the r2 in a P value from the significance test are the two things that I tell my students have to go in their papers, you now that's what tells me, one tells me the statistical significance of the trends that you've observed, the other one tells me something about how strong and certain that trend's going to be, a .95 that's very significant is a law, right, you know it would be a chemical or a physical law because I can say almost every time it's going to go exactly like this, .5 it's worse.

DR. HEERINGA: Dr. Majewski.

DR. MAJEWSKI: I would like to make a comment on the acceptable versus appropriate comment of Dr. Portier, I agree that extending this model to other regions isn't appropriate and although it may be acceptable from a regulatory point of view I can almost guarantee it won't not be acceptable from the farmers that are being regulated by this. And they would have a just cause I think because this model was developed for the worst-case situation and there are regional differences in meteorology and soil characteristics and moisture regimes.

As we've seen the soil moisture plays a big part on how this MITC dissipates, and if you use the results and the buffers, the resulting buffers based on the worst-case sandy soil situation I think you are going to over-estimate in other areas, most other areas I would guess.

DR. HEERINGA: Yes, Mr. Dawson.

MR. DAWSON: I would just like to make a quick kind of clarifying comment about our process with regard to this issue because it seems like

listening to the conversation that I'm getting the sense that the basic methodologies that are included here seem appropriate and that as we go through our regulatory process and we involve our decision making and the basis for it that the thing that is needed you know as we begin to look at other regions and look at issues related to say let's certain types of agriculture in certain parts of the country we're going to need to you know looking at getting additional kind of monitoring data and evaluating different sources of weather data, so that's something that's built into our process and as we go along those are all kinds of issues that we'll be thinking about and it's kind of a no reasonable offer refused approach from our perspective, if there's additional data we're going to consider it in the process.

DR. HEERINGA: Thank you very much, Mr. Dawson, that helps a lot. At this point -- Steve Heeringa -- I would like to make one comment or -- Dr. Yates.

DR. YATES: Scott Yates, Riverside. One

thing that kind of I thought that occurred to me in the discussion that we just went through, when it comes to the flux measurement it seems to me that the indirect method has a potential to produce fluxes that are biased, and the reason for this is that the measurements depends on concentration, so if the concentration is bias, the flux measurement would be bias, and concentrations are you know often times when you do the sampling and the analysis you may have like extraction efficiency problems or a variety of things can happen that could cause the concentration to systemically be high or low, although generally I think it would probably be low.

Turning the to direct flux methods, in particular the aerodynamic radiant method, if you do have concentrations that are biased when you calculate the gradient, you do a subtraction which subtracts out the bias, so it seems to me that in developing the methodology there should be, careful attention should be paid to running experiments that use the indirect method with a aerodynamic radiant method to look for situations where bias is occurring. And as you go between different chemicals this potential bias could

be different since the analytical techniques are different, so anyway I just throw that out there as kind of a comment.

DR. HEERINGA: Dr. Baker.

DR. BAKER: Just a small point of clarification maybe just for my benefit, but on question 3C we've been focusing on the flux, entirely on the flux side of the issue and I agree with the contention that Bakersfield and the arguments put forth supporting the convention Bakersfield represents a high, probably one of the highest flux cases.

We really haven't talked about the water sealing, is water sealing equally effective across all soil types.

MR. SULLIVAN: We have tested it in clay loam and sandy loam, so far in those conditions it was effective, it may not needed in all locations, it probably wouldn't be, if you were in a cooler temperature situation or a heavier soil, may not be a need to do it, but no it has not been tested beyond the two, the intermittent sealing shank test described, the chemigation shank test, and the two

follow-up studies in Bakersfield funded by USDA in

2002, and so there are four studies of that intermittent sealing concept.

DR. HEERINGA: Dr. Yates.

DR. YATES: Just a follow-up on that, a water seal is effective because it plugs the soil porous and so in terms of thinking of it from soil physics it's effective as long as the water can remain at the place its applied and you have a wicking effect in soils and that depends on soil type and you know what the water distribution is in the soil, so basically what you're looking for is soils that don't wick the water away as quickly they'll have water seals that will last longer and they'll be more effective.

DR. HEERINGA: Dr. Wang.

DR. WANG: I want to revisit log normal a little bit. The data shows a lot of zero values, so you can't really log zeros, so what that means memory is that change zeros to .1 or .01 or something -- the transformation, how did you deal with that data set?

MR. SULLIVAN: Since the detection limit of the measured day was 0.2 we took half the (inaudible) as the surrogate for that, we did the same thing on the model side, if it was zero we put .1 and as I showed we did add a .75 just to avoid near zero anomalies to the data, that was one that was described in that reference that was copied and given out yesterday.

DR. WANG: I'm not a statistician either, but I wonder if there are better ways to do that, deal with that large pool of data of zero, but just a comment.

MR. SULLIVAN: Yeah, I'm open to suggestions and I did try using zero and .5 and one various constant and looked at the fourth major studies that we had and the .75 seemed to be the most effective across the board, but if there's another and better way to do that I would like to hear that part.

DR. HEERINGA: In our report minutes for this session and the past session and I would say for the future session we will be sure to clarify our best judgment on that issue.

Dr. Shokes, did you have a comment?

DR. SHOKES: I was just going to mention about the soil, there are some soils that like some of real course sands that they have in Florida, some of those things are bottomless and they're not wicked at all and they -- I don't think water seal will do much good in that situation.

In our situation in Southern Virginia we don't put any kind of water seal on because typically we fumigate when there's good soil moisture and use a mechanical seal and that's adequate, in fact if we sealed it very much we'd have a difficult time getting rid of the fumigant, I guess it would have to biodegrade to get rid of it, so there is a lot of different situations and, yeah, the model needs to be tested on all those different regimes.

DR. HEERINGA: Thank you very much, Dr. Shokes. Dr. Winegar?

DR. WINEGAR: Yeah, back to the regression question one point of clarification, mention had been made previously in regards to sorting versus not sorting the data, how do you do it, sort that?

MR. SULLIVAN: We do not sort the data, I mean we keep, the measured and modeled data are kept matched, so with the regression space upon the matched pairs of sites.

DR. WINEGAR: Yeah, the other model had talked about how sorting appeared to have improved the fit, do you agree with that or have you tried that?

MR. SULLIVAN: Well I agree that if you sort the data and rank both from low to high that they'll automatically improve the regression equation, and I understand the reason why and it's true that the models do a better job of identifying the maximum where it occurred and they do in exact locations.

Just to be honest, I felt uncomfortable in sorting like that because I felt that I could sort any distribution that way, two variables that way and get a correlation and I wasn't sure what it really meant.

DR. HEERINGA: I would like to in the interest of time move onto question 4, Steve Heeringa here, I have one comment with regard to the minutes of this report. There is a question that lingers from the past session about the reference to the use of a linear regression fit as opposed to linear on the log scale as being more appropriate for the underlying physical model and that was Dr. Small's comment and we will be sure in this report and in the previous report to clarify that interpretation vis-a-vis the comments that have been made here.

MR. SULLIVAN: Thank you very much.

DR. HEERINGA: I had just one observation that I had with regard to, I think the physical model relates as Dr. Small I think was referring to is the fact that in the Gosien (ph) model concentrations are supposed to be a linear function of distance from the point source, is that not correct?

DR. SPICER: Linear function of the emission rate from --

DR. HEERINGA: Linear function of the emission rate, yeah, and I think that was his tie to this linearity notion and one of the things that we have here at least as I observe it as sampler is that you sort of arbitrarily set sampling points at specific distances so the concentrations you see in the data are not going to necessarily -- the sample data itself, its distribution itself could be different from the overall population distribution of concentration points too.

And one other comment which I think may come up later too is the issue that if you're trying to back-fit the flux rate from using a model which only allows a certain profile for those emissions and you're sampling outside the cone or that profile of those emissions are you not sort of fishing outside the river, I mean the model could never bring those points back into confirmation and I just wonder whether those are really valid points to be included in the model fit.

I recognize they represent the real emission situation there, but in terms of the calibration where you know that the model itself, you're calibrating with this model and the model can only produce a certain range of outcomes within a certain model profile and if you're outside that profile should we be using those data points for this calibration effort.

MR. SULLIVAN: I wondered the same thing and that's something that we would appreciate guidance on because for example, if we have 15 monitoring sites it may be that 8 of them are affected by the plume, 7 are not, and so in the least gross fitting do we ignore all those other -- would be better to ignore these other points, the extent of error would of course increase that way, in some of those points do you weight them, but I would appreciate any thoughts on that because that's something that we have dealt with and it needs to be resolved.

DR. HEERINGA: I believe that Dr. Yates suggested yesterday possibly looking at fits including different combination of points, one that I would certainly look at is those that would include points that sort of theoretically are outside at least the extremes of the plume of the model.

MR. SULLIVAN: And I think that's a good suggestion and one way that could be done is we do have minute-by-minute meteorological data, could look at the wind direction and identify that cone which would be different than the cone you see which is Gosien one-hour modeling and use those sites as a basis for the regression, that would be another way that might refine it a little bit more.

DR. HEERINGA: Thank you very much. Dr.

Bartlett?

DR. BARTLETT: Just to add something, that's the defect of the Gosien plum is that in reality the shapes of the plume are quite different and we've been experimenting with using scattered particles at random in a semi-random distributions, some are constrained and then tracking in a different way and the actual movement in a micro in that environment is much more complex in a Gosien plume and that's why I'm comfortable with the lower r2, because you would expect that to some extent, but I agree with Ken Portier that it's also because we are dealing with a small sample set.

DR. SPICER: Can I add one point?

DR. HEERINGA: Yes, Dr. Spicer.

DR. SPICER: Unfortunately I would suggest that simply following the wind direction may not necessarily follow the plume because you're sampling the wind direction at a different location than the plume may be, so as I was trying to suggest earlier in my comments I would simply leave out the concentration measurement stations that showed zero concentration.

MR. SULLIVAN: Whether the -- in some cases the measurement may show zero, the model may show zero, they both, would you leave out any matched, any pairs that had a zero in it, would it be measured a model?

DR. SPICER: No, I would leave any measured concentration in, but exclude and zero concentration measurement.

DR. HEERINGA: Thank you very much. At this point in time I guess if we could move onto question number 4.

MR. DAWSON: Question 4, The integration of actual time-base meteorological data into ISCST3 is one of the key components that separates the FEMS methodology from that being employed by the Agency in its current assessment. The Agency had identified several potential sources of these data including the National Weather Service, Federal Aviation Administration, California Irrigation Management Information System or CIMIS, and the Florida Automated

Weather Network or FAWN. The Agency is also aware that there are several approaches that can be used to process meteorological data and acknowledges that FEMS used PCRAMMET which is a standard Agency tool for this purpose. Upon its review of what meteorological data are available and how it can be processed for use in an assessment such as this, the Agency has identified a number of questions it would like the panel to consider.

A. The test case example in FEMS is based on te National Weather Service ASOS the meteorological monitoring station in the Fresno, California. What are the SAP's thoughts on the use of National Weather Service or Federal Aviation Administration meteorological data sets in comparison with either CIMIS or FAWN or this type of application?

B. What criteria should be used to identify meteorological regions for analysis and how should specific monitoring data be selected from within each region?

C. Anemometer sampling height has been identified as a concern by the Agency in preparation for this meeting. For example, some data are

collected at 2 meters while others are collected at a height of 10 meters. What are the potential impacts of using either type of data in an analysis of this nature?

D. FEMS uses "assumed distributions" to account for uncertainty in the meteorological data based on Hanna, 1998 and that was referenced in Dr. Sullivan's background paper, is this an appropriate technique?

E. Does FEMS treat stability class inputs appropriately, especially the quantitative manipulations of these data that have been completed?

F. Is the concurrent use of emissions and meteorological conditions in FEMS useful in identifying concurrent upper-end conditions that could lead to peak exposures for bounding exposure events?

DR. HEERINGA: Thank you very much. At this point our lead discussant is Mr. Barton.

DR. BARTLETT: For for question A, what are SAP's thoughts on the use of National Weather Service meteorological data sets in comparison with either

CIMIS or FAWN for this type of application. In this current application that we looked at they used the

National Weather Service data from Fresno and the advantage of the National Weather Service data is that they're available nationwide, they're consistent, have good quality control, and releases complete data sets.

These qualities make NWS data valuable for FEMS. A single consistent data source is important for comparability between meteorological regions, quality control is important since identification and understanding of the more uncommon tail meteorological events that result in the largest buffer zones are the objective of the FEMS project. Poor quality control could result in events that either underestimate or overestimate buffer zones through the anomalous data.

If data sources other than NWS use, the problems of data quality need to be identified. We basically need to know what those defects are and whether they would affecting these tail events. Regardless of the data source air and warning routines need to be written in the code that identifies impossible and unlikely met data and report the results, especially for -- you may have them

written-in already with flags but you may need to have something if somebody else uses the data set.

The FEMS methodology requires a complete met data sets. The FAWN data sets as Dr. Shokes and others reminded us in earlier session have a significant amount of missing data apparently due to equipment failure due to lightening strikes and other problems inherent to more specialized research stations. The FEMS methodology requires missing data to be replaced presumably by either in interpolation or reconstruction with climatic data in the aid of other nearby stations data, this can be done if alternatives do not exist in a region, but can be time consuming, it can conceivably create false weather events that either under-estimate or overestimate buffer zones, so care has to be done to that.

Sometimes when you interpolate you can result in longer periods of stagnant air that are unrealistic and other types of problems. The down side of a National Weather Service stations is that they are usually at airports, are not generally representative of rural farm areas. CIMIS stations are in rural areas but have other limitations. I have to defer to other people here for discussion of CIMIS stations.

There's two aspects of using the weather data from these stations, one is for ISC it's for the stability conditions for the experiments and the others of course application to an area and these projections and development of the idea of the -- the development and protection of buffer zones, so there's an inherent problem of using a station that's distance for stability conditions, but generally that's what we always have to do.

There are other alternatives that we'd used in regional analysis or long distance which is data sets prepared by NOAA Air Research Laboratories and I'm not sure how good they would be for these situations, but in general the problem of vertical mixing, the general problem isn't an issue for a very short range of transport, but it's been indicated before a defect of ISC is the step-function stability functions which can mischaracterize, so I don't see any way out of that but using AERMOD and then using more continuous variables, roughness sigma, all the

other types of stability conditions.

I did notice that you had vertical weather movement parameters on-site which is very important, but I'm not sure how much that Gosien plume analysis is able to take that into account.

MR. SULLIVAN: Once AERMOD is available and we could run it now, the sigma W, sigma (inaudible) data from all those on-site studies could be used to characterize stability and dispersement coefficients as continuous functions, so that's why we collected it thinking for the future in that sense, so that can be done, we were trying to wait for AERMOD to be officially released and are still waiting.

DR. BARTLETT: And I can appreciate that. But again to the tie-in to the monitoring periods is that the shorter periods are of value because of the rapid change, this weather that other people have been talking about which are not always captured by this weather data and that has been previously mentioned here.

MR. SULLIVAN: The sigma W really helps

address the point that Dr. Spicer raised because those (inaudible) are very sensitive and do show the turn-on/turn-off of turbulence pretty effectively. And to clarify all of the GOP studies we're describing here, what we have always done is set the meteorological station up on the same field, not the applied area of course of the field but on the same basic plot, generally within about a couple of hundred feet of the field, they're always flat, unobstructed areas so meteorologically those locations are very representative, so yes there can be differences interjectories of course, but in my judgment is pretty representative data set for emission fitting purposes for those studies.

DR. BARTLETT: And as far as the other application goes your suggestion that you made earlier to today is to use multiple data sets in an area, and I assume that's instead of using one five-year data set you would use multiple five-year data sets to construct your 200-plus years and I think that's a good idea, but care also as has to be done with some of these other less quality controlled data sets because then that could actually drive the tails, the

poor quality data in certain instances, so you obviously need to develop routines to spot that kind of data quality problem.

MR. SULLIVAN: I agree.

DR. BARTLETT: So question B, what criteria should be used to identify meteorological regions for analysis and how should specific monitoring data be selected from within each region, so meteorological regions should be chosen according to common climate, terrain, topography, and customary planning times.

The last item I'm not as familiar with all the applications, but you had mentioned seasonal analysis before and planning times, a lot of us have found are very much driven by the weather and the meteorology, the timing periods are a lot of the times very short windows in different regions. In work that I had done notice quite a bit of difference just between zip codes and how it follows, of course they're following weather forecasts a lot of times, but that is a subset of the weather data that is relevant to these events, so I think it makes sense to take some care to do some analysis of those seasonal sets.

But on the other hand the whole context of which we keep isn't always present during these discussions, if you're already working on an extreme case and are upper bound, so a lot of these comments that we're seeing actually may result in coming up with smaller buffer zones in these areas, these types of comments.

I also felt as supplement to that same thought that some MITC measurements down winds from some of these applications although not being a full-fledged study might be useful if you're doing certain applications with a few -- when prevailing winds are fairly obvious to take sample measurements different distances if you have a predicted buffer zone of 500 feet, 400, 600 feet to be out there in the field and confirming that as far as since these larger studies are so expensive you might get some ballpark ideas of whether this is working or not.

The question of the sampling height, question C, measurement of most data points of the type is important to consistently evaluate dispersion to

estimate the mission by the method of back calculation for ISC. The height of 1.5 to 2 meters has advantage of being human exposure in a rural area whereas 10 meters is more customary height for met data and is more representative of the regional conditions. Below 10 meters loco-micro (ph) meteorological events can prevail. It's preferable to have vertically resolved air concentration and to have meteorological data for 1.52 meters and 10 meters during the testing period, and basically you have weather stations at both levels during your emission testing which I think is the way to go, but as far as vertical further and as we mentioned earlier the use of sigma and the other types of data, so I think I will stop there with that.

Question D, FEMS uses "assumed distributions" to account for uncertainty in the meteorological data based on Hanna in 1998, is this an appropriate technique. The method has advantage of carrying through uncertainty for meteorological inputs to the results, the technique also allows the creation of two hundred years and more meteorological data sets out of five-year data sets in this instance from Fresno,

however there may be a drawback to the introduction or independent exogenous random disturbances at each time set, this may result in introducing unrealistic meteorological time series patterns, this is likely to be most consequential to boundary conditions in regards to wind direction when a low-speed prevailing wind can produce the boundary condition air concentrations, by varying the wind, this is a point I made earlier that this could result in diluting concentrations in a boundary position.

This can be evaluated as we mentioned earlier by conducting a comparative run with disturbances applied to wind direction turning it on and off so then you can see how much this is actually going to be affecting that region, it may or may not be significant. I also mentioned that, well, I don't know if it's appropriate here but in this particular section, but there has been 50-year data sets of weather data that's been provided for long distance and long-term forecasting and for climate and other types of research that are -- we're getting better and better data long-term data sets being reconstructed,

so they may be of interest of understanding variability by then of course there's quality control problems of switching, you know, station types over periods of time and you might spot patterns that have much more to do with the method of and measurement, method of measurements.

Question E, does FEMS treat stability class inputs appropriately especially the quantitative manipulations of these data that's been completed, according to the authors PCRAMMET an Agency approved method was used to construct the stability classes for use in ISCST3 from the Fresno WMWS station data, there's an inherent problem in using a met data source from a distance location, but it's not readily overcome. Replacement of ISC with AERMOD should allow for more customization to local stability conditions such roughness and in this discussed in earlier question, there's some overlap here.

The last question, is the concurrent use of emissions and meteorological conditions in FEMS useful in identifying concurrent upper-end conditions that could lead to peak exposure for bounding exposure events, and I believe I somewhat discussed that in the earlier questions how that applies to those situations, but I'm sure the colleagues here will have plenty to add to that.

DR. HEERINGA: Dr. Hanna.

DR. HANNA: Dr. Hanna, University of North Carolina, concerning the part A, I just add I think Dr. Bartlett give a very good review of the answer to this question but I just tried to fill-in some of the parts that I feel, besides the of course the National Weather Service is the best data source and it has been used in this study and he cited a number of advantages and disadvantages, also was the use of the other data from California and Florida, some of the parameters are not accounted for like the cloud cover which is used for the addressing or calculating the stability and size CST3 model so that adds to the support of use of kind of credible data which is in measure with the service data.

The part B is what criteria should be used to identify meteorological regions for analysis and how should the specific monitoring data be selected, and again this has been addressed correctly and we mentioned the additional state climate offices data that in a number of states actually that could be used if we wanted to move the application from one region to the other, and they go even to agricultural target, a agricultural applications like I had mentioned before the North Carolina (inaudible) Network and that can be helpful in really targeting other regions in the country, of course besides the usual or the requirements related to climate and (inaudible) as Dr. Bartlett mentioned.

Also if we are designing a field study to try to get actual measurement, meteorological measurement information for a certain experiment or application I think we should look at the climate or the terrain and see what is the upwind and downwind locations that which would choose even in a microscale we should choose the location of the measurement station in order to be able to track the plume.

Part C is the 2 meter versus 10 meter kind of wind measurements. I think when you're moving to the

AERMOD maybe and I think Mr. Sullivan has mentioned that would be having measurements and boosts levels or moves one level over to characterize the sigma W which you will be able to characterize the turbulence and give us a more accurate measure of the kind of stability during a certain process.

In part D, assume distributions that Hanna which is Dr. Steve Hanna is different (inaudible) to me as being calculating and published about it, that is I think an appropriate approach although even in more detailed approached you can even get the feeling of the uncertainty distribution using a comparison between data sets themselves, even if the National Weather Service data, even with the modeling study by slicing the say wind feed and I'd say with lists and five meter per second, five meter per second to fifteen meter per second and so on, so we get a bit to actually track of the kind of uncertainty that you may find by a calculated course of standard division and all this kind of parameters that make you, give you a distribution, usually it is a normal distribution for the wind as at least for zone to speed and also we can do the same for the wind direction in order to get the

more detailed feeling of or measure of this (inaudible) in the meteorological parameter especially when you are doing a specific area and you have the meteorological stations available in this specific area and you want to do the applications in the -- or the FEMS application in some of the parts of this area.

And the part E is the stability class as was mentioned the ICST has a kind of one-hour target for it certain stability class, hopefully the AERMOD will move to more accurate measure of the stability which is key really to this kind of application. Well, again I leave it to maybe other colleagues that can address it better than me.

DR. HEERINGA: Thanks. Dr. Ou.

DR. OU: I don't have much to add except to say that you know probably a good idea to add you know regional meteorological data such as the (inaudible) and I mentioned previously there are some region may have (inaudible) particularly in Florida in the June of summertime we have frequent sunburst shower and at nighttime, so it may by a good idea (inaudible) event.

And the other point I want to make that since the MITC (inaudible) is biodegradable and (inaudible) including the soil temperature and the soil water content since this items in (inaudible) a good idea to add you know and in relation soil temperature and the soil moisture content was (inaudible) or the MITC in soil.

MR. SULLIVAN: To clarify the more recent studies we have done with Dr. Ajwa he has tracked temperature and moisture at multiple levels in the treatment zone and also has data to characterize gas phase and liquid phase MITC throughout the treatment zone which is helpful in that regard.

DR. HEERINGA: Thank you very much, Dr. Ou and Mr. Sullivan.

Are there any other comments? Dr. Bartlett has some follow-up.

DR. BARTLETT: I neglected to make a point that we had made in the previous session. In regards to terrain topography, terrain of course can be taking care of with roughness and topography those aspects it was suggested by Dr. Hanna before to look at RAMS in the previous thing and MM5 both RAMS and MM5 can

create micrometeorological data that can be very useful and though that is extremely time consuming to do that to a particular area, a lot of these data sets have been created for other purposes that could be used experimentally as data inputs to AERMOD and would work better with AERMOD and then or ISC to see if using this approach to see how boundary conditions might change you know in these situations of farms and valleys and other things that might be characteristic of other agricultural regions.

DR. HEERINGA: Dr. Hanna has --

DR. HANNA: A quick comment and Dr. Bartlett that's true, I would also like to add the RAMS with the regional atmospheric model system which is a (inaudible) has more final resolution in the application so it could be more appropriate actually and (inaudible), but the RAMS has more physics related to (inaudible) that could be really more applicable to what we are doing here.

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

DR. SHOKES: Fred Shokes, yeah, I would just like to emphasize that in some parts of the country local weather can be very different in very small areas and we operate a peanut/cotton info net for disease forecasting over an eight-county area and in order to forecast disease accurately over that eight-county area it requires ten weather stations out in the field because local conditions differ that much, it's one aspect, we don't measure the wind direction and wind speed and all, but we do measure things like soil temperature and moisture and leaf wetness and things like that.

One other aspect of that if particularly in areas of where you have localized showers which we do frequently and I know in Florida they do also, those can change soil temperature rather rapidly so there are a lot of effects there. If you're going to model things so that you're going to set conditions for regulation you really need to use local weather because those are the conditions that will actually be effecting the movement of that fumigant in the weather and the emission.

MR. SULLIVAN: What we have proposed to do is to identify a set of meteorological stations in a region that would cover the gamut reasonably well of terrain and climate and so forth as discussed before, and it comes back to the issue of what's acceptable, there would be so many specialized specific differences but the issue is can you adequately characterize that variability in general, and I think that we could whether it be five to ten, it may vary by region and how that's done needs to be discussed among those involved, but I think the intent is we can't expect to estimate for specific locations or counties, but (inaudible) by region would be reasonable and would have multiple stations I think would allow us to do that reasonably well.

DR. HEERINGA: Dr. Portier and then Dr. Seiber.

DR. PORTIER: On the question of meteorological regions the thing that pops into my mind is hasn't this possibly already been done. One of the responsibilities of the state climate office is to actually do this kind of stuff, and I remember back as a young statistician participating in a USDA

regional project on ag climatology, so I would think you would need to talk to the USDA about whether they've created climate regions and also look at the state climate offices and ask them about that.

MR. SULLIVAN: The National Weather Service has established climate regions throughout the United States that could be used.

DR. HEERINGA: Mr. Dawson, on this topic.

MR. DAWSON: That was going to be exactly one of my follow-up questions because sitting here thinking from a practical standpoint about how we're go to use this and having a wife who is a gardener I think about the USDA growing regions as an example, so you know you have zone 8 or zone 7 and those kind of things, and I think that would be potentially that kind of system or something like it a good premise for us to start because that work's been done already.

MR. SULLIVAN: And, Jeff, you might be able to overlay those two, meaning the USDA farming regions on top of the National Weather Service climatic regions, that might be one way to identify common areas to include in the modeling.

MR. DAWSON: Right. For example with the

USDA, the growing reasons I believe they're based on soil type and kind of met conditions, but we would have to go in and look at that in more detail.

DR. HEERINGA: Dr. Portier has a follow-up on this.

DR. PORTIER: Just quickly, there's also a national, a loose organization of climatologists to which we can ask these questions, there's something called climb-list (ph), you may belong to it, I belong to it, and there's all kind of research like that that goes on internationally that try to actually answer these questions.

On the issue of the assume distributions I should point out that this looks very similar to what's been done with other risk models that have come before the panel, for example in lifeline calendex (ph) and the sheds procedures, I mean this idea of adding on a perturbation distribution to move a parameter, the difference here and the concern I have and I'm not quite sure how to state it correctly is that we're dealing with weather time series and the

natural time series have a persistence, they have a pattern to what goes on and as you put these perturbations in if you don't put them in right you break up the pattern and then we don't have natural time series and I don't know what the impact of doing that on an hourly basis or whatever is, to me that's another research question entirely that a good meteorologist or a climatologist may be able to answer, but there is some concern, I think it was Paul that mentioned, Dr. Bartlett mentioned that it kind of breaks up that pattern and I would hate to lose that.

MR. SULLIVAN: I think that could be shown and it could be taken and do the perturbation and overlay that onto what it was before and just to confirm it doesn't have the anomalies like you were mentioning, and I think when they did that expert elicitation they set those bounds, they aren't that large if you look at them I think with that consideration in mind.

DR. PORTIER: The concern actually the anomalies are the things I may want to maintain, the fact that you may have a long wind blowing for eight hours or ten of one meter per second, you would say that's usual for Southern California, but maybe that's going to drive that extreme observation that's going to be your 99.99 percentile, and if you break that up it moves that tail distribution further in and then you're anti-conservative in your estimate, so I don't know how those things work out.

DR. HEERINGA: Dr. Seiber.

DR. SEIBER: Jim Seiber. Just on that point, there's something called Santa Anna winds in Southern California that might be that type of wind. My comment is on the last sentence, the concurrent use of emissions in meteorology to identify upper bound, just to harken back to discussion earlier and I think Dr. Yates brought this up that use of shorter time scales during the application are particularly important because that's when you will get drift in spikes in your downwind concentration data.

We try to use when we run field experiments like this is shorted sampling duration is physically possible to still change all the samplers, this is in the experimental part, 15 minutes or 30 minutes is not uncommon so it seems to better characterize that initial flux material that comes off during application or just after application.

DR. HEERINGA: Thank you very much, Dr. Seiber. Dr. Spicer.

DR. SPICER: Yes, I did have a follow-up to what Dr. Portier was saying in that my understanding is that you allow variability of the wind speed and wind direction, but you don't allow variability of the stability; is that correct?

MR. SULLIVAN: We don't recommend that. The switch is in there which would be appropriate when AERMOD is available.

DR. SPICER: Fair enough. And you presented a plot that indicated that you could actually have, underpredict concentrations if you did so and I respect that. The thing that I thought about was the fact that if you allow the variability in the wind speed then you may actually find yourself in a situation where you would have a wind speed which would now be inconsistent with the atmospheric stability that would be.

MR. SULLIVAN: It does loop back on that

issue to confirm it doesn't cause a mis-match with stability.

DR. HEERINGA: Dr. Winegar.

DR. WINEGAR: One real last very quick question is basically one word and that is representativeness, I keep thinking of that's essentially what we're talking about in terms of the meteorological data. The EPA goes through a lot of effort to establish representativeness in their air quality monitoring stations and that's one of the ways that that data is qualified and validated on that basis is that location representative for the monitoring data that you're using, so it kind of underscores a lot of what we're talking about here.

DR. HEERINGA: Dr. Yates.

DR. YATES: Yeah, this is a follow-up on Dr. Portier's comment also. It seems to me that you know I think you're right about that, you know it seems thinking in soil physics and there's geo statistics an area that used to be fairly active and basically the idea that you know sampling locations that are close

together are more likely to be the same, so they are especially correlated, it would seem like the proper way to do this would be to try to remove the, you might call it the deterministic process, and then pertubate (ph) what's left and then add the deterministic process back in so that you don't do what Dr. Portier was saying where you take out the correlated structure that's in the data, so I think you're absolutely right, I think that that's the way they should be doing it and I don't think it's being done that way right now.

DR. HEERINGA: At this point I think unless there are any additional comments I want to do something that I neglected to do at the end of question 3 and I will go back to do that, but I would like to go systematically through each of these six subpoints just to make sure we feel we've touched on them and Mr. Dawson and Mr. Sullivan if at the end of this if you feel there's anything we haven't covered if you would just indicated that please.

If we could go back to part A, we'll just let everybody read it. I think we have had a fairly firm recommendation on the use of the NWS data and the suggestions about some of the other alternatives. Does everybody feel that we've operatively covered that, have you been able to say what you wanted to say on that, I think we've also had a good coverage and a good discussion of meteorological regions and how data sources might be adapted to different local climate and other growing and soil conditions, I think we have also had a good technical discussion of the sampling height issues and how that it goes, point D, I think we've also had a fairly stimulated discussion about this topic and I'm not sure that we have come to a concise recommendation, but I think that some of the suggestion between Dr. Portier and Dr. Yates here I think will definitely need to be -- is there anything else that the panelists would like to contribute on this particular issue? Dr. Bartlett.

DR. BARTLETT: I was wondering if there's some sort of consensus that the suggestion of bringing in other data sets I'd ask might take care of some of this problem, when I asked you the ideal question like if you had 200 years of real weather data would you take that in place of have now and I think you said yes under further reflection, and I was wondering if this other idea you've just introduced of pulling five or more weather data sets in a region whether that you really need to do this disturbance or not to create the results you need for the boundary conditions.

MR. SULLIVAN: I think that if you want to look that the upper tail as Dr. Portier was discussing earlier when you have especially persistent winds during adverse meteorological conditions and other ways that it would still be advantageous to take the two hundred years however you get there, I mean if you had two hundred years of data from all over that region you could start with that point, but it would still makes sense to -- but I guess that would in a sense that would take care of it because we are right now generating two hundred years, if you had two hundred years to start with that would probably be sufficient to do that.

DR. HEERINGA: Dr. Baker.

DR. BAKER: You can run the model in the mode where you don't perturb and you get one answer and run

it with the perturbed and you can another answer with

--

MR. SULLIVAN: You can do it either way that shows the sensitivity to each one of those things you're perturbing.

DR. BARTLETT: Maybe that would be a consensus to try to experiment with the pool and then compare them and then you have a sensitivity analysis and an understanding of the uncertainty.

MR. SULLIVAN: We do know that unless you go in fairly high up the upper tail that these perturbations on wind speed and wind direction do not make a large difference, so in that sense any changes done are not major in the big scheme of things, again, it's on the issue and you get to long recurrence interval evaluations that it becomes a factor or a significant factor.

DR. HEERINGA: Dr. Portier.

DR. PORTIER: You know since we're talking modeling here if you really wanted to go the far extreme really all we're interested in is a four-day weather pattern for these things, I mean you could --

if I'm talking about strawberries in south Florida I could collect all of the periods where I might possibly apply this stuff and do some kind of analysis do come up with what's a typical day and then what causes an extreme day on either end, and actually develop four-day model scenarios to run through this thing rather than -- I mean the whole idea of trying to get real data is to maintain real patterns with real distributions but if you got enough of it you can synthesize it in a model, that's what we're doing with the ISC model, it's a lot harder with climate I got to admit and rain and wind, I have no idea what wind would look like.

MR. SULLIVAN: There would be ways to streamline the process like you're suggesting and that could be done, we took the path of using TOXST as our study point because that is an approved EPA procedure and if the consensus of the group was that it would be appropriate to streamline it for run times that could be considered, the issued comes down to when would it stabilize, how many applications would it take to stabilize and if it took ten thousands simulations

then at that point you might as well run the whole two hundred years, but there are ways that it could be streamlined but that's my hesitation is we're dealing with E approved models at this point.

DR. PORTIER: Going back to the previous models on food dietary stuff, we haven't synthesized diet, we've gone and used what little real diet data we have for exactly the same reasons that the Agency likes to stick with, kind of real measured data than synthetic scenarios, so I'm just offering here's an alternative, one that you might look at, but I suspect the Agency would say no, we're doing to stick with real data in these situations.

DR. HEERINGA: I do believe that in this short discussion and the previous discussion there is some consensus for just some sensitivity analysis on this introduction of added variability on these clinicological inputs or meteorological inputs here. I think just to see that affects the ultimate objective and that is of measuring buffer zones at some certain specified levels of concentration and I think evidence that didn't consistently sort of lead

to 10 or 20 percent underestimates on those would be at least comforting and obviously -- anything else Dr. Winegar?

DR. WINEGAR: On quick question, I'm wondering if the Hanna `98 paper is available electronically, you know so we can take a peek at that that before we do our final written report?

MR. SULLIVAN: I'm not sure if it's available electronically, but I certainly have a copy that I could share with EPA that could be distributed to the panel.

DR. HEERINGA: We will see that we get a copy made and distributed to members of the panel and the copy would also be placed on the docket for these proceedings if that's appropriate. Anything else?

Point E on this question, do we feel we've adequately addressed this, Mr. Dawson, Mr. Sullivan you can join in too.

I think Dr. Spicer had a very, very good introduction earlier showing the effect of these stabilities and potentially some inconsistencies on that. I think we're covered on this at this point.

And then finally point F which is the impact of the concurrent use of the emissions and meteorological conditions and characterizing the upper-end concentrations. Again, here the objective seems to be a -- the thrust of the question is whether in fact we are adequately simulating extreme conditions and extreme events in the upper tail, I think Dr. Baker made the point about the emissions distributions and that sampling how you could actually someway may have more variability that you might expect.

Mr. Dawson, Mr. Sullivan, are you satisfied at this we've covered each of these.

MR. SULLIVAN: I am.

DR. HEERINGA: At this point I have 11:38 and I'd like to call a break. If we could reconvene at five minutes of 11, 10:55 and we'll continue with questions 5 and hopefully 6 prior to lunch.

Just looking at the individual questions and the content and I sort of anticipating necessary discussions on question 5 is going to take a little longer, some of the others may go a little faster, but we'll try to get questions 5 and 6 in before lunch and tackle 7 and 8 after lunch.

(Brief break.)

DR. HEERINGA: I believe we're question 5, if I could ask Mr. Dawson to read question 5 into the record.

MR. DAWSON: The Agency model, ISCST3 is the basis for the FEMS approach. This model has been peer reviewed and is commonly used for regulatory purposes by the Agency. FEMS also uses other Agency systems such as PCRAMMET and TOXST.

Question A, are there specific recommendations that the panel can make with regard to any parameter that should be altered to optimize the manner that they are used in FEMS.

Question B, ISCST3 can treat "calm", in other words period where the wind speed is essentially zero conditions in one of two ways including the concentrations is set to zero and an approach that uses the last non-calm wind direction or concentration. FEMS uses the first approach. Does the panel concur?

C. In Section 2.2 Specific Technical

Considerations With Regard To The Design of FEMS of the background document, there is a section entitled Computing Endpoint Distances. Please comment on the procedures included in this section.

D. The FEMS analysis is based on a single field being treated once per year. The FEMS analysis is based on a single -- that's a double in there. On this basis ISCST3 files include 200 full years of hour-by-hour sequential data. Application start times are randomly selected to match the user-supplied application frequency. For example, if a model user entered 10,000 simulations, there will be approximately 10,000 randomly selected standard times with batch modeling treatment of four days duration for each application. In addition, FEMS allows for more than one application per year to be modeled.

Question 1, does the panel view this as an appropriate process?

Question 2, If not can it suggest recommendations or modifications that may improve this process?

Question E, Can the panel comment on the source geometry using FEMS and the implications of this choice?

DR. HEERINGA: Our primary discussant on this multi-part question is Dr. Eric Winegar.

DR. WINEGAR: Yeah, primarily I guess this overall question is addressing the ISC input and I look at the question in two parts, there are individual parameters that go into setting up ISC and then there are data inputs and we've talked a lot about data inputs in terms of mat data, et cetera. The specific question A I can only think of something that I don't think is currently available in ISC the discussion that's been made in regards to dispersion coefficients, it's my understanding that that cannot be modified in ISCS; is that correct?

MR. SULLIVAN: Well it could be modified --

DR. WINEGAR: Hard wiring.

MR. SULLIVAN: Yes.

DR. WINEGAR: Well that may be something pre approval of AERMOD that could possibly be done if you look at that -- wanted to look into that question about the sensitivity of those dispersion coefficients. I'm sure your programmers are talented enough to deal enough with that.

MR. SULLIVAN: Correct.

DR. WINEGAR: Question 2, the calm issue, I think it's good that both of those options are available. Essentially it's a question of how -- what level of conservatism, whether you are going to emphasize the lower wind speeds, et cetera, and how the implication that that is the for the flux rate and downwind concentration obviously. Having the ability to deal with both of those I think gives a flexibility for the users to be able to look at different situations.

As I think about how modeling is done it's not like you think of I'm going to put in all of these parameters, all this input, and run it and that's it and walk away, no, you look at different things and you get a feel for what gives the best, for what looks good and for what starts feeling appropriate and you have to compare that not so much always on a strict statistical basis, but it's a lot of times just

looking at patterns between different runs and trends, and so as I see it having that capability available is useful for combining the different types of data that are going in the different types of input and so having that flexibility is a pretty critical feature. I can't really say that -- make a recommendation about which is the best way to do it because I think that does depend on a number of factors.

Question C about the end point calculation, that appears to be appropriate from what I can tell, I have no real in-depth comment about that.

Question D, multi-point basically, based on what I read in the background document it seems like the investigation that you did in terms of running a different number of simulations, I don't recall the exact figure, but you plotted the results for different percentiles as a function of the number of simulations and how it converges and I presume that you came up with this basis on the number of simulations here from that investigation and that appears to be an appropriate process.

MR. SULLIVAN: We did vary -- did the same run and varied the number of simulations from 200 to I think it was 20,000 years to see how it did stabilize as a function of end point concentration, and it was very similar, we did it for two test cases and it was very similar results both times.

DR. WINEGAR: And this is a process that basically is coming out of that Hanna `98 paper?

MR. SULLIVAN: Well I mean it's based upon considering those factors, I know it runs and I believe we treated as varying uncertainty for wind direction, wind speed, and emission rates, and so those three parameters were variable and then we made multiple runs of different simulation numbers to see how different the results were and we found that it did bounce a little bit if you had less than a thousand or so simulations, by the time you got to five or ten thousand nearly all the end points we looked at would be quite stable.

Our goal was to achieve accuracy to within 10 meters and when we round up that's sort of a conservative feature, we found that if we found five to ten thousand simulated years that we would achieve

that goal, and then if we went to a long recurrence interval and the example we gave as a 20-year recurrence interval then the recommendation was 100,000 year simulation just to be safe.

DR. WINEGAR: But parts of my perspective which is really from a limited experience in this regards it does appear to be appropriate and it does appear that the adequate amount of work into refining how and making this process does appear to be appropriate in my opinion.

And the last question about regarding the source geometry, I do agree that the capability to do different shapes of fields is appropriate. I believe that models should represent reality as much as possible obviously and the reality out in the fields is that most are not square and having the capability to do different size adjustment of the source geometry is important and useful and it should be useful for the eventual users of the data, so I believe that that is an appropriate thing to include and a very useful one in terms of making it more broadly applicable.

DR. HEERINGA: Thank you very much, Dr. Winegar. At this point I would like to ask Dr.

Portier for his comments.

DR. PORTIER: On item one as previously mentioned with this application it seems that the issue is more with the design of the other model components then with the key internal programs that have already been approved, so I don't really have much to say about that other than you know it's the extent of the climate admissions data that's passed through the model that's more important than the models themselves at this point.

On the calms I started thinking about this and what is the impact that these two choices, the true impact of calms to my mind is a little bit difficult to determine, if wind speed was observed to zero for a four-hour period does that mean that there was no flux, meaning if you choose calms and wind speed was observed then by the back calculation method you're going to get no flux for that location I think, right, so the assumption of a calm is an assumption of no flux and that's probably not the correct assumption, right, so where wind speed and flux in this case are just so highly linked that maybe it's not reasonable because setting concentrations to zero -- but it alternative approach results in flux for the zero wind period being exactly that of the previous period, it's almost like there should be some option in the middle, kind of like a detection limit option that says the zero wind speed really means a below detection wind speed and it's probably half of what the wind speed used to be or maybe a quarter, and so if I were to -- I think the problem is both of these options are too conservative or too anti-conservative, we need an intermediate option, so if I were to go and mess with the ISC code that's where I would kind of put an option two under calms, that would let me split the difference.

MR. SULLIVAN: There is one in compromise, I mean we'll follow the regulatory mode and with the averaging times that we're using at ISC, the one-hour averaging time, it treats calms in that way by saying that the concentration's going to be zero. The alternative would be to use a no-calm procedure if it was a four-hour average and there more one or two

hours with non-calm it could use those two hours to represent the period.

My thought was that may be preferable as long as they do the follow-on exposure assessment the same way that that would you know may be better than assuming the zero.

DR. PORTIER: On the issue of Section 2.2 I will preface my remarks by saying I haven't read all the additional papers, so some of this may be explained in here and so I am going to be a little critical here, here at this point we're kind of asked to take the developer's word for it that this works because I didn't see any data that provided support for the statement of the method. I mean if I could quote the method, the largest difference around the compass that is need to reach the input concentrations of concern is computer through FEMS by log-rhythmic interpolation of the TOXST output, this log-rhythmic curve is based on the expected drop in concentration with distance and match as well the modeling results. Once the approximation range is determined by assessing the rings with the closest number of

occurrences of the threshold value, the two closest distance rings are used to target the proper distance by interpolation based on the equation of the curve that best simulates the decline in concentration -- so it goes on and then it says I've got an exponential curve, but there was nothing to support that. I mean I guess I was looking in the documentation for a here's what I got kind of thing.

MR. SULLIVAN: An example perhaps would be helpful.

DR. PORTIER: Take the TOXST output and kind of put it into a graph and so me how that works because the problem that we had and we've mentioned this earlier that getting documentation on TOXST is the hard at the moment because the documentation's not available so as I tried to work through this and see if I could create it I couldn't find the critical documentation.

MR. SULLIVAN: Would it be helpful for us to provide that to the panel a copy of the TOXST manual?

DR. PORTIER: Well you can work that out with the designated Federal official. My point here is it needs to be documented. On the other hand it sounds reasonable, I mean it sounds like a reasonable thing to do and I don't expect -- I mean I expect that you can prove to us that this is a good idea, it's just I couldn't -- I couldn't re-create it which is either a failure of the method or a failure of the documentation.

The last issue is whether we can suggest recommendations or modifications to approve to look at the two-hundred year hour-by-hour sequential data. If one considers 200 years of hourly data there's 1,752,000 potential starting points in the output file generated by ISCST3. Each four-day period consists of 96 hours, dividing this 1,752,000 x 96 yields 18,250 potential non-overlapping study periods, and it's choosing 10,000 randomly selected start times shouldn't produce too many overlapping study periods.

And the reason I go through all of this is that when you're sampling from this population of 1.7 million you have to worry about how many times do I really sample the same thing, right, the same scenario is repeated over and over again, so I went through

this to kind of assure myself 10,000 wasn't that much if we're dealing with a year. Now that said, suppose we go back and we rerun the analysis and we limit ourselves to six weeks in the winter as the window of application for a specific crop, that's going to dramatically reduce the number of potential four-day periods and 10,000 may overwhelm, so this ending distribution that we're looking at really is a distribution that's created through a lot of copies of the same files, and so we end up fooling ourselves into thinking what we really know what that distribution looks like.

So some exercise like this has to be thought about as to what's the minimum window when we take the IST file and give it to TOXST and say okay give me some samples run in some four-day periods, how limited the pool of possible four-day periods will produce -- every hour can produce a little sample, but the samples are not going to be that different.

MR. SULLIVAN: Dr. Portier, if we were doing a seasonal analysis would it make sense to have a longer period?

DR. PORTIER: I think if you're going to run

10,000 you're going to need to think about doing that, having more than 200 years I'm hoping you're not going to run over 200 full-years to do you know six-week analyses, I think you're go to have to think about the data base issue.

MR. SULLIVAN: It would be the one of interest would be the one shown.

DR. PORTIER: And then finally the geometry, I agree with Dr. Winegar that this allows you to take this factor sore shape into a sensitivity analysis at some point which is something we haven't really talked about, we'll talk about it again, but it's another factor, it's probably not a major factor, but it's nice to have that capability.

DR. HEERINGA: Just to insert a comment here before Dr. Hanna has a chance to give his comments, the TOXST documentation we will get that to the panel members and of course it will be made part of the public docket as well, it's distributed to members. Dr. Hanna.

DR. HANNA: Dr. Hanna, UNC, concerning about the again as I mentioned before it is possible really to get into the model and look at the sensitivity or unsensitivity related the dispersion, there are horizontal and vertical dispersion. There are some field studies that really compare the calculated horizontal dispersion with measured dispersion and with those you can -- the (inaudible) 100 year or 10 years or something but there are at least field studies that can give a sense of how much a difference we can expect, what is uncertainty and it is like a log-normal -- you can creat a log-normal distribution with an uncertainty bans (ph) that really you can pick certain values randomly and to test the model sensitivity it will be multiply up to the sigma Y and sigma Z in the model, but it will be given idea about how much a sensitivity a model in ISCST3 and its results can be related to this kind of dispersion of effects.

So in this case inputs we will have been mainly talking about the inputs is that (inaudible) emission flux, but actually here we are talking about the in a way is a model formulation itself and also

including the parameters that are not adequately as we hope presented like the stability classes and so with the (inaudible) it will better, but there is a way really to measure the sensitivity of the ISCST3 to horizontal and vertical dispersion.

In part B about the calm winds, I agree it is a kind of puzzled issue but the thing I want to mention that the weather stations I think most of them have a lower bound, I mean they report that speeds of less than certain values this a calm, so in a way calm might not be really calm, it could be -- maybe its an interpolation it's more realistic I mean it depends really, so that's a kind in my opinion is open issue but again as was suggested is maybe sensitivity to see what is the effect of the interpolation or calm condition between effecting the results, it may not be effecting the results a lot.

And, yeah, I satisfied with Dr. Portier's answer to section, to part C, and also fort part D I agree with Dr. Winegar about the number, the 10,000 versus and you say that 20,000 did not make a big difference, I think that could be an acceptable answer for me.

For E is (inaudible) definitely I think it's better that we have a model (inaudible) to present a more realistic source (inaudible) just a square or rectangle.

MR. SULLIVAN: Dr. Hanna, just to clarify one point, in terms of the concept of modifying the ISC dispersion coefficient terms that would be a useful concept, but my question, if we were to simulate that uncertainty from the field studies in the input parameters, in other words if we were to use a model such as AERMOD to do that and to compute through turbulence theory what those values would be then perturb them to show that uncertainty we would have to modify a model, would that in your opinion be equivalent?

DR. HANNA: I see what you're saying that you are perturbing the U and V so you would perturb in the sigma Y and sigma Z.

MR. SULLIVAN: Right.

DR. HANNA: Well, the point that we had a lot of discussions related to that and it was so that even

the field analysis or the field study for the sigma Y, sigma Z seem to be having its own kind of pattern or not necessarily pattern but the distribution that really it would be more beneficiary to test as a model into in perturbing it's a sigma Y sigma Z rather than depending on the perturbing U and V, and I can send you some information related to some field studies related to that.

MR. SULLIVAN: I would appreciate that very much.

DR. HEERINGA: Thank you very much. At this point I would like to open it up to comments from any of the other panel members. Dr. Spicer and Yates.

DR. SPICER: Well the first comment I had was with regard to the calms issue and I agree with Dr. Portier that when you have the wind speed going to zero that does not mean that the flux then stops, in fact you're going to be piling up material in the gaseous phase over the source. There is some rational reason for choosing the concentration of V equal zero at downwind receptors because of the simple fact that the materials piling up over the source and it doesn't go downwind so I can understand that choice, but what means physically is that the real discrepancy may show up in the period after that calm condition because now all of a sudden you're advecting material down field that has a much higher concentration than would have been predicted by the flux model in the first place and so that can lead to discrepancies and uncertainties that would be amplified by these situations.

And so such effects need to be taken into account in my thinking both in terms of the flux considerations and in terms of the predictive use of the model.

DR. HEERINGA: Dr. Baker.

DR. SPICER: I'm sorry, and then there were a couple of other things.

As far as this computed end point I messed around with the algebra a tiny bit and I think that what is being down is that this is a semi-log interpolation.

MR. SULLIVAN: That is correct.

DR. SPICER: Okay, but it looks like what you're doing is you're taking the log of the distance versus the linear exceedances, is that...

MR. SULLIVAN: That's I believe correct. The issue is we tried various ways to do these extrapolations over a number of years and found that servant peeracle (ph) is shown there to work the best, and what I do want to do is to follow-up on Dr. Portier's comment is in the final documentation provide a better example and show how it work, maybe show a plot, a concentration plot as well to make that point, and there may be a better way to do it, that's certainly true too, but to get within 10 meters or so that works for us.

DR. SPICER: Well and certainly you could test it out against you know you could use three rings that are predicting and then see if you can predict the middle ring between two values and so that would be one way to check it.

MR. SULLIVAN: That would be a good example.

DR. SPICER: And like I said, this looks like its log-distance versus linear concentration if you equate exceedances with concentration. When I look at concentrations I tend to use log-concentration versus linear distance, the difference in your example is very small, but that might be something to consider.

And then the last point and question that I had can a field application be started at 2 o'clock in the morning?

MR. SULLIVAN: It certainly can be and I would say the data we're showing here is representative of applications that start shortly after or near sunrise.

DR. SPICER: And I guess that since the flux data is done once again for a single realization that's done in the hour after sunrise that it makes -- it makes less sense to me to have meteorology that can be started at a different time. It would make more sense to me especially due to the fact that you're talking about changes in stability and those sorts of things right at this hour associated with sunrise to restrict your starting times to those that are associated with sunrise as opposed to 2 in the morning when it simply can't -- it's not going to be done.

MR. SULLIVAN: Yeah, actually, in reality the actual in all cases when a start is made regardless where it comes in that cycle if you look at the whole distribution it would match as if it were starting at 7 in the morning on day 1, on period 1, because you know it's just going to pick into a cycle, that 24-cycle business again.

DR. SPICER: Right. And maybe I'm just not understanding, but the point is that if you say that you start at 2 in the morning then your weather conditions, and you start with 2 in the morning weather conditions then they're not reflective of what happens at sunrise when you do this change of stability thing. Now granted you still take those two hours after midnight and put them at the end of the cycle, but they're out of sync.

MR. SULLIVAN: Well we start at 2 in the morning on day 3 of the 4 then we are using emission rates that are appropriate for that 2 in the morning on day 3 and meteorological data that was from that period of time, so there is a matching between the emissions and the meteorology wherever it comes into the cycle, but your point is well-taken, that cycle is

representative of applications that do start shortly after or around sunrise and you know would not represent an application that you started at 2 in the morning or 8 at night.

DR. HEERINGA: I think Dr. Yates -- why don't we have Dr. Baker follow-up first since I think you were on this particular.

DR. BAKER: I just wanted to follow-up on the calms and the issue that Dr. Spicer first mentioned and point out again that to the extent that the field data used to calibrate ISC has captured calms and is calibrated against what is called the robust highest concentration from the field studies, then the issue is captured although not you know we might not like the formulation in the way its captured, those types of scenarios are captured to the extent that we have field data and for the back calculation approach there is some degree of self-correcting by using the same model in the back calculation and then the prediction.

DR. HEERINGA: Dr. Spicer.

DR. SPICER: Well I respectfully have a

question with that because of the simple fact that the met conditions immediately following the calm may not be the same between the two points and so you're -- well it's definitely a square peg in a round hole.

MR. SULLIVAN: But the point that came up yesterday -- probably good advice for anyone doing a study in the future to minimize this problem is to perhaps consider a sonic anemometer as some way of getting very sensitive resolve data so it doesn't happen very often, that would minimize the problem. But also your point is well-taken that if you have a very calm period for four hours you may be -- then the next hour you'll be very much higher and it's over -- it's just carry-over, that would probably happen to some extent.

DR. PORTIER: Ken Portier. In the model formulation when you see a zero wind speed can you just put half the speed of the previous one and just run it in and then the ISC thinks it's real wind data, right, I mean there's an easy wake to look at that effect, right?

MR. SULLIVAN: You can but you'd have to then assume persistence in the direction as well, you have

a calm you miss them both and that could be done, maybe just assume persistence, that's another way you could consider it.

DR. HEERINGA: Dr. Yates.

DR. YATES: Yeah, I was just curious about whether with the ISC model whether a zero value for the velocity would be something that would give you a numerical answer out of ISC because for water flow you know there's also dispersion and defusion and generally speaking you know you have both terms in there, so if the velocity of the water goes to zero you still have a defusion term and things work, but I just asked Dr. Spicer and he said no, they don't have the defusion term, so when you get a zero velocity things are undefined, so that answered my question.

DR. HEERINGA: Very good comments.

DR. YATES: Although in a sense I guess if it's possible and this would be up for the people who understand meteorology a lot better, if it would be possible to somehow put in a defusion term I suppose then the this issue of calms might not be so important because the zero velocity would lead to just defusion

and you would still get a flux from the soil and but maybe there's something in terms of the meteorology that makes that difficult or not possible, I don't know.

MR. SULLIVAN: Defusion's a real tiny little term compared to turbulence and you may want to address that.

DR. HEERINGA: Dr. Baker on defusion.

DR. BAKER: No, not defusion. My comment is on the legacy or the history or where ISC came from and it was for longer distances than we're looking at here and addressing different standards and formulation again is a study-state plume that is instantaneously thrown out in each direction and has concentrations in those directions at far downwind distances that travel faster than the speed of light, so that there's time of light considerations that can't be brought into the way the model's formulated so there is some inconsistency here and I think that's one of the issues, but I still agree that ISC is a good starting place for doing this work.

DR. HEERINGA: Dr. Winegar and then Dr. Yates.

DR. WINEGAR: Mention has been made of kind of -- Dr. Portier mentioned about the analysis of the detection limit question and you have a zero and one and you go half the detection limit so you go half of that, and I know other people have done in the context of detection limit issues, created a distribution between the actual zero and the detection limit.

I'm wondering if that has been done in this regard about the calms anywhere, I mean it seems like the calms issue is not unique to our application here, I mean have you seen anything in that regard.

MR. SULLIVAN: In terms of if you have a calms use half of the value, I've seen that done occasionally but then again the problem is the wind direction, you don't really know that wind direction so you have to assume the one that happened before was calm was applicable. You can get situations though potentially where there may be a mismatch in the sense of the wind vane versus the anemometer, you may know one of the two parameters that is varying and then you can make some judgments in that situation, but it's tricky, I like the concept of having more sensitive anemometer and that is the answer I think to avoid this.

DR. HEERINGA: Dr. Yates.

DR. YATES: To go back to the analogy with water flow, another thing too is the velocity becomes very, very small, that defusion term becomes more important and so in the calm or as you approach calms if the defusion might become a factor, but aside from that given the purpose of the FEMS model is to develop things like buffer zones, maybe all you'd have to do is just take a look at as low as you can go in terms of velocity you know find out if the defusion terms are about the same order magnitude as a -- I mean the dispersion terms are about the same order of magnitude as a defusion term and if not then forget it, and then just look at what the buffer zone would be and if it's less than say I don't know, 50 feet, 100 feet, something where you know that the regulatory agencies are going to have a minimum not matter what and if it's less than that then it probably isn't really even an issue because -- except for what Dr. Spicer was saying about for maybe the next-time step it might be a problem, but in terms of the current one if it's in what would be in the minimum, absolute minimum buffer zone that would be allowed then nobody's going to be there so there wouldn't be any exposure.

DR. HEERINGA: Mr. Dawson.

MR. DAWSON: I think Dr. Barry would like to add a clarifying comment to this discussion.

DR. HEERINGA: Dr. Barry.

DR. BARRY: Dr. Barry, DPR. The one thing about the wind speed for people who aren't modelers you can't really go below one meter per second because the wind speed is in the denominator so of the concentration estimate, so in many of our studies we can measure the wind direction, the threshold of our wind vane is enough that we know the direction lower than one meter per second wind speed, but the wind speed may be lower than that, you have to round it up to one, so but the calm is less than one meter per second, so the issue if you know the wind direction theoretically the wind speed's not quite right though, but you know the direction below that threshold.

MR. SULLIVAN: That's true that if you put into .5 the model will round it up to the 1, correct.

DR. HEERINGA: Dr. Wang.

DR. WANG: Our general comments since the main topics system design inputs you know sections 3 that's where you'll set up the whole sampling sequence for atmospheric data doing (inaudible) sampling procedures, you did indicate that sampling for wind speed is after you did the stability check, that's great because that conditions those type of wind speed like (inaudible) correct, but then you also mention that samplings for certain analogies are independent basis, I think it's great statistically but realistically it probably may not be always be the case since I done some work previously in the just statistics area that you look at a special correlation that's on the special skills, but in this is case the time is a threat, so the wind speed will likely have the correlation from time 1 to time 2 say if you say from one second to another second they could change that fast but likely there will be a clustering effect and that correlation I don't think is considered in

your scheme as far as sampling those events to generate your data, but how to do that I assume there is probably a data base available to help you determine those type of other correlation to further modify the way that you are currently doing in your (inaudible) processes.

MR. SULLIVAN: You raise a good point because you know one of the reasons I think that the stability and wind speed are not well you know correlation is not high is because for example if you have a two or three meteor per second wind speed that could happen with A or B or E and F, either sides of the spectrum could take away your correlation, but in reality there are some linkages. For right now we're essentially not recommended to -- we're not using the stability term as a variable in terms of uncertainty so that may be more of an issue in the future when AERMOD is connected then it is going to vary. There is a connection between wind speed and stability, it's much more than the other ones I think and in this context.

DR. WANG: Because the consequence would be that it's doing to drive the dispersion process you

have a burst of wind for a second converse to a wind sustained for say five seconds or a minute and then that's going to have a different, very different outcomes for the transport for the dispersion process.

MR. SULLIVAN: It's a little more complicated too than it is now with the distinct stability classes because if you do a turbulence where you're bumping up or bumping down your wind speed which will affect the turbulence parameters so it does get a little bit more complicated at that point, but it could be done but it's just a little more complicated.

DR. HEERINGA: Thank you very much. At this point if there are no additional comments from the panel I'd like to again sort of systematically go through the subparts just to make sure we feel we have touched on each of them.

We have addressed A, comments, we've had a good discussion of the treatment of calm periods, I think there will be some development to that. Dr. Portier's discussion on this I think and augmented with others, everybody is satisfied that we have covered part D, I think we have addressed part E as well, I think we've generally seen that as a favorable option in this. I had one question just out of interest on the use of rectangular fields, do you provide some compass orientation to those with regard to the wind or do you always assume the wind goes the length of the field and can the user --

MR. SULLIVAN: You mean in terms of setting up the model run?

DR. HEERINGA: Setting up the model run.

MR. SULLIVAN: Generally for example in California many of these studies have been done, the orientation is such the fields go almost exactly north, the growers are very good about that I guess with the GPS systems, but you can almost align your met station by how their rows are set up, so they almost always there go north/south.

DR. HEERINGA: I thought I heard that yesterday and I assumed that was the case when you mentioned the valley there.

Is there option in the program for other alternatives, let's say that a farmer plants north/south and you got a prevailing northwest wind?

MR. SULLIVAN: The flexibility is to describe the east/west and north/south components, so if they went east/west that could be handles, if they went diagonally it is not designed to know how to handle that situation.

DR. HEERINGA: Thank you. Mr. Dawson, are there any areas in this particular question that you feel that haven't been adequately touched on or you would like to see expanded on.

MR. DAWSON: No, I think the responses have been excellent, thank you.

DR. HEERINGA: Okay. Any additional comments from the panel?

Let's move onto question number 6 please and, Mr. Dawson, if you would read that into the record.

MR. DAWSON: Jeff Dawson, EPA. Soul fumigants can be used in different of the country under different conditions and they can be applied with a variety of equipment.

A. Does the SAP believe that the methodologies in FEMS can be applied generically in order to assess a wide variety of fumigant uses?

B. What considerations with regard to data needs and model inputs should be considered for such an effort?

DR. HEERINGA: Our lead discussant on this particular question is Dr. Seiber.

DR. SEIBER: Well this is the shortest of the questions I believe, I don't know whether that's significant or not. It's my feeling that the FEMS basic methodology and the mechanics appear to me to be applicable to use with other fumigants and other geographic regions with this fumigant as well as others, but because the physical chemical properties including volatility, water solubility, Henry's constant degradation rates in soil and air can very considerably among the fumigants, and by the way I take this question to mean is it applicable to other fumigants besides metam-sodium if I'm mistaken let me know.

MR. SULLIVAN: That's correct.

DR. SEIBER: Since they can very considerably FEMS should incorporate air and soil degradation processes as standard features in addition to the

volatility downwind dispersion characteristics that have probably receive and rightfully so the primary emphasis in the model development.

I believe Dr. Sullivan referred to the ability to put in deposition processes in the model, but I don't think really we were given any examples of how that would be done, but it seems to me that with other fumigants this one and others in other parts of the country and so forth this might become more important.

To the extent that a chemical breaks down in soil obviously it's less available for emission to the air and to the extent that it breaks down in the air or is deposited from the air to downwind vegetation or other surfaces it's less able to survive transport to downwind receptors, so I mean that's just a restatement of why these are important, could be important processes.

So that would be one comment, the second on regions of the country that obviously we all are aware they vary considerably in features like terrain and cropping systems and cultural practices and what I refer to as obstructions, I'm sure there's a better term than that, things that affect the boundary layer or rather the roughness layer, foliage coverage, trees as wind breaks, forests, hills, valleys and mountains, so FEMS will need to accommodate such local specific features to the extent possible.

Field shape is an interesting variable and actually the discussion at the end of the last question kind of got into this. The possibility of having irregular shapes of course is real, we saw an example of a center pivot irritation system early in the discussion and that would suggest maybe a circular field in some parts of the country or just plain, irregular shapes due to terrain, very common say in the Ohio valley and various parts of the country, maybe in Virginia, I'm not sure, so that maybe needs more attention.

I think you can approximate certain geometries with squares and rectangles, but I'm not sure about some of these other really odd-shaped fields. And of course it depend also on how the farmer, the grower tends to want to apply his fumigant, whether he blocks off a part of the field at a time and does it section or part-by-part.

But let me just ask that as a question, is there a way to approximate, have you looked at approximating really irregular field shapes fields with your rectangle?

MR. SULLIVAN: We haven't, but I believe it may be possible to allow the user to put in points you know that could then be connected in the programming to create an area source with the features in ISC. I think that could be done and I guess how many fields like that would be determining factor. The center pivot is a circle like you said and that could be approximated by a square, that one would be probably easy to provide that option. I say that and then my partner is probably cringing back there, but that's an easier one to do, but also the concept of having more flexibility in the code, for example if some fumigants did have an issue of atmospheric degradation half-life issues, that could just be a switch that what half-life do you want to use.

Similar to the issue about calms, no calm option, that could be an optional switch as well, so we could provide more flexibility -- all it is -- those things are doing are creating input files for ISC so we can put whatever switches we want in there as a user defined option, but give more flexibility like you're saying. And then regions though vary also in weather and soil type, soil microbial activity as air quality and I already mentioned field geometry deviation, so it will be important for FEMS to be calibrated to different growing regions in cropping situations with appropriate -- with field experiments that are appropriate to that region or growing system.

And those field experiments I feel should include determining flux not only by the back calculation method but perhaps one or more other methods and we've talked about this already either another model approach or aerodynamic measurement or some other stable tracer release perhaps, we didn't talk about that but that's another kind of field experiment.

And also these different conditions might affect sampling and I think Scott Yates alluded to this previously, when you're sampling through an absorbant like charcoal it's very sensitive or it can be sensitive to moisture, humidity because obviously charcoal will have absorptives, it can be covered over with water, so that needs to be taken into account as well.

MR. SULLIVAN: If I could just clarify one point, in terms of the regional variability I agree and having additional studies would be helpful and useful and, number 1, the models being submitted to EPA in the context of metam-sodium for now from the position that we are using upper bound emission rates and that's how it's being submitted at this point in time and you know if and when the manufactures choose to do additional studies as you suggest in other areas, cooler temperatures, different soils, the expectation is they would have lower buffer zones most likely in those areas, so until that point in time the assumption, it would be by the data being submitted at this point which tends to be upper bound, but the point is well taken, it will be different when more data is available, there will be lower buffer zones.

DR. SEIBER: And then the regions will vary quite a bit in climatic variables it might affect these things such as frequency and duration of rain and frequency and duration of fog. These things obviously can act to scrub out vapors from the atmosphere, wash them out in the case of rain.

In the case of fog, fog water if it partitions MITC out of the air it does a couple of things, one, maybe the more important consideration and this is beyond the model but something that EPA and health effects people need to consider it really presents a different means of exposure, people are perhaps inhaling aerosols rather than only vapors and they could even be exposed dermally I suppose if it was a heavy fog and you had fog water on your body.

Anyway, fog as an important consideration in coastal regions in the California central valley in the winter time but also in other parts of the country for certain regions and times of the year, so I think maybe it's worth considering and somebody commented earlier in our discussions, so they probably don't apply these chemicals during fog but I think they probably do.

MR. SULLIVAN: In some cases I believe they do. In terms of the modeling and you're representing different regions of course the fog that occurs during a radiational cooling periods at least the stability would be considered the fog wouldn't be, evective (ph) fogs like in California you're referring to wouldn't necessarily have to have a stable condition, so we would -- the model would not see that and anything that was removed by that fog from the air would be, you know it would not be seen by the model in that context.

DR. SEIBER: And then FEMS, the documentation that you provided included indoor versus outdoor air concentrations downwind and not clear to me how that can be modeled since it varies so much from structure to structure and individual homeowner's preference on whether they keep the windows open or use air-conditioning, but that really hadn't been discussed before but it is in the documentation that was provided.

Can you comment, is that something that will be part of the model or just an experimental variable?

MR. SULLIVAN: We have coated that into the model, we're not presenting it here, we don't feel the assumptions are ready and the infiltration rates and so forth, but the concept there was to provide a probabilistic approach to address things like you're saying, the tightness of the home, are the windows open or closed and to revive the context because what you tend to find when you run these kind of models where you link an ambient air model to indoor environment through an indoor model that on a short-term basis like one or two hours especially in a fairly tight home, the home provides a buffering agent against the peak concentrations, as every time increases at some point it doesn't make any difference, but I thought having the ability to have it in there to be able to approximate and we're talking about you know bracketing, personal exposures, indoor exposures as well as ambient could be useful to risk assessment manager, realize and they'll make the decision on the ambient side most likely, but with this additional perspective maybe in considering the margin of exposure or other things made to deal with.

DR. SEIBER: And then finally just a comment on the FEMS allows for multiple field applications via I think what was called custom runs in the documentation and this may be again more important in some regions of the country than others and with differing farm sizes, for example, if a region with smaller farm sizes there may be more potential for multiple applications, I don't know, but it just seemed to me that down the road you might want to consider making that a standard feature rather than a custom depending on you know whether that turns out to be the case in parts of the country, and that was all.

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

DR. OU: I suggested use (inaudible) and gone by the FEMS and compare the result with the California DPR model, California DRP model (inaudible), the result between the two model are comparable than the (inaudible), that's my suggestion.

MR. SULLIVAN: I guess the question to DPR is the availability of that data in the public domain, is

that (inaudible) data set mostly DPR AERB or is it privately owned?

DR. OU: The other suggestion enhance duration to be included with the (inaudible) one day or two day then you will know the (inaudible) automatically in to enhance the (inaudible) buffer zone.

MR. SULLIVAN: So your suggestion is to include a decay term in the model to account for breakdown?

DR. OU: The first whole value.

MR. SULLIVAN: That would be easy to do.

DR. HEERINGA: I think Randy Segawa.

MR. SEGAWA: Randy Segawa, DPR. In answer to David Sullivan's question regarding the availability of the methyl-bromide (ph) data, all of DPR's studies are posted to its web page which accounts for half to two thirds of available methyl-bromide data. The balance was conducted by methyl-bromide (inaudible) which would be able available to most people under a public records act request.

MR. SULLIVAN: That would be available you say?

MR. SEGAWA: To most people, yes.

DR. HEERINGA: Thank you very much, Mr. Segawa. And I think at this point in time I think we have to wrap up, we have Dr. Shokes and Dr. Yates.

DR. SHOKES: Fred Shokes. And we'll wrap it up quickly here because most of the things that Dr. Seiber said are the things that I have in my notes relative to the model. I see some real benefits to the model and that it could work on individual fields or sequentially on multiple fields. The way that it works it looks like it could definitely I think could be appropriate, that was questioned earlier the appropriateness of moving it from one location to another, but I think the real appropriateness there to me would be the appropriateness of the data that's input into the model that I would agree with Dr. Seiber that those inputs need to be more local or regional so that they pertain to the area of the soils, the climate, the field shapes, field shape is a consideration.

I know in some parts of the country field shapes are pretty much square or rectangular where people deal with whole sections where they're farming, but then in some areas of the world they're determined more by where the local creek is running or the local swamp and the fields tend to be much smaller, so this would be a definite consideration and also that brings us to the obstruction aspect which is going to affect your wind currents and things like that so it needs to be some consideration for things like that and those I suspect would be somewhat difficult to model in every case.

MR. SULLIVAN: If you were doing regional assessments they have the southeast as a region and identified a number of meteorological stations say from 10 whatever the number would be, some of those ideally would be in areas that had more obstructions and more tree cover, some would be in valleys, some maybe in a more coastal area, just cover the gamut that you expect to see in that region even knowing you wouldn't get everyone, but that range would be sufficient to be hopefully acceptable.

DR. SHOKES: Yeah, I think so, I think that would definitely would be a factor there and beyond that I think that some measurements of the flux rates in those different conditions would be essentially to improve the model, and the only way to really determine I think the appropriateness of it in different areas is going to be get some of those data sets and input them for different areas and do some validation and I think that would really be the proof that it would work and that would be essentially I think.

DR. HEERINGA: Thank you Dr. Shokes. Dr. Yates.

DR. YATES: Most of my comments have also been addressed so I will just want to make a couple, I just want to emphasize a couple of things, I think that there should be guidance in the document to indicate situations that the model may be either inappropriate or it may be questionable to use them, some of the terrain issues and like a field in a densely wooded area, how you would deal with that.

Also with respect to using the model over regions or states you know, again, I want to emphasize that the input parameters need to be representative of that region or state and the uncertainty information needs to be representative as well.

Other comments deal I guess most with data needs. I think that there needs to be information for some of the potentially -- potential methods for reducing emissions. You're already doing that with the surface sealing which I think is pretty good, although there is a potential consequence of surface sealing which I will get to in a minute. It doesn't seem it would be very important to look at conducting studies with high density polyethylene film covers since the emissions should be approximately the same as not having the high density polyethylene film there because the permeability to MITC is very, very high, it's the highest of all the fumigants that we've tested and the number is so high that it's almost the same as not having the film there.

If you look at a modeling, if you put the film there you don't really get a concentration buildup underneath the film, so it's basically the same as if the atmosphere was there. For virtually impermeable films on the other hand there's a very good, they're very good in terms of the permeability, the numbers of like a hundred or a thousand times less somewhere, you know it's so impermeable that you will get -- you should get the emission reductions, I won't say you will because it seems like when you do things in the field there's no guarantees, but that would be conducting studies to get information on fumigation with virtually impermeable films would be helpful.

And then the other thing getting back to this idea of a surface water seal, there is, it seemed like over the last couple of days I've noticed the more I think about things the more I see potential problems that occur that aren't probably going to be picked up by the data or the measurements, and sometimes maybe would just not you know, just might be overlooked and surface sealing is one of those things.

If you are sealing the soil with water in an area you -- and the water seal is put on the -- the hot soil starts re-evaporating which stable atmosphere over the field and meteorological probably located on the field is measuring things that might indicate that

the atmosphere is unstable and yet over the field it's stable, so in a sense you're confining, the stability would be confining the concentration that gets into the air in the area over the field and so you sort of have a cap on it you might say until you get to the edge of the field and then you move into a dry soil that the atmosphere starts turning -- changing from a stable to unstable and yet I don't think that any measurements you have would point this out and the only way you would really would ever see it is if you irrigated the soil underneath your weather station or maybe you might want to think about sometimes when you're running a study in the airy climate to put some temperature sensors in the field to look for inversions, but that's the kind of thing that you know it's not something that people generally think about but could have a significant effect on risk to someone near the field during the daytime when you generally would think that well there's so much atmospheric mixing that it wouldn't be a problem, so what you would be relying on is the mixing from the edge of the field to the bystander is sufficient enough to take that problem away I guess, but the model I don't think could handle that.

MR. SULLIVAN: Well the model can't handle multiple stability regimes interjectory, but I think the issue is if you're in an arid climate like Bakersfield in the summertime that you're absolutely right that the actual wet surface is going to have somewhat different characteristics than the rest of the fetch, go and talk to the bystanders, but I wouldn't say it would be stable, I mean the situation -- it would be less unstable than the rest, but those surfaces are so unstable during the hot summertime in California, you seen dust devils, huge dust devils all over the place and so you're going to be damping that down a little bit, it will not be as unstable, but once you get you know part way across the winds, part way across that field it's still going to be an unstable situation in my view.

DR. YATES: Well I think with the winds, the warmer winds coming in over the field and then the cool air underneath I think it actually would be, you know you would have cooler air at the surface then you would have up above. I know a micrometeorologist who in some of the discussions that we had with the -- with our early field experiments was describing this to me and he mentioned some references to this kind of phenomenon and I could provide those, I won't be able to do it until I get back.

DR. HEERINGA: You could include those in the minutes.

DR. YATES: Yeah, I'll do that.

MR. SULLIVAN: But in the upwind edge of that field like you're saying it would be that situation, but as the wind blows across that field it's equilibrating to the field and it's building up, they call it internal boundary layer, about a ten to one feature, and so as it's gone well into the field it's within that adjusted situation and that dealt temperature issue becomes much less of an issue, it's right at the edge, the upwind edge is where that boundary is much more important. I would like to see your paper, it would be very useful.

DR. HEERINGA: Dr. Ou.

DR. OU: I have a comment with Dr. Yates comment, the (inaudible) to MITC and the (inaudible) to MITC.

MR. SULLIVAN: But one question, Scott, is the issue of water and in the terms of let's say that you had an application that was, the soil moisture was at the upper end of the label in the (inaudible) capacity that the ability of the top to retain the water which would retain the MITC, what are your thoughts on that?

DR. YATES: Scott Yates, Riverside. You're talking about putting a tarp over the field and condensation on the bottom of the tarp?

MR. SULLIVAN: Correct.

DR. YATES: That's interesting that you'd bring that up, we were curious about that. I had a colleague working with me a while back that did a study where in our permeability cell she actually put a layer of water over the top of the cell and then used that as the injection cell, so it would be basically the same idea of having condensation underneath with the chemical coming up from the bottom. And we expected that that would drastically improve the restrictive barrier and as it turns out it has no effect, and we have some ideas as to why, but to be honest with you it seems counter-intuitive, however I guess the way I look at it is that MITC partitions into the water very readily, so the concentration of the water phase will be much greater than the concentration in the gas phase because of Henry's partitioning, so what you end up doing really is increasing the concentration at the tarp, and so that ends up compensating for the fact that defusion through the -- I mean the defusion through the water should make the barriers better, but you increase the concentration gradient which offsets that so in effect it doesn't matter.

DR. HEERINGA: Dr. Seiber.

DR. SEIBER: This is Jim Seiber. In that reasoning then does that mean that would be the case for MITC, but what about a more hydrophobic fumigant?

DR. YATES: I think that this is going to happen with pretty much all the fumigants, it would be, MITC would be most affected by it because of the

Henry's constants, the smallest, but you still I mean

even if you, methyl-bromide you know you're talking

20, 25 percent for the Henry's constant, you know that's still, you're still increasing the concentration right at the boundary and I would think that it would be unrealistic film thicknesses I mean, if we put two or three inches of water that might be different, but you -- condensation's not going to do that, it's going to be on the order of a millimeter or two, so basically you know this seems to be just the way it is and it's unfortunate because you know we were hoping that with the you know condensation does form underneath the film and we thought that would improve the barrier properties and add no cost to the grower, but it doesn't.

DR. HEERINGA: Okay. Are there any other comments that panel members would like to make on question 6? Let's just make sure again that we have covered each of these two points satisfactorily. I think that with Dr. Seiber's introduction and the follow-up that I think each of them have been addressed. Mr. Dawson?

MR. DAWSON: No, we have no further clarifications, thank you.

DR. HEERINGA: Okay, at this point we finished question 6 and it's just before quarter after twelve, I would like to call for today a one-hour lunch period and resume here at 1:15 when we will address question 7 and question 8 and conclude the two-day session this afternoon.

Thank you very much for everyone who has participated this morning.

(Lunch break.)

A F T E R N O O N   S E S S I O N

DR. HEERINGA: Welcome everyone back to this conclusion of our two-day meeting of the FIFRA Scientific Advisory Panel Consideration, the topic of fumigant bystander model review focusing on the FEMS model system with metam-sodium as a case study.

At this point we have completed out initial comment session on directed questions 1 through 6 from the Agency and I think that we're ready at this point in time to move onto question number 7.

Mr. Dawson, if you would please read the question into the record.

MR. DAWSON: You will have bear with me a second -- I'll just read it because Dr. Wang had asked me to post some additional slides, but we're having a technical issue so I'll read it from the paper here.

Does FEMS adequately identify and quantify airborne concentrations of soil fumigants that have migrated from treated fields to sensitive receptors, that's question A, question B is the Agency is particularly concerned about air concentrations in the upper-ends of the distribution, these results presented in a clear and concise manner that would allow for the appropriate characterization of exposures that could occur at such levels.

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

DR. WANG: Well hopefully we can get our slides on there sometime soon, I'll start talking.

DR. HEERINGA: Please take the time you need to.

DR. WANG: The goal, ultimate goal of FEMS is still to you know to protect the bystanders from fumigant exposures and to this end FEMS is that forward by increasing the capabilities of the EPA says C models in reaching assessment, especially in the delineation of the possibility of the buffer zone for MITC dispersion, the sensitive receptors.

It appears true that the FEMS can identify and quantify of site airborne concentrations of soil fumigants, but it's not quite clear how adequate the predicted concentrations are and how efficient the program is. The FEMS model did show reasonable

comparisons with, between predicted emission fluxes and measured concentrations with an r2 of .65 I believe showing the figure 19, the report, that's for the chemigation scenario and then the r2 of .53 which is figure (inaudible) in the report for the shank injections scenarios we discussed r2 issue before, you know what's acceptable .5, .75, or .9, but at least it's not .1 or 2, so it does show that potential, it's probably reasonable to be quality table on that.

The FEMS model also showed strong dependence of airborne concentrations with wind speed and that's a plus especially in the report it's a figure of 20 showing the higher concentration was observed at a lower wind speed, it's definitive data so it's great, however this may be a result of the calm options being used in the (inaudible) simulation, so which one is attributed to that outcome, is it SC or is it FEMS, but maybe it's the same or since FEMS is producing that atmospheric inputs so either way it's generating those type of data will identify the magnitude of aerophase concentrations and that's the main interest in this case anyway.

To answer this question is still difficult to do an absolute yes or no answer, it's a difficulty for providing a yes or no answer, it goes to the lack of independent validation of the FEMS model, we have discussed this a few times already. I still think that an independent modeling of field studies likely will help determine the adequacy of the FEMS model for identifying and quantifying the true airborne concentrations that likely we are interested in.

The second part of the question 7 to B, this is if you remember back, this is very similar to the one we discussed yesterday afternoon, the question 4F, but I can elaborate a few more words on that again. The C model is -- I keep hearing that its claim is to be able to capture the upper-ends of gas concentrations, well if this is indeed the case then the FEMS model should have the potential to do the same since it's using that as a way to predict the dispersion, the distributions of the gases.

Furthermore, with the added capability of this problemistic (ph) assessment of weather variations I think FEMS model should be more useful for risk assessment so it's a step forward. Since the question is on the clarity and the conciseness but I believe the presentation of this model on the clarity and conciseness of the procedures and results to derived from this FEMS model can need another improvement probably especially in the reports it can be tightened up.

And it's not very clear to me that current status of FEMS can achieve the claimed results without the person that's spending more time on studying the program, it has a lot of capabilities but it probably has, require more effort to really fully study that.

I also brought up earlier by Dr. Seiber since we're talking about the clarity and sensitivity issues in the upper bounds that for sampling, a short sampling duration will increase the probability of capturing the maximum concentrations we call upper bounds, as you phrase in that question, and again that poses some practical issues, I mean it's cost and labor requirements but scientifically that would be one way to get at it.

The other thing on this upper bounds issue is that the case study associated with the presentation of the FEMS model was conducted in Fresno, California, and that particular locale does represent one of the extreme cases that favor fumigant emissions and outside transport as discussed earlier, so in that regard I agree with the claim on capturing them, the upper bounds, it's appropriate using that as an example, but how this may be transferred to other locales I think remains debatable, we have been debating that.

I have a couple of slides, I think it's probably interesting to show you especially some of those has had a personal experience working with fumigants in the past.

MR. DAWSON: Do you want to go right to the first picture or the first slide?

DR. WANG: First slide please.

Injection of a chemical in the soil will immediately, say like metam-sodium relates to this conversion and must change to gas, MITC, there will be dissipation or defusion, it's going to be moving away from the source I'm using an arrow to the right it's

actually going out three dimensions from the source where it might be applied and then there may be leeching if you're watering that and degradation would be occurring simultaneously, but the interest here mis moving upward, the gas defusion that's going to generate the fluxes we're going to see.

Next slide please. So in the field scenario this is a shot from an experiment that we did in 2002 in a forest nursery. In that particular experiment we used waypam (ph) which is a form of metam-sodium and also bazemet (ph) supposed to produce this MITC products.

Can I have the next slide. This is applying a waypam, we can see there's a container in the front of the tractor with tubes going to the back, it just gets sprayed on the surface and then incorporated in the soil, that's metam-sodium application.

The next slide please. In that we actually measured the fluxes directly from the (inaudible) using a flux (inaudible) in this case, it's like a container, it's one of my students -- you can see in the distance there are other crops, actually

(inaudible) only that's probably 50 feet, 60 feet away and then there's resident houses a couple hundred feet, you can see there's a little red dot in the distance and there's some other residents around behind me while I was taking the picture, so it's in the middle of a forest almost, but you see it's very common actually so it's a real issue, how the gases may go.

In fact this leads me back to our earlier question on the final toxicity analysis with a gas density even flow since there are occurrences that has been reported in some other locations applying MITC in the location the land is not that flat and when the inversion layer occurs at night the gas can flow along the land surface by gravity to places where there's crops and plants still actively growing, and then that causes tremendous damage because the (inaudible) toxicity.

Can I have the next slide please. Also we were able to monitor weather data in that scenario, we have the air temperature and wind speed and the barometric pressure so it's highly, highly variable

you know for that duration you can see it changes --

there's some (inaudible) trend but those factors will dictate how -- I mean I didn't show the direction in this particular slides, but that will translate to which direction and how far the potential gases may move off site.

Can I have the next slide please. I think this is the last one. These are the emission flux density models measured over time and tremendous (inaudible) in that case we do have replications for each treatment, so each dot is an average of four plows you may say in close proximity and I did not show is the standard deviation (inaudible) and that's quite large, and (inaudible) a general trend and since we were using a two to three hour interval so we were able to pick up some of the peak status very high and some were low even at the beginning the first 24 hours you see a tremendous variation in terms of flux values and magnitude is very small in this particular case, it happened to be with probably cool temperature in that environment we did this and so I think that's the last one isn't it, thank you.

DR. HEERINGA: Thank you, Dr. Wang.

Our next discussant would be Paul Bartlett.

DR. BARTLETT: I concur a lot with what Dr. Wang said earlier on question A. It's a difficult question to answer in a yes or no and we've been answering aspects of this throughout the day and yesterday because basically it gets at the question of variation emissions, variation and deposition transport -- I'm sorry, not deposition -- air concentrations, the phenomena itself is hard to quantify and characterize in terms of what the adequate buffer zone might be, where it might be located even if we have perfect instrumentation, so given all those and this is a step forward, FEMS, from what I understand the previous methods of determining buffer zones is trying to make a lot of the uncertainties much more transparent, trying to communicate those uncertainties through probability distributions and other methods and sensitivity analysis, and I think our the discussion the last two days has brought forth a lot of other uncertainties that may be difficult to quantify or compare at this stage.

The fundamental difficulty of answering the question whether it's adequately identifying and quantifying the concentrations is we don't really have much evaluation of the particular field experiments we have, we don't have something that we traditionally have in these areas where you have one method of doing emissions and then you have an air transport model and then you take measurements to see how well you're predicting and you're not supposed to be calibrating your model, you're supposed to be understanding what made your model go wrong, make better predictions and then the next stage around see how well you d, so unfortunately we don't have that process available to us.

The other way of course approaching this is to do lots of measurements so then you know exactly where the buffer zone is and direct measurements of emissions, so there are different ways to go about it and there is of course the soil modeling as well which could be modeled and measured, so it would be good to see as FEMS moves forward in which a lot of new models

do when they move forward is to engage in comparative modeling analysis to have more testing, some sort of validation of testing going on.

One intermediate step we had suggested earlier in discussions outside this particular question period was taking subsets of the data and seeing if you -- how well it predicts the outer areas where we'd expect the buffer zone to be. It's somewhat of a fixed game in some sense because you're using the same model to make the emissions backwards as you are going forwards, but then you get some idea of the uncertainty of predicting the buffer zones outward, even though again it may not be adequate measurements to do that and I think it's been referred to as a jackknifing technique and something that could be done with other data sets.

And this question is pretty comprehensive and I think generally we've discussed a lot of these issues earlier. The question of exposures which is the second question is it appropriate to characterize exposures, and I realize that we're given the parameter of a 24-hour exposure and the history, and I brought this up earlier in the beginning, is that a lot of the history of a lot of the toxic substances that we've dealt with have had a traditional 24-hour exposure regulation but since then we found one two-hour acute exposures to be more significant. And as we talked before if we change our averaging period less it goes up to 24 hours, we're not really adequately capturing an exposure if there's severe diurnal variation, what we're really interested in is when are the people out there and what are they being exposed to at that time, and I believe that's the information that toxicologists and people working on the health effects would like to have.

So I like that this model has the capability of giving periodic exposure, so even though the call of the Agency is 24 hours that I see in the next five or ten years will be much more concerned about those successive acute exposures, even a lot of times it's not one single exposure sometimes it's a question from what I understand of you know repeated exposures over a few days which as we saw the peeks of these four days you can get exposure due to different conditions, even a lot on the second day if you hit those areas so

I think the information for exposure beyond 24 hours of the period that it's being exposed to and also the shorter period is important to preserve which the model has capability of doing which is a good step forward.

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

DR. BAKER: I think in the earlier discussions we agreed that the FEMS approach was basically sound using ISC linking that with tox taking the high end if not the maximum of the emission fluxes, so I believe it does adequately identify and quantify airborne concentrations of soil fumigants, issue two sensitive receptors to the extent that sensitive receptors will be identified for particular fields I think the answer to that is yes.

And the question is in the context of results it was meant to several of the features of the model the results were discussed but currently those capabilities are being worked on, so you know the results that were presented I think were reasonable.

For question B, again a little bit more

comprehensive, now we're talking about the upper ends whereas I took question A to be just the general approach of predicting airborne concentrations, the total say concentration field, now we're looking at the upper ends of the distribution. If I could just recount a little bit what has done a five-year temporal variability is captured in a data set from a weather station, we talked about multiple weather stations could be looked at, that would give you geographic variability either local or regional and then we look at uncertainty and I think the FEMS model is unique in that it does look at uncertainty and it keeps it decoupled to the extent it can from variability, so I think that's a positive and that's an issue we were looking for being decoupled in the previous two days or the Tuesday/Wednesday discussion trying to keep variability and uncertainty separated.

But we've raised some questions about how uncertainty is introduced and whether that was a complete picture, uncertainty in this case is being introduced on an hourly basis to generate hourly

extremes of the met values, to the extent that the hourly extremes and the met values drive the risk decision making process, this procedure captures that feature but we did raise the question is it simply the hourly extremes that would drive the risk making decision and some of the thoughts they came up, they may or may not be complete but directional persistence in the five-year record and low wind speed in the five-year record, persistence of low wind speed, and it was felt there's concern about chopping up those records, the persistence in those records.

So to the extent persistence in a feature like direction and low wind speed drives the risk decision making process that hasn't been captured and some ideas were presented as to how that might be looked at and I think we would use up more time than is allotted to try and work the issue now, but I'll just raise the issue that hourly extremes are only one feature and we've captured that.

The uncertainty we've introduced I'm happy see uncertainty introduced, the uncertainty we've introduced I think in the expert elicitation focused on the National Weather Service Stations if other stations like the FAWN system or the CIMIS system is looked at that might have to be revisited not in another elicitation but just considering the records and do the uncertainty bounds have to be expanded. I think that was the main point I wanted to make, thanks.

DR. HEERINGA: Thank you, Dr. Baker. Any other comments? Yes, Dr. Majewski.

DR. MAJEWSKI: I would like to make a comment on Dr. Wang's field study results in reference to field verifications. He had what I consider real good data and because he replicated it he's got many data points for the same sampling period. In the aerodynamic method you pretty much only have one and he mentioned that the error bars associated with each point were very high and they are associated with the aerodynamic method can also be high during some periods. And if I'm not mistaken I think I heard yesterday that the error associated with or the uncertainty associated with the emission source estimates using the FEMS model was about 40 to 50

percent, is that reasonable?

DR. BAKER: Fair assessment.

DR. MAJEWSKI: So I guess and I don't really have a question, but I guess it's just you know there's uncertainty associated with all these verification methods, so I guess we have to keep that in mind.

DR. WANG: If you want to know that coefficient variation for those sampling points varies somewhere between 30 percent to about a hundred percent.

DR. HEERINGA: Thank very much, Dr. Wang. Dr. Baker.

DR. BAKER: Previous recommendation also was to try and keep separate the emissions from the meteorological issue, so I agree there are uncertainties in other areas and to the extent we can keep them separated and studied individually that would be important, eventually you'd like to put the whole thing together, but decouple it as much as possible.

DR. HEERINGA: Thank you very much, Dr. Baker. Steve Heeringa, just a point of clarification on your previous, Dr. Baker, just for the minutes, I think your concern is about sort of a serial association of weather events and that by introducing random variation on an hourly basis that we might be essentially attenuating serial correlation that's important, is that --

DR. BAKER: Right. You will generate hourly extremes but do hourly extremes drive the concentrations of concern essentially that would drive the risk decision making process, if there's a persistence in wind direction not wanting to work the issue but to give an illustrated example, the block of time could be looked at and that block of time has uncertainty associated with what actual direction is but the persistence could be a true signal that you don't want to lose, similarly the low wind speed persistence in a record has uncertainty associated with the wind speed but the fact that it is persistently low is a true signal that you don't want to lose and you want to test those features to see, okay, do those drive the concentrations that ultimately affect your risk decision making, I can't say (inaudible) they do but I also can't say (inaudible) that hourly individual extremes are the most important for driving the risk decision making also.

DR. HEERINGA: I think that's a very important point and I don't know, Mr. Sullivan, whether you have any comment on whether the model -- you have hourly inputs on meteorological data so unless you somehow model that serial correl (ph) it's sort of difficult to smooth over these changes from period to period.

MR. SULLIVAN: I think it's something that can be looked at and compared, the original data set versus the passes through there, I mean it's a good point because you can break some of those up and the -- considering the uncertainty for wind direction as an example can go either way, it can increase your buffer zones or decrease them, one option that a risk manager would have would be to look at the benchmark results which would be straight, if you put all the switches to don't put uncertainty in that's your

benchmark, when you do put in the uncertainty for emissions wind speed and wind direction, that's an alternative look they could elect to say also which one has the higher values to be safe that avoid the issue that Dr. Baker's mentioning in case it tend to disrupt some low wind speed events the benchmark would be a fallback to ensure that didn't under estimate the risks.

DR. HEERINGA: Dr. Baker.

DR. BAKER: The only other additional issue would be do uncertainties in the persistence of direction or low wind speed amplify, they could amplify in some cases or moderate the extremes in the hourly met data, so you couple those two together you may have a little bit more extreme that setting all this the switches to zero and doing the hourly doesn't quite capture the combined impact that it could have.

DR. HEERINGA: Excellent point. Dr. Ou.

DR. OU: In Florida we apply the metam-sodium by two (inaudible) one is the (inaudible), the other is the drip (ph) application and one or the experiment we (inaudible) and also put some emission on 20 centimeter away from the center and 40 centimeter away from the center, (inaudible.)

MR. SULLIVAN: In terms of the shank injection studies that I showed those were bedded applications, so those were for (inaudible) and they were bedded portions as well. In terms of the modeling you know we are assuming in the composite sense that's it's a uniform emission rate and don't try to separate out the rows from the beds, that could be done but in expectation it wouldn't change the overall buffer zones, it would tend to average out.

DR. OU: (Inaudible.)

MR. SULLIVAN: On a localized basis that would be correct, but again generally we are quite a distance from the field and the objective by going further away is to avoid near field effects like that and to be monitoring the signal from the composite plume, I know that's the basic objective here.

So I think that once you know you do get in these GOP studies a hundred meters, a hundred and fifty meters away that all those non-homogenous natures of these things tends to become less of an

issue, but you're absolutely right that can produce localized issues near the field.

DR. HEERINGA: Dr. Ou, just an administrative note for the record, we do have a copy of the TOXST documentation and if anybody would hike to scan that to answer a specific question related the their comments it's available here and just motion to Paul and we'll have it brought over to you.

The Hanna 1995 paper, it has been located and is being photocopied, Steven Hanna I believe has been located and will be distributed shortly, 1990 paper we're still trying to locate -- 1998 we're still trying to locate at this point, so just for the record those documents are available here or will be available shortly.

Any other comments on this question? Let me just, before we move on then let me just make sure we review and summarize. I think with regard to -- we've had a substantial amount of discussion about the transference of this model to other crops and other settings and other regional, but in the context of the

Bakersfield case studies and Bakersfield field area

central valley-type applications I think, does everybody feel comfortable with their responses to this?

Let me just ask a question of the experts here too, are we all as a group convinced that this model as implemented in each aspect would represent a worst case across the nation for other applications conditioning on application method, is that too extreme?

DR. SPICER: Tom Spicer, University of Arkansas. I would be concerned about that especially given situations where you might have drainage flows and those sorts of things, especially applications later in the day where the flux would be higher and so I understand the point about Bakersfield being a worst case as far as temperature's concerned, but there could be circumstances where it would not be the worst case.

DR. HEERINGA: So other than topographical and meteorological exceptions it might be very local. Dr. Yates.

DR. YATES: I would tend to agree, I think that the location being in a warm-aired climate in soils that tend to have low organic matter is putting the experiment in the right place, but until there's several experiments that have been conducted for each particular fumigations type in the same area you really don't know and until that information is there I would not feel very comfortable saying that it is a worst case.

MR. SULLIVAN: One thought that I had that might useful in the final report would be to take the data collected in Bakersfield in the fall of 2002, September, early September 2002, which was intermittent sealing for shank injection and chemigation, two concurrent studies, that that may help or at least provide another -- a replication of it under similar conditions.

DR. YATES: And to continue with it a little bit that would, having more experiments in the Bakersfield area might be a worst case for the San Wakeen (ph) Valley and maybe the Imperial Valley. I don't think this is even an appropriate study to be applied say in Minnesota. I think in Minnesota you need to go there and run some studies and find what

Minnesota's worst case is or maybe various regions in Minnesota, so sometimes we're a little bit flipid (ph) with worst case, you know find the worst case and apply it everywhere, that it really isn't a reasonable way to do it, it needs to be dependent on the location, the time of year, a variety of things like we've talked about.

DR. HEERINGA: Of course, thank you very much for that. I think that's a good clarification at this point.

Mr. Dawson, are you satisfied at this point that we've covered this question, do you have any specific --

MR. DAWSON: No, I think we're satisfied.

DR. HEERINGA: Okay. At this point then I would like to move onto the eighth and final question, it's been directed to the panel by the EPA and, Mr. Dawson, if you could read that into the record please.

MR. DAWSON: Question 8, a sensitivity analysis has been conducted and is described in the

FEMS background document. What types, if any, of additional contribution/sensitivity analyses are recommended by the panel to be the most useful in making scientifically sound regulatory decisions.

Part B, what should be routinely reported as part of a FEMS assessment with respect to inputs and outputs.

Part C, are there certain tables and graphs that should be reporter.

Part D, What types of further evaluation steps does the panel recommend for FEMS?

DR. HEERINGA: Okay, our primary discussant on this is Dr. Yates.

DR. YATES: Okay, I see this question as being composed of at least two parts, first of all I think this is trying to address or at least I can see how this should be used to address the manual and the description of the FEMS approach where sensitivity analysis in a sense would be looking at model performance and how sensitive are outputs, how do they depend on the inputs to the model.

And another part of it seems to be looking at when you use FEMS in an assessment you know its nice

to have some idea of what kind of variation would be in the data that might affect output results. So let me, so I'm not really going to be able to address this in like an a, b, c, d, so when I write-up my comments for this section I'll try to put them in that form, but for now it's just going to be kind of the way I've them on the piece of paper here.

The report includes an appropriate discussion of sensitivity of various input parameters, they show correlations between inputs, they show a number of things of like scatter diagrams and there is discussion that talks about how the correlation and the uncertainty between various input parameters would affect results, in particular the scatter plots, I know there was quite a bit of text in those.

All these examples are really specific to the study that's described in the document and I think it would be good if there was more or less what I would call a true sensitivity analysis added where you change the input variables by a certain amount given some kind of standard case where you change the input variables by say 10 percent and look what the change in the output would be, and of course you would have to determine what output you want to determine the sensitivity of and I think a logical one would be a buffer zone given some you know standard case.

I think this would help the person who wants to use the model to focus in on the inputs that are going to really be important and you know what you might be able to get out of the data that you currently have, but some of those things could be specific to the fumigation type that you're looking at, so you might just want to look at a more like numeric-type sensitivity analysis.

There was also a fairly detailed analysis that looked at what the buffer zone would be as affected by the stochastic treatment of input variables. Several cases were given from the benchmark without any stochastic treatment to everything being stochastic. I think this is pretty useful information, so that should be retained I think.

Yeah, I had a comment which I think you know we discussed earlier about there really isn't any information given about using the expert elicitation survey and how that would affect the sensitivity of the results, but you know we've already addressed that so I won't really say more. And there isn't any information looking at uncertainty in FEMS in a predictive mode, and I know that's already been addressed a little bit in the previous question about using jackknifing would be one way to do that, but I think that there needs to be the use of an independent data set to do the analysis and then compare it to the measured values somewhere in a second data set and somehow you know give the reader confidence that this will work, that it's a fairly robust approach to looking at risk assessment.

One other thing, this is a thought that I got this morning from the discussion, this idea of the mass it seems to me that some kind of an analysis needs to be done to investigate how much mass the model will, given the fact that you are doing an uncertainty-type modeling approach, you know when you run through this kind of analysis at say the 2.5

percent, the 50 percent and the 97 percent, the 97.5

percent level, that the 97.5 percent are you saying that you know more than a hundred percent of the mass is being evolved -- I shouldn't say you, is the model saying that more than a hundred percent of the mass is being omitted into the air and if so then something has to be done because that physically that doesn't make any sense.

MR. SULLIVAN: We do not allow any of those percentiles to exceed the available mass, the overall available mass. From the discussions we've had here it would appear a very easily change that could be made would be to confirm that every time there's a simulation going on of an application, that during those four days when it did hit had actual, total amount available it shut things down. At this point I believe it is conservative, it can be reduced to some in that sense and also the way the fit was dealt with the log-normal we do of course have askewed distribution on upper tail, is that conservative in that sense, I guess nobody really knows.

I mean it seems to me that where the -- you can't have negative numbers -- that it would be

reasonable to assume it could be skewed in the top, we've modeled it as if that's the appropriate thing to do, and so in some cases that may be overpredicting, but at least we could make sure that during any forward day period we're not putting out more than what's available.

DR. YATES: But then it would seem to me that over the four-day period, I mean I would think it would be highly unlikely that you would have too many experiments, I mean I would say less than. I mean I can't imagine that you would have in four days a hundred percent loss of the chemical, so if you are running through the statistics somehow after you know a day you've lost a hundred percent, I wouldn't consider that physically possible.

MR. SULLIVAN: The most recent assessment we've done of that, we've seen a range, the lowest was available, after four days is 14 percent remaining and the highest was approximately 80 percent remaining depending on how it was sealed and so forth, but that's the kind of range that we expect.

DR. YATES: Well I think though that at least in the documentation or there should be some kind of an example run where the reader and the eventual user of this can have confidence that mass will be distributed throughout the experiment in a way that seems to make sense physically.

I got a couple more things, in terms of things that should be reported, one thing I think would be nice is if there was some kind of a graphical, like a Windows interface that had maybe for the kind of curves and graphs that are output that it would be -- the user would be able to kind of check boxes and get the kinds of things they want because I have a feeling that there'll be a variety of people that will use this and their needs will be different and dumping out all sorts of information, you know it's not necessary is kind of inefficient and sometimes can be somewhat confusing.

But some of things I think you should probably show would be or make available I should say would be graphs of the probability distributions for the input variables, let's see, of course outputs of contours in a field that indicate like the buffer zone

and the contours that indicate the zones of say like the 95 percent level, I think those are all very good, and one thing I didn't see which I realize now is because it was in the model or in the documentation for the model we talked about before, but they had curves of probability versus buffer zone distance I believe it was, I don't the manual with me right now, so that you could kind of go up to like the 50 percent level and you can look that what the buffer zone would be, so you could kind of compare things all from data on one particular graph, that would be useful.

Some information on rare events where maybe you sample from higher in the distribution you know might be useful, the correlations that you show I think are pretty good and a table of correlations between input parameters is good. Let's see, but I think that at least for the types of tables and graphs that are going to be reported you know that to me I think would be you need to get the input of the regulatory community because clearly this is going to be most useful to them and I think whatever they find useful. It seems to me that the methodology is general enough that you could pretty much produce any kind of information they might need, so I guess with that that's most of my comments.

MR. SULLIVAN: Thank you, Dr. Yates. Dr. Ou.

DR. OU: Well since I'm not (inaudible) I have one (inaudible) comment, it was one that the sensitivity can result are the most (inaudible) to the emission and I'm not surprised about the (inaudible) since the -- you know there's no emission, there's no (inaudible) to be concerned and there's no buffer zone to be established, so that's my comment.

MR. SULLIVAN: Thank you very much in focusing on the sensitivity analysis on the emission modeling. Next, Dr. Shokes.

DR. SHOKES: I don't have any other comments to add to what they have.

DR. HEERINGA: Very good, thank you. Dr. Portier.

DR. PORTIER: It helps to go third because Dr. Yates said most of what I had written down, so I just deleted it, so you don't have to worry about it.

There is a protocol for doing a formal sensitivity analysis and I use the same phrase he did, a true sensitivity analysis where looking at how changing levels of key model parameters related the changes in the end response, and as I was sitting here I just started listening down what are the parameters, the model parameters we've been talking about the last couple of days, there's the flux estimate uncertainty which the model currently takes into account, there's wind speed perturbation, wind direction perturbation that have distributional parameters that could be played with.

At this point you've put one distribution in, but those could be looked at as distributions on the parameters themselves and you can kind of play around with how sensitive the model is to how you change what you do on those perturbation distributions. Something we haven't talked about is the receptor grid density and what does that do to the buffer distribution if you get a more dense receptor grid, that's something else that could be looked at. Days of off-gassing, we're assuming four days, but it may be six or three might actually change those results and we don't know

the impact of that averaging times whether you're using four hours, two hours, six hours, we've talked a little bit about some of that.

We just talked in the previous question on this interpolation method and if I remember correctly there's two way to do that interpolation the way you did it and then they way that a chemist might have done it, we might want to see minor, whether that made any differences, I think that's way down on the list but it's on the list. There's the degree or auto-correlation or persistence in the wind speed and the auto-correlation persistence in the wind directions, and that's another level above just changing the pertubation parameters, but it's actually building-in this climatological persistence to see what the impact of that is. You know as I go down this list, these things are kind of less likely to be impacted.

And then there's this concept of degree of persistence and stability. We've kind of talked a little bit about it, but since stability is a class variable it's not quite sure what do we mean by

persistence in a class variable the ability of the model to allow it to change from state-to-state very quickly, and I realize from this discussion that stability is probably one of more stochastic of the parameters at certain times of the day, so between four and eight in the morning there's a lot going on between eight and eight there's probably not as much going on and between eight and ten there's probably a lot going on again, right, and I don't know how the model deals with that.

And then I got a gap and I start thinking about other things like factors like application method and how does the model really address issues of application method, the time of day of application we've talked about and that could be looked at as another factor. And then there's this ideal of regional data source using neighboring climate data and examining differences in the model, so if I'm looking at Bakersfield but I can get five stations around Bakersfield, if I change from station to station what does that really do to my view of the buffer zone distribution.

As a side comment you know earlier today you were saying well we can always run other stations and all I could think of is bad data replaced with bad data replaced with bad data does not make good data, so we really have to think about -- students fall into that trap sometimes, you know well you say that's bad data, well I'll just go get this other bad data and that will make it good, right, I put it together, I got twice as much data.

MR. SULLIVAN: (Inaudible) National Weather Service sites are representative and have decent data, the thought was that by bringing in -- I'm just using number 10 -- 10 stations from a region to capture the reasonable variability was the hope there.

DR. PORTIER: Well I think the point on representation that someone made, we really have to do an objective examination of whether that station has even a chance of being representative for the site we're looking at, so if you're looking at San Francisco and trying to describe what's going on in Fresno nobody's going to believe you, I don't think you're going that far.

MR. SULLIVAN: The paradigm in my head is different than that. What I'm thinking about if the region is some portion of California for example wouldn't want to say well this is representing San Francisco and whatever, I would like to say that this data set represents the variability I would expect to see in that region and it's reasonably, you used the words earlier, what is acceptable, acceptably represents that, it won't hit every different possible met regime, but it will do a decent job of it.

I think from that perspective it looks a lot better, but if I'm trying to say this will represent every location, every field in the regional county, I just can't do that.

DR. PORTIER: That's why I say it's on the side, it's just the thought that was popping in my head, are there certain graphs and tables that should be reported, Dr. Yates was talking about basically the data and tables 14 and 15 that display the sensitivity results and these tables present average buffer distances for given the exceedance levels where specific individual or accommodation factors are assumed to be stochastic. What's not provided in these tables are confidence intervals or standard errors for the outcome buffer levels, so providing these values allows us to examine the extent to which adding stochastically to these values have an impact on the uncertainty of the outcome, and the fact that the values in the table are close to the rever's (ph) case, tells me that on average the stochastic situations that you've created matched the reference case and to me that's a baseline, it better do that otherwise your stochastic model is biased, you want it to do the reference case to a certain extent and then you're putting a stochasticity around that reference case, so table 14 and 15 were good to show biasness which I don't think you have, but it didn't give me that next level which was the effect of uncertainty.

In figures 28 and 29 you show how the means stabilize with increasing sample size. And again what I'd want to do is look at what the standard deviations do at sample size as well because around each of those lines you have uncertainty distributions, hopefully they're stabilizing at some point and then you can say for 750 replications I get a good picture of what the distribution looks like, if I stop too soon then I don't get a good measure of the standard deviation, and I would point out that it typically takes many more samples, replications to get a good grasp of variability than it does to the mean (ph), right, because the central limit therum (ph) kicks in really quick and the mean becomes the mean, but there's no central limit therum for that standard deviation and it needs much more data.

MR. SULLIVAN: So it may take more than the five or ten thousand to stabilize that standard error?

DR. PORTIER: I don't know, I mean your mean standardized at a thousand, maybe fifteen hundred or you may be lucky, I don't know because you will know it when you run it, we won't know it, I can't predict that.

What types of further evaluation steps does the panel recommend, I think we've been talking about more regional differences, less extreme situations, more fumigant application methods, and analysis of whether -- something we haven't talked about is

analysis of which weather situations define the extreme buffer size values and I think Dr. Baker was kind of getting to that. After you've done this analysis kind of in a post-analysis review you go in and you pull the more extreme ones and you go back and say what were the four-day conditions that produced this map to produce my extreme buffer, does this make sense, how often do I think this would happen, that's often a very good diagnosis for a risk assessor who can look at that and say those Santa Anna winds, they seem to come every twenty years and so I guess that's a one in twenty kind of scenario and it makes sense or they would say we would never see four days with five meter per second winds consistent and no stability, that kind of post-diagnosis would be really useful, and of course provided tools to allow them to do that, it means the model in a sense has to cash that information somehow so you can spit it back out again after the analysis is done and your model probably throws all that away right now.

MR. SULLIVAN: You're exactly right, there's a lot of data there that's -- it's counting, it's not keeping all the records, but that certainly is possible to do.

DR. PORTIER: And again this has been our experience with the feeds, with the lifeline models and calendex and they first duration they didn't have it and then we started thinking about it and they had to really go back and develop whole big chunks of code just to handle that post-diagnosis and our last evaluation we decided that was the most useful stuff of the whole process because they could identify whether those scenarios were realistic or not.

DR. HEERINGA: To follow-on this particular comment, if I could ask a question, does the TOXST module, does that retain for you what I would call sort of independent predictor variables, say the inputs for that four-day run that produced that buffer level value, in other words could you actually go back and look at those buffer level values and regress that on a set of variables including sort of composite variables to look at, what types of predictors, that was actually done and you may want to look at that in one of the exposure, the dietariex (ph) or the

cumulative exposure models that we looked at and I think as Dr. Portier said it was about the third replication, I can't recall which one it was at this point, but they actually did do that sort of regression analysis, and while that isn't completely definitive it allows you to actually look at not only main effects which you can do in sort of a prospective simulation, but it actually allows you to look at potential interactions too that might be produced in extreme values.

MR. SULLIVAN: The way TOXST is coded now is counting, it's counting how many times it goes over different threshold points of concentration, up to six concentration thresholds as counters, so what you have to do is start storing that information, it's not a difficult thing to add, and of course computers are a lot bigger and more powerful than when TOXST was written, so back then it was to limit you know, this is when a 66 megahertz computer was the fastest you could get, so that's -- it would just require --

DR. HEERINGA: I guess I would encourage you to look at these other examples first before you dove into that, but I think it was very informative in the evaluation of these other models to be able to go back after simulation runs and essentially through these prediction-type models analyze what the combinations of effects were that produce the extreme values on these, whatever outcome measures were being looked at here, buffer level, buffer value lengths I guess or distances.

DR. PORTIER: What I was going to say, even just indicating for each of those one thousand or ten thousand runs whether the scenario -- whether the scenario was in or out, but it's really buffer length, I mean you really just want to keep the buffer length and the settings of how they run.

DR. HEERINGA: One more formal discussant at this point, Dr. Seiber, do you have anything to offer at this point in time?

DR. SEIBER: Just a few, this is Jim Seiber, most of it's been said, as far as sensitivity analysis goes I saw again the emission sensitivity check against emission wind speed, wind directions, stability, and to a certain extent method of application was referred to in there. What I looked for almost when I first picked up the documentation I think I finally found it in different places would be more graphs showing measured versus model concentration versus distance, in other words the decline in concentration with distance and there's apparently seven or six or seven field experiments that have been run, so I assume that that could be generated for all of those, if it's in there and I didn't see it just ignore my comments.

And particularly if you could show measured versus modeled data for the same distances on the same graph and if you plotted both the direct decline concentration versus distance and the log of the concentration versus distance, and the latter one's kind of important because one of the assumptions that was stated up in the very early part of the document was concentration falls off, log rhythmically (ph) or you know log plot will give you a straight line I think, but I didn't see any proof of that, again correct me if I --

MR. SULLIVAN: That one's a pretty safe bet, that is easy to show. One of the things in the atmosphere that's predictable is that concentrations will fall of that way, log normal.

DR. SEIBER: Right. But again if there's something funny going on like a deposition or a breakdown then you see it in the log plot, it's not a straight line anymore, you may have to do a double log to get it to break down.

MR. SULLIVAN: And you can probably see it during convective conditions especially where it becomes rather spearious (ph) and doesn't look so good and that's when you expect to see that sort of thing not looking like a straight line.

DR. SEIBER: And another thing that would have been nice to have seen on again those concentration versus distance plots is why the less than 50 meter downwind data were not used, you referred to that -- they were not precise enough to use but I didn't actually see the data you know so I could make that judgment myself.

In our field experiments I think we're able to use and of course it's hard to compare them, but were able to use distances down around 50 meters.

MR. SULLIVAN: We can go lower than that, but it would require repeating the runs with those very small distances to add the tighter receptor coverage in there. Of course with FEMS it's set for 720 receptors and if you have distances that you want to refine you can refine the grid and run it again to get tighter coverage, would have had to do that for those short ones and we were running out of time putting the report together, so I just said less than 50, it can output down to 10.

DR. SEIBER: Okay and kind of along the same theme is how the buffer distance would change with all of these parameters both individually and in combination I guess that's basically what the others were saying too. But the emphasis there was what combination of conditions would give you the longest buffer zone which of course the health, from a health point of view that may be the first question you'd ask, but I'd want to know what the combination is that would give me the shortest buffer zones so I could go in and do some risk management or mitigation.

MR. SULLIVAN: That's a very good point,

especially in a situation where you look at what produced the worst case situation and you find this is not plausible, as you mentioned earlier that would be good to know and build a display.

DR. SEIBER: That's the end of my comments.

DR. HEERINGA: Thank you, Dr. Seiber, and that's a very good point because I suspect the conditions that produced the worst would not be the compliment of the conditions that produce the shortest buffer zone either necessarily.

Are there any other comments from panel members at this point on this question. Dr. Portier.

DR. PORTIER: I know I had a long factor list and then I realized I left two more things out which is field size and field shape.

In terms of for the uncertain analysis, right, those are two other factors we've been talking about, it's a long list, this is not a trivial task at all.

MR. SULLIVAN: No, it doesn't sound that way, but these are reasonable things certainly to look at and it would include for understanding to show that did sort out.

DR. HEERINGA: Dr. Baker.

DR. BAKER: When I talked about the low wind speed I talked about persistence, perhaps that's not the right parameter or right way to feature it. Just as there are predominant winds those are usually moderate to strong winds that people notice, they have directional tendency, low winds can have a directional tendency too, so if low winds and direction are correlated they don't necessarily have to be sequential, they might be a few hours, certain part of the day a few hours, and a certain another part of the day and repeat like that so if there is directional dependence between low wind speeds and wind direction if you're individually perturbing those then you could be not capturing the significant high exposure that you want to capture, so as I think about it it becomes more compliced in wind speed as to how to track the correlation, it's not just sequential in hours as persistence and direction would be.

MR. SULLIVAN: That's a good point. I think where that factor comes up is in a tight valley situation where at night you get drainage flow in the valley and the winds can be very, very steady, and that's why I say the option of running in the benchmark mode makes sense especially there, you would want to represent in that region the valley situation and definitely would want to show what it looks like without any -- considering its uncertainty for the wind speed, wind direction term.

DR. HEERINGA: Dr. Spicer.

DR. SPICER: Well just a brief comment about that, that doesn't necessary even have to occur in a valley flow, that's the sort of meteorology that's present at Frenchman Flat in Nevada, at nighttime they have steady winds in a study direction, there's no valley particularly associated with that.

MR. SULLIVAN: In a situation like that that one might be a tough one to represent in the regional data set, but if you know that up front you try and include something like that in the set of stations that you're using.

DR. HEERINGA: At this point are there any other comments from the panel in response to question number 8? Dr. Baker.

DR. BAKER: Showing a wind rose there might have been wind roses in -- I can't recall -- but showing a wind rose of the actual data and then a wind rose with the uncertainty applied to it would show us how much of a difference that uncertainty made. A lot of what we're discussing might actually be very minor in the overall scheme, but there's just no way right now of assessing that.

MR. SULLIVAN: The wind roses would be aggregate or marginal graphs which would nought illustrate the serial correlation problem that you have, so you might still get the same average over the four-day period in the wind rose, so that sort of marginal picture might look the same, but the actual associations time period to time period might be very different.

DR. HEERINGA: Dr. Portier.

DR. PORTIER: Ken Portier. But what I envision Dr. Baker was looking at is a wind rose with confidence bass (ph) so we at least see with the magnitudes of the variability, I mean one of the concepts is that magnitude is -- the variability is higher for the lower wind speeds and that would graphically illustrate that very quick, that you'd say high speeds, fall deviations, low speeds, high deviations.

DR. HEERINGA: Dr. Yates and Dr. Baker.

DR. BAKER: I was just thinking -- trying to remember back 20 years to a class I had, am I correct if you do a spectrum analysis and you get the power spectrum that it will give you the frequency of things so that you'd be able to tell if there's kind of a periodicity?

DR. YATES: In a perfect world.

DR. BAKER: Okay, well I was just thinking that if that would work for this where you could do it in the same idea of the wind rose you could do that with the five year data and then do it with the data that's been transformed and see if you have a different power spectrum and if so -- it wouldn't work the real world?

DR. PORTIER: Well with climate data I've rarely seen that work very well. These techniques were developed for engineering-type situations with fairly strong periodicities and then you get into climate data, they have periodicities but they're not on nice 48 (ph) cycles so they are changing, but there are some other tools but I would think that that would be something that you would hire out to someone who really knows how to analyze cycles and ballens (ph) and time -- it's one of these specialty kind of things, functional data analysis and things like that that you would want to look at.

DR. YATES: But if there was some way to be able to look at the transform or the 200 year transform data carries the same kind of properties as the five year data, that would help at least for me I would feel more comfortable with the whole thing if it's possible.

MR. SULLIVAN: We're looking at serial correlation more simplistically, would that answer the question almost as well?

DR. PORTIER: That might answer Dr. Yates question. I mean you're looking for an index that kind of says this looks kind of like this and that might actually work and I guess from what you're asking for certainly you can run the procedure and use the spectral map kind of as an index and say here's the spectral maps of five real years, here's the same spectral pattern for 200 years, are they similar, I guess using it in that sense yeah you could do that, that would work.

DR. HEERINGA: Dr. Baker did you have -- all set. Anymore comments at this point? Dr. Wang.

DR. WANG: Talking about periodicity about some of the key parameters I was wondering it will be intrusting just to look around the data, I mean it's only a short duration but it does show that I think there are some but itself has variability but it does exist, it's not totally a random process. Would you mind to show the last slides I had earlier, there's an interesting point to show you. No, the second last, it's kind of small or hard to see.

The middle figure shows the wind speed over time for a duration of eight days, no six days, --

sixteen, I'm sorry, yeah, so you almost can say that there are no transferred wind speed which is -- that means the magnitude will change a little bit, maximum wind speed.

MR. SULLIVAN: Usually you will find of course the higher wind speeds in the afternoon and lower wind speeds at night and that cycle will repeat itself.

DR. WANG: So it's a another random process.

MR. SULLIVAN: That's not random, but superimpose upon that many other random features as well.

DR. HEERINGA: Any additional comments at this point? Maybe ask Mr. Dawson and Mr. Sullivan if you are satisfied at this point with the response or if there's anything you would like to raise that's specific?

MR. DAWSON: No, we have no further comments or requirements.

DR. HEERINGA: Given that I neglected to go back systematically through our responses on question number 3, we page back, I just want to make sure, I think that was a fairly critical one, I just to make sure that we've touched on it, all we have to do is nod our heads, but let's make sure.

Let's go back to the first thing this morning which may be a chore at this point, but let's just go through here and we won't read them out loud, but in item number A we talked about general refinements and I think in the minutes of our meeting here there will be discussions certainly of that topic I believe, there have been a number of suggestions including the kind of point by point comparison that Dr. Yates raised yesterday, the issue of log transformation, I think there was sort of a consensus in this particular panel that for a calibration that the model should be fitted as the data has shown which if a log transformation would appear appropriate in the regression setting I think there's a little bit of clarification that we need amongst ourselves between the recommendations from the two sets of meetings this week and we'll be sure to incorporate that in both reports.

Any specific comments again on this, have I misinterpreted the sort of consensus of the panel, if somebody wanted to restate I'd be happy, but I think we feel we've have captured this at this point. Number C, I think we have addressed that in a number of different discussions and will be covered. Point D, I think that also was addressed. Point E we had several suggestions on the missing data, are there any other thoughts on that at this point, anything we think we need to clarify on that? These were primarily missing data in the meteorological sequence.

MR. SULLIVAN: Just in the mixing heights (inaudible) but we follow standard EPA practice.

DR. HEERINGA: Very good. And I think the one suggestion we heard was potentially instead of simple interpolation of trying to work with some sort of spline or some sort of weighted spline type just to smooth that out over instead of two-point interpolation, multiple and point interpolation. Okay.

And F I think we sort of disagreed on that, Dr. Winegar had the .9 standard and the meteorologist had the .6 standard and working with a lot of epidemiological and social data I would have dropped

to the .3 standard, but I don't want to be so substandard in this crowd, I won't mention that, but there are people in this world who live far below the .9 standard but it's still good to hear there's still some order in the world any specific recommendations on this, it really gets down to a matter of explained variance, and I think you know the alternative would you simply through out the data or would you go to a different approach, let's say that you saw .2 r2 on something would you feel you had a better alternative at that point?

MR. SULLIVAN: I think I would look for a better alternative. We did discuss a lot of things here that could be tried and what I would like to mention that was mentioned by Dr. Spicer for example was the re-sorting of the data by distance and that was an interesting concept that could be tried in that kind of a situation rather than sorting all of them all at once do it by ring, that might show when the model has a trajectory wrong be one way to maybe correct that, so that's something that could be considered for the future.

DR. HEERINGA: Very good. Dr. Portier.

DR. PORTIER: Ken Portier. The highlights that Dr. Spicer did kind of also is something that should be factored into the alternative methods, you know what is happening on some of these big values that don't seem to follow the pattern once the impact of that on that r2, those chunk of values up there has to have some impact on reducing the r2.

DR. HEERINGA: Dr. Spicer, you also made a comment this morning about data to be included in the fit of this model and you identified a specific set pairiums (ph) of data that you felt probably should be excluded, would you clarify that again?

DR. SPICER: Well what I was trying to suggest was that since the latest methodology is to simply ring the field then as long as the wind direction is predominantly in one direction then you're going to have at least half the sensors not showing any data and they do not contribute to the fit of the model at all, and the present practice in FEMS has been to set those to the lower detectable limit over 2 and then include them in regression. It seems

to me appropriate to simply leave those out if no concentration was measured, not if no concentration was predicted however, if no concentration was predicted I think they still should be included.

DR. HEERINGA: Okay, thank you for that clarification. So you would retain points where the model predicts no concentration, but the measured value show a concentration?

DR. SPICER: Absolutely. And although -- once again I'm not a statistician, but I suspect that if you leave out points where no concentration is measured then your r2 value is going to increase.

DR. HEERINGA: Dr. Portier.

DR. PORTIER: But I would - -the caveat on that is if you have a measured zero in the middle of some observed concentration -- I mean if you had a slightly more dense measurement grid you might find a zero with values around it, I'm not sure I'd throw that one out.

DR. SPICER: Well that's fair enough, but that may very well be a situation where the monitor didn't work for some reason, I mean that's not uncommon.

DR. HEERINGA: Thank you for that, I wanted to make sure I clarified that because it was a discussion we had and I think I understood what you said, but it's good to have that repeated.

Anything else to repeat on this particular topic? And I think we've also have had a chance to discuss alternative approaches and in the context of that should be collected and appear in our minutes of our meeting.

So at this point in time I guess if everyone is satisfied that we have in fact gone back through question 3 I'd like to do two things in this order, I'd like to ask Mr. Dawson and Mr. Sullivan whether there are any outstanding questions they'd like to pose to the panel and then I'd like to give the panel members a chance to make any final comments they would have either directed specifically to these questions or directed to the development and application of the FEMS model.

MR. DAWSON: No, I think we've had all of our major issues addressed and we appreciate the work of the panel over the last couple of days.

MR. SULLIVAN: And I would like to thank every panel member sincerely for the help that they have given us in developing this model and the DPR and the EPA. The comments over the last couple of days were very, very good and much appreciated.

DR. HEERINGA: Well thank you very much for everything that you and also for Dr. Barry, we appreciate your participation in the last few days.

At this point I guess I would like to give the panel a chance to make any additional comments that they'd like and we'll begin with Dr. Yates.

DR. YATES: This isn't about -- actually this is more of in the future for these kind of panels, having the questions presented in the manner that's shown up here with the a, b, c I think would be a lot better than the way they were sent to us before because I was under -- I mean this leads me when you put the A's and the B's it leaves me to believe that you really want answers to questions and when they're in kind of a paragraph you kind of think that we can just kind of -- I don't know how to say it exactly --

it's more you want a discussion about things like this and so when I see them like that I am more inclined to actually focus in on the answer to that particular question, you know maybe it's just me, but I wish that we would have had those sent to us because I think I would have come with a different idea about how to input information to the process.

DR. HEERINGA: Excellent point Dr. Yates. For those of us who've been part of this process for a number of years there has been question content inflation and question 8 would have been a long question two years ago, but very good point, but I think this level of -- clearly we don't want to have a 24 question sequence, so we make it look like an 8 question sequence with 24 questions embedded, but making them as subpoints I think as Dr. Yates pointed out allows the panelists to focus their thinking very much.

MR. DAWSON: Message received and the taxpayers would be happy to get our job.

DR. HEERINGA: We'll go down the row, Dr.

Spicer at this point?

DR. SPICER: No particular comment.

DR. HEERINGA: Dr. Baker?

DR. BAKER: I feel compelled to say that I fully support the inclusion of the uncertainty in this work, in fact I know several groups are looking at how to include uncertainty into the analysis particularly for the meteorological side of the issue as opposed to the emissions side, so it does raise a number of questions that haven't been worked out and complications, but I didn't want that to be the main point to be lost that I hope these complications don't detract from continued pursuit of how to do this properly, it's just some details have to be worked out but I think it's a significant improvement versus not including some uncertainty into the analysis.

DR. HEERINGA: Thank you very much for that comment. I think that in experiences with other SAP panels on other probabilistic modeling systems and objectives that we have gone through this process and its been a very constructive one and I don't think we've ever seen people sort of back away from this in the process of continued refinement, continued improvement. Dr. Majewski.

DR. MAJEWSKI: I don't have anymore comments on the models, but I just want to say that in the past I pretty much avoided using models or even considering models because I'm kind of a field-type guy and these last four days have certainly given me a new appreciation and for modeling and what they can do and I think I'm coming over.

DR. HEERINGA: To the dark side. Dr. Ou. Dr. Winegar.

DR. WINEGAR: No additional comments.

DR. HEERINGA: Dr. Wang.

DR. WANG: I would like to know what's going to happen next with all the time and effort we put in a nutshell, in a time line and say the next month, next six months and in the probabilistic approach, you know plus/minus a few weeks or maybe a month, so what's going to happen?

DR. HEERINGA: I think Mr. Metzger, can you respond to that as best you can.

MR. METZGER: Yes, I can. Mike Metzger, EPA.

We're going to take what you've done, take the comments that you provided and use these models very,

very soon, if we think we're able to we're going to put out -- I'll give you kind of a brief synopsis of what we're planning to do over the next four months, we're going to put out some risk assessments within about 35 to 45 days which don't use these models, those are going to go to the registrants for comments and after we get the comments from the registrants we're then going to incorporate whatever comments they provide us into these risk assessments and at that point take these models if they're usable at that point and for each of the six chemicals do probabilistic buffer zone assessments, so hopefully by December or January we will have done all of that.

So I wanted to say this as part of my closing remarks, but I would like to add at this point all of the comments that you give us today will make a very significant and concrete difference in the near term in how these pesticides are regulated, so they are very important.

DR. HEERINGA: Thank you very much, Mr.

Metzger. Dr. Seiber.

DR. SEIBER: I just wanted to comment that I felt this was a very thorough evaluation of the information that we had to deal with, so from my point of view it fulfilled the expectations.

DR. HEERINGA: Dr. Shokes.

DR. SHOKES: Fred Shokes. I just want to say for Dr. Baker's benefit that I included a lot of uncertainty in everything I said. Not being a modeler this has been a great learning experience for me trying to look at the practical aspects and see how they could fit into a model and one thing I particularly liked was something Dr. Seiber said was relative to validation of models really with real data and looking at the best management practices within a region with an eye toward getting the shortest buffer zones and I think thereby the least bystander exposure and I think in the process of that validation you could pick out the best way to use a given fumigant within a given area, it could be very useful.

DR. HEERINGA: Dr. Hanna.

DR. HANNA: Dr. Hanna, UNC. Again a question when the AERMOD will come to, is there any plan to

bring the AERMOD into the system or will you continue for a few years until the AERMOD (inaudible) evaluation has been established?

MR. DAWSON: I guess we'll take the lead from office of error on the release and implementation of AERMOD at this point our plans are to continue on with the ISCST approach and like pretty much everything we do it's an evolutionary process, so you know we'll have to see what the next six months or year brings as far as whatever kind of regulatory decisions we make and we'll have to overlay that process with the implementation of AERMOD and I guess adapting it in a similar manner to our approach.

MR. METZGER: I would like to add something if I could. One of my 41 -- it's really funny, I have eight pages and forty-one points just like I did in the last meeting in response to all of your comments, but one of my points here is to see if it's possible for us to get AERMOD moving quicker or if we can't if there's someway we could use it in its current state now to move forward, so we are going to be looking into trying to get that implemented more quickly.

DR. HEERINGA: Thank you, Mr. Metzger.

MR. SULLIVAN: AERMOD can be used for this now, we have tested FEMS -- tested the fitting procedure using AERMOD, it does work more or less. There are some quirks to it that still remain, but it's a possibility that it could be used for the fitting procedure, of course then it has be used for the exposure portion as well, but it's a possibility but the model is still going through testing as far as I know.

DR. HEERINGA: Dr. Portier.

DR. PORTIER: I've enjoyed it.

DR. HEERINGA: Well I want to thank all of the members not only of the EPA staff but the visitors from California and to the audience for their participation, the public commenters, and of course to all of the members of panel, I am always impressed as somebody who lacks specific expertise for a lot of these topics at how much concentrated expertise can be assembled and in large part that's due to the efforts of the staff of the FIFRA SAP and I guess I feel we are very well served with those of you who are able to

be here and we appreciate your contributions in this sort of last of the summer weeks and I'm sure many of us head back to our real jobs and fall schedules with teaching, but thank you very much for your time and effort.

At this point in time are there any closing remarks that you'd like to make, Mr. Metzger?

MR. METZGER: I'd like to do my thanking now as well. I would like to first of all thank Dr. Sullivan for coming in and presenting his model to us. I think it is -- sounds like it's something that we will be able to use. Secondly I'd like to thank our friends from California, Terry Berry and Randy Segawa for their coming in, for helping us to evaluate these models and also for all the help that they're going to give us over the next couple of months in implementing all of this stuff, for all six of these chemicals.

Dr. Heeringa as well for very effectively chairing this meeting and to the panel for their very good comments and ideas, different kind of ideas for how these models can be used. And again I'd like to reinforce that all of the comments and all of the

ideas that you've provided to us today will be considered and will have a real concrete significant effect in how these things are regulated.

DR. HEERINGA: Thank you very much. At this point in time before we wrap up I'd like to turn to the designated Federal official, Paul Lewis, to see if he has any closing notes and comments.

MR. LEWIS: Thank you, Dr. Heeringa. I want to begin by thanking all the panel members for your diligent work over the past two days in preparing this meeting and your very constructive comments and remarks and I'm looking forward to working with you over the next month or so as we prepare our meeting minutes that summarizes the panel's comments over the past two days and any questions that we deliberate on.

And for members of the public, again the meeting minutes will be available in about eight weeks, it will be available in three places, on the SAP website, paper copy and the (inaudible) docket and also in e-docket (ph) system.

I want to again thank Dr. Heeringa for

agreeing to serving as chair the past two days and for

his efforts in moving us along and enabling us to respond to the eight questions we have here, and for my colleagues and the SAP staff for working with me and making this meeting a success and again my colleagues in the (inaudible) programs, and our friends in California who worked so diligently to prepare for this meeting.

Members of the panel if we can meet for a few minutes in the break room just to go over some administrative issues as we are (inaudible) minutes. Thank you.

DR. HEERINGA: Finally one last administrative correction, for the record the panel has received a paper from Hanna et al in atmospheric environment, it is a 2001 paper and copies of it would will be included in the docket for public review.

At this point in time I'd like to call our meeting to the close and again thank everyone who has participated in some fashion over the past two to four days, if you've been here for all four days and safe travels and we look forward to seeing some of you in early September.

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I, Monica Knight Weiss, 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.

Monica Knight Weiss