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The above entitled meeting was called to order at 9:00 a.m. by Joseph A. Tikvart.

PRESIDING OFFICER:

JOSEPH A. TIKVART, Ph.D.
Group Leader
Air Quality Modeling Group (MD-14)
Office of Air Quality Planning and Standards
EPA
Research Triangle Park, NC 27711

Executive Court Reporters
(301) 565-0064

PANEL PARTICIPANTS AND SPEAKERS:

JOE WEIL
University of Colorado (CIRES)

PAT HANRAHAN
Oregon Department of Environmental Quality
Portland, Oregon

ROBERT PAINE
Senior Air Quality Scientist
ENSR Corporation
Acton, Massachusetts

GEORGE SCHEWE
Environmental Quality Management
Cincinnati

MARK GARRISON
ERM

GALE F. HOFFNAGLE, CCM, QEP
Senior Vice President and Technical Director
TRC
Windsor, Connecticut

ELDEWINS HAYNES

HOWARD FELDMAN
API

JOHN S. IRWIN
Meteorologist
NOAA

DICK SCHULZE
Trinity Consultants
Dallas, Texas

JEFF McQUEEN

NOAA
Air Resources Lab
Silver Spring, MD

WALTER LYONS, Ph.D, CCM
FMA Research, Inc.
Fort Collins, CO

JOHN VIMONT
National Park Service

JOE SCIRE
Earth Tech

TOM COULTER

IAN SYKES
Titan Research & Technology, ARAP Group
Princeton, New Jersey

SPEAKERS FOR PUBLIC HEARING:

PETER LUNN
Program Director for Atmospheric Science
Office of Biological and Environmental Research
Department of Energy

JOHN HOLMES
National Research Council

DOUG BLEWITT
Air Quality Resource Management
National Gas Institute

KEN STEINBERG
American Petroleum Institute

ANDREA BEAR FIELD
Utility Air Regulatory Group

MAIDHILA SHARARAN
India

STANLEY VASA
The Southern Company

Birmingham, Alabama

SPEAKERS FROM AUDIENCE:

K.C. Chen,
Argon National Lab

Arnie Stracongas (ph),
U.R.S. Radium (ph),
Austin, Texas

Speaker, Jules ...,
Science Consultants,
Toronto, Canada

Speaker from Bechtel Power

Phyllis Diosi (ph), Malcolm Perny (ph)

PROCEEDINGS

DR. TIKVART: Okay, we're a little thinner this morning, but we still have a good crowd. Again, I'm Joe Tikvart with EPA's air quality modeling group in North Carolina, and this morning we're going to have a number of presentations from various groups, commenting on the proposals that were made yesterday. First there'll be the AMS committee perspective, AWMA, a panel on prognostic models, comments from state and local agencies, then federal agencies, and finally, general public comments. I have not had any additional requests to speak other than those I announced yesterday, and those speakers are Peter Lunn speaking for DOE, Doug Blewitt, speaking for the Gas Research Institute, Ken Steinberg for the American Petroleum Institute, Andrea Field and Bob Paine for the Utility Air Regulatory Group, and Maidhila Shararan -- Professor Maidhila Shararan making personal comments, and that latter block will be after lunch. So if anybody wants to speak, see Tom Coulter in back or me.

Just a reminder about the telecast that's scheduled for August first and second. It was videotaped yesterday, and we will try to make an effort to summarize the comments that are made today but there will be no videotaping today, so you have the website address there if anybody needs to copy it down that hasn't got it there, or get it from me later.

I'll just remind the speakers this morning that if you have copies of your presentation, I would definitely like you to get them to the court reporter in back, and I would say for planning purposes, from the speakers we have, I see no way that we'll be here beyond three o'clock this afternoon. If there are more speakers who come up, fine, but right now it looks like three o'clock will do it for us.

With that, I'll turn it over to Jeff Weil who is going to lead comments for the American Meteorological Society.

MR. WEIL: Thank you, Joe. My name is Jeff Weil and I'm here this morning representing the AMS committee on the meteorological aspects of air pollution. We commissioned two small working groups to prepare draft comments on AERMOD and CALPUFF, and I mention that they're draft at this stage. Two reports will be given, one by Pat Hanrahan on AERMOD, Walt Dabberdt couldn't be here for CALPUFF, so I'll be doing his speech.

The draft comments have been prepared in writing, but they will not be submitted to the docket until after the conference. So we'll begin with Pat Hanrahan talking about -- commenting on AERMOD.

MR. HANRAHAN: Good morning. Overall, the comments that we have on AERMOD are favorable, but what we have here is an outline of what I'd like to discuss here, starting with theoretical basis, going in after that -- getting into how that applies to regulatory modeling, talking about input sensitivity as some of the variables, getting into AERMAP, talking about the user friendliness, getting into computer limitations, and then finally, a comment on symbols.

Overall, we see this as a big improvement over the existing models, the existing models primarily being dependent on the physics that we knew of in the 1960's and earlier, sometimes much earlier.

The specific areas that we see improvements are -- the first one is with respect to complex terrain receptors, especially bringing in the concept of the critical dividing streamline height. In past air modeling simulations, there's been a number of times where we've always predicted maximum impacts on top of a hillside. Somebody would put a monitor up there and nobody would find anything.

So for once, I see a chance to do things right, to predict the maximum impacts where they actually are, rather than on the side of a hill.

In terms of -- underneath this, in terms of the classes of terrain, we're not going to see problems with intermediate terrain, we'll have smooth transition going from one terrain area to another. We have a chance for having more objectivity with coordinates, with -- instead of having to fit some type of an ellipse to a hill. And also, in terms of its impacts versus CTDM-Plus, it looks like it actually does very well in comparison with that.

The next comment is with respect to the dispersion. Instead of having discrete stability classes, it depends on a smooth transition of stability dependent on the specification of the Moning-Oberkoff (ph) length, and so no longer will we see this discontinuities as we go from one class to another, for instance, if we go from a neutral class from a stable class.

And the next one is with respect to convective condition dispersion and the convective boundary layer. This is allowing for the fact that there are, in general, more areas of downdrafts than there are areas of updrafts in the atmosphere.

But yet, at the same time there are some omissions, and the primary one being with respect to building wake downwash. We strongly recommend that the PRIME building wake downwash algorithms be added to AERMOD as soon as practicable. One of the goals of AERMIC was to eliminate discontinuities, and there's already some big discontinuities with the Huber-Schneider and Schulman-Skeery downwash algorithms. I know one of the ones that I've seen that's more obvious is seeing huge increases in concentration with respect to increasing the stack height. I've seen it where you increase the stack height by one meter and you

can increase your predicted concentrations by 50 percent or so, and that's simply because you're at a boundary between the two types of downwash algorithms that are there.

Another omission is with respect to deposition. The ISC improvements with respect to deposition have not been added to the code. And the last thing is with respect to dispersion. This is a common problem of any steady-state model in that it does suffer, and it suffers the same limitations as -- of all steady state models in terms of predicting a concentration instantaneously downwind.

As I mentioned before, and as this applies to regulatory modeling, this is something that is very important. EPA's proposal is to have AERMOD be restricted to conditions where downwash is not a problem, and they also ask for input as far as the burden of merging model results. This is something we really don't want to get into. In the 1980's we had the nightmare of intermediate terrain, where we had an analysis that needed to be done and we didn't have the tools to do it.

But at the same time, perhaps we don't need to get into this, and this is something that I mentioned in a comment yesterday. I do believe that we can expand the domain for AERMOD by including conditions where the current conditions Huber-Schneider and Schulman-Skeery are conservative, and I believe you will find this will actually be the case more often than not. I think that's what we've seen with some of the initial comparisons of ISC versus ISC-PRIME. And I know the response yesterday was, in terms of MET conditions were one may be considered as diverse as the other, but I see other conditions as well, and that's by looking at what type of a scenario you have with a building with respect to a stack. If you have a tall, narrow building with a stack some distance away from it, that type

of a condition certainly would most likely be conservative with respect to the other conditions where Huber-Schneider and Schulman-Skeery are conservative.

The next comment is with respect to AERMOD versus ISC. There is a one year interim period being recommended for the both of them. We see that as something being positive in that ISC is a workhorse model at this time, and we are looking for some type of a hammer to use to get EPA to put PRIME into AERMOD, and one of them is to expand the period that this interim period is for getting PRIME into AERMOD.

Next is with respect to the consequence analysis. Like I mentioned earlier, we now will see a change in terms of where the maximum impacts are with respect to simple versus complex terrain, and it looks like we'll finally get the maximum impacts in the right areas. One of the reasons for this is the impacts we generally see are higher in simple terrain. Part of this is because of the convective condition sigmas, but another part of this may be because the gradual versus final plume rise requirements between ISC and AERMOD. And I'd like to see a little bit more information as far as how important this is as far as using gradual plume rise versus the current final plume rise we have.

And the last reason I have there, which is the center one is the critical dividing streamline height, which is certainly good physics to have in the model. Next slide, please.

Next comment is with respect to air screen not having a screening model to use for AERMOD. And screening meteorology is important in ISC in that we can permit a number of facilities without having to wait a year to get onsite data. We can get conservative estimates, protect the environment and get on with the permit.

However, with air screen, I believe you're proposing both the use of screen, screen 3, and CTDM-Plus -- and I see some problems with that. One of the problems with respect to screen is, and that's something I've seen from the work being done with the San Diego air pollution authority, in that they have run both screen 3 and AERMOD with a full year's worth of data in AERMOD, and they saw impacts from AERMOD on the simple terrain that were on the order of a magnitude or two higher with AERMOD than from screen. And that is not a conservative way of going, so the sooner we can get a screening meteorology in the airscreen, the better.

The next one is with respect for the need for National Weather Service data. There's a number of times where we have onsite data, where we have everything we need to run AERMOD, but AERMOD will still ask for National Weather Service data. In particular, it will ask for cloud cover data, and that is used in deriving the heat flux within the model. This certainly can be fixed by changing the algorithm so that you use such data as your SRDT -- your solar radiation delta T -- information and get away from using National Weather Service data that may not be representative.

In terms of input sensitivity, we have had commentors that have found that the surface roughness length, Z_0 is a fairly important parameter insofar as how you determine -- or how that affects concentrations. Right now the tables that are in the user's guide are somewhat subjective. I understand that there are better tables that you can use for determining -- to give guidance for determining Z_0 and there's also monitoring ways of determining Z_0 as well, and I'd like to see those mentioned in the user's guide to make it so that we can come up with better definitions of Z_0. And also, as I think I mentioned yesterday, there are ways of

specifying Z_0 by season and by sector, and we certainly recommend that those be used when modeling with AERMOD.

The next comment is with another h_c -- not to confuse anybody, but this is the receptor height scale, which is used with respect to the topographic data. What we find is that if you have a very large topographic feature, such as a mountain, within your domain, that will affect your receptor height scale for many receptors that are included with that domain. So selecting your domain becomes important.

One of the ways that we see of improving this is to define the -- define the receptor height scale by sector, which is -- you could have as a part of your preprocessor, where it looks to see, is there a hill -- what is the maximum hill height in this particular direction?

Next item is Bowen ratio. I haven't seen any sensitivity studies that show how important the Bowen ratio is. However, if so, we will need to have better input and we should consider such things as antecedent rainfall and then also we may also want to consider the data that's now being gathered and being collected from NCEP -- these are the ETA model soil moisture fields which are available to be used now.

With respect to DEM data, they mention both one degree data and 7.5 minute data. We certainly would like to see that be restricted to 7.5 minute data if possible, because the 7.5 minute data is readily available. And then there's caution that needs to be done in terms of using DEM data. People may want to use this as a black box.

Units -- you may have some data in meters, some in feet. Some people may not realize that there's two origins that you can use for your -- whether

you use 1927 or 1983 North American data information. You can have a point physically move on the earth by several hundred meters and still have the same latitude and longitude. You may have a problem where you have one map with one datum, one with the other, and you'll see overlap in some places, in other places you'll have the two separated from each other.

And then the other -- the next point that I have is there's different levels of quality to the DEM data. Level 1 data, versus level 2 data -- that's something that needs to be considered.

And one suggestion I've seen is why not improve AERMAP so it has some QA checks, to make sure that there's -- that the datums do match and that also, if there are large changes in terrain height that those be given out in a warning to the users so they can be aware of that and they can say, gee, that's a certain hill or that doesn't belong there.

And the final comment is that Canada doesn't have 7.5 minute data, and they would like to see it in terms of -- that you could just put in x, y, z data and still use the model.

With respect to user friendliness, AERMET is not it. If you're going to have any problems with AERMOD it's going to be with the meteorological model -- that goes to its MPRM origins. This is something that -- it could be fixed. The stage 1, stage 2 -- it can be confusing and perhaps when other updates are made to AERMOD, this is something that could be done as well.

With respect to input formats, there's been upgrades within -- with respect to MPRM in that it'll take other formats, just as FSL upper air data, or the HUSWO -- ASOS surface data, but those have not gotten into AERMET. Those need to go in there as well.

Next comment is with respect to a debug option. There's a mysterious debug option that's available with AERMOD. They can give you some interesting information on what your model's doing, but there's very little documentation as far as what those variables are. And if that's documented better, that debug feature can be used better.

In terms of computer limitations, we're not in the 386 computer mode any more. This model was originally written back in 1992, and this limit of 1500 receptors is going to be quite a burden in that we may need to run four, five, six or more runs in order to cover a domain. That certainly needs to be expanded and the .exe files need to be provided so that the users don't have to go out and buy their own Fortran compilers.

And as I mentioned earlier, AERMOD is based on an early version of ISC -- it's actually ISCST2 from 1992 -- and a number of features did not make it over. One of the features is not having Fortran-90, and another very important thing is there's an event model in ISCST3 that is not in AERMOD, and that's certainly something very useful that needs to be in there.

But at the same time that you are upgrading the code, it would be a great time to add PRIME. I'm not going to emphasize that too little.

In terms of symbols, there's something that's very confusing in here, and that's H_c, whether it's a big one or a little one. Recommend that your receptor height scale be redefined as R_h so we can get around this confusion. I know any time I've been in a class on this, I always get confused on which H_c they're talking about.

Finally, conclusion. The overall comments are favorable. The main comments are for recommended improvements, and of those like I said before, we

can't emphasize too much about getting PRIME into AERMOD. And that also we -- the other main recommendation is that is while we don't have PRIME, let's look at what cases that the existing downwash schemes are conservative, and I believe that can be done based on looking at a lot of the existing work.

And the last main comment is let's not get into merging model results. Let's use either one model or the other.

And I'd like to acknowledge the following people that have provided input for preparation of this. Thank you.

MR. WEIL: The next set of comments from the AMS-CMAAP is on CALPUFF. These were prepared by Walt Dabberdt with inputs from Steve Sakiyama, Guido Franco, Kit Wagner, Bob Karpovich, Steven Mauch, Chris Walcek, Larry Simmons, and Jerry Allwine.

The commenting process for CALPUFF consisted first of forming a small ad hoc steering group of ten people which formed the list of questions and subjects about which the commentors should respond to. This was then put out on the internet and a call for comments was issued. Eight people responded, hence the eight authors on the previous page, and these were put together by Walt Dabberdt in a report which was distributed to the steering group as well as the AMSCMAAP.

I'm not going to go through all of these names, but this is ad hoc steering group and just to tell you it comprises people from national labs, ENCR, a number of consulting firms, EPA, the National Park Service, and the university professor.

So these were the main areas in which comments were solicited. Physics and kinematics: transport, dispersion, downwash, plume rise, et cetera. The chemistry -- the range or really types of species, non linear reactions. Operational

aspects: grid resolution, some of the postprocessing that's done, user friendliness, documentation and training. And then there's some other issues pertaining to the regulatory applications which we'll talk about.

The main comments about CALPUFF, I think, are all pretty positive. They state that the CALMET/CALPUFF approach is a state of the practice simulation tool. They're happy with the integration of mesoscale models with diagnostic mass-consistent wind models, and that is a welcomed advance. They're also happy about the treatment of terrain effects on wind flow and dispersion, particularly for long range transport models, although they state that more can and should be done, and that some of the results would be applicable to other dispersion models.

Insofar as specific comments on the complex terrain dispersion, they recommend that adding a capability to use nested grids both in CALMET and CALPUFF would be a useful feature, particularly in very detailed, complex terrain areas. They are happy with the flexibility in treated dispersion in that there are three optional approaches, however they say that guidance is needed on the selection and use of the different terrain dispersion options in complex terrain.

And finally, they recommend that more research or capability be used for treating terrain effects on the sub-grid scale -- I'm ont exactly sure what that means, but they consider high resolution sub-grid terrain data, which is available from USDS and NASA as something that ought to be considered as input.

As with AERMOD, they recommend incorporating PRIME into CALPUFF. I think Joe Scire mentioned yesterday that this is on the books for happening some time this year.

As far as long range dispersion is concerned, they are happy about

three things. First, the complex terrain wind flow treatment and puff splitting; the better and generalized treatment of dispersion using boundary layer parameterization, that is in forecasting turbulence using boundary layer parameterizations; and finally, with an improved dry deposition treatment using the resistance formulation.

Of course one of the key features of a puff model is that it's able to handle calms, or very light wind situations. And they consider this a very desirable and -- well, it's certainly an important feature of a puff model, it's very useful. But they recommend that further evaluation studies be conducted to assess how well CALPUFF simulates extended stagnation events -- and they weren't specific, but I'm assuming they're talking about six, 12 hours, maybe even up to close to a full day of stagnation. And I don't know if there are any data sets on that, but they suggest that's one area where it needs further testing.

Vertical wind shear. They say the present method is somewhat awkward in that it does not consider thermal wind effects, and they recommend a more generalized approach.

Now as Pat mentioned just a moment ago, surface energy budget -- I think incorporating precipitation events into the surface energy budget is something that is -- it needs to be done with any kind of energy budget model that you're going to use, whether it be for mesoscale modeling, short range dispersion modeling or in this case, mesoscale models connected with a puff model. And this is especially true if precipitation events -- precipitation occurred recently in the last 24 or 48 hours. I think that's been a problem that's been bugging or plaguing heat flux estimates for years, and it just needs to be -- input.

Now as far as the chemistry is concerned, they state that the

atmosphere photochemistry in CALPUFF to be somewhat empirical and parameterized in a way which generally mimics the gross behavior of SO_2 and NO_x oxidation most of the time. However, it can be improved. In particular, with some dispersion options, they state that the NO_x - SO_2 oxidation rates are parameterized in terms of PG stability instead of using sunlight or the intensity of sunlight, which is the primary agent causing the oxidation.

Another point is that the NO_x oxidation is calculated using two reactions. One represents the total NO_x oxidation rate, while the second is NO_x to nitrate only, and they state that the total oxidation rate, of course, should always be greater than that to HNO_3, however this is not always the case because the two schemes are not modeled separately and this is what they say should be done, that you should model NO_x oxidation to nitrate as one mechanism, and all other products -- the oxidation of NO_x to other products as a separate scheme, and then just add sums at the end.

The formation of PAN and nitrate in heavily polluted areas, they state, is sometimes inconsistent with comprehensive models of NO_x oxidation.

Comments on the data, documentation and evaluation as far as the inputs are concerned. I have been familiar with this acronym, ACARS, but what it represents is wind and temperature profile information which is available from aircraft -- commercial aircraft takeoffs and landings, and they say that an option should be added to allow the use of this information in establishing -- or use of this profile information in CALMET.

As far as the mesoscale models that are used, right now I think the primary models are MM5 and CSUM (ph), which was developed at Colorado State, and they say that should be expanded to include the ARCS model from the

University of Oklahoma, HOTPAC of Ted Yamata (ph) and MSEPS ETA model.

Guidance should be provided on the optimum number and siting of surface and upper air observing stations. This is a matter of expert judgement and CALPUFF should be enhanced to allow -- they also say that CALPUFF should be enhanced to allow multiple profiles of turbulence, as these are currently, as I understand it right now, turbulence profiles only at one location, x/y location, are allowed -- and they say that that should be expanded to allow turbulence profiles that could be available for multiple towers or come in from a mesoscale model -- and they mention one here, the simulation model, but I don't think people are thinking that way yet.

One final note here is that they recommend that allowing the use of a mesoscale model relative humidity predictions, not just surface observations, that these should be included and this is important for modeling visibility over large domains.

As far as the post-processing recommendations, they note that currently a single run of the CALMET/CALPUFF post-processor, CALPOST, can only return information for one species parameter at a time, and this can result in a large number of postprocessor runs to obtain information needed for summary. So they recommend that that be expanded to allow multiple species be included in a single run of CALPOST.

They also recommend that the visualization capabilities be enhanced, that the analysis and diagnostic tools be improved, and that better means for conducting sensitivity studies be incorporated.

As far as model testing and evaluation, they think that the model has been adequately tested, but they also feel that more would be better, especially on

these periods of extended stagnation. They also feel that the user guides are adequate but could be improved -- and I don't know that there are any specifics on that.

Now I think one of the main comments is that they ask that a protocol be prepared for regulatory use of CALMET and CALPUFF. And that special attention be given to development of a protocol for prioritizing the five options for parameterizing dispersion, which -- and these are, first, use of metaturbulence, if it's available; calculate turbulence from micromet variables; a stability class approach, with the PGT expressions; and there's an alternate stability class approach in another; and a fifth approach. But at any rate, I believe that right now the default regulatory option is the third, which goes to the PGT system, and they say that that ought to be changed to calculate dispersion from micromet variables -- that should be the default regulatory option.

The final comments are that CALMET/CALPUFF will provide users with a powerful and flexible simulation tool that can be applied to multiple types of problems, from short range to long range. And future support, enhancements, and refinements, et cetera, is encouraged and that they encourage EPA to set up a plan for doing this.

So, that's all that we have. Those are the comments from the AMS CMAAP group, and they will be submitted in paper form during the comment period following this meeting.

DR. TIKVART: How about the slides -- a hard copy of the slides, have they been --

MR. WEIL: Yes, we can give you those.

DR. TIKVART: Okay, that would be good. I have one question for

Pat and it has to do with the slide that was handwritten, and in there, and I'm not sure I quite understood the point. Pat was concerned about the National Weather Service data, and I want to make sure I understand that there was a deliberate attempt to allow the use of National Weather Service Station data as a default input to AERMOD. Pat, were your comments directed at not doing that? Or did I miss the point?

MR. HANRAHAN: The point on this is, there's no problem with using National Weather Service data when that's all the data that you have. But quite often you may have a site that's between a coastal area and a drier area where National Weather Service site is not appropriate, and you already have sufficient onsite data to define the PBL parameters that you need. You should be able to depend entirely on the onsite data to develop everything you need for AERMET. You shouldn't have to bring in a National Weather Service site for that.

DR. TIKVART: Where you have the data.

MR. HANRAHAN: Where you have the data, yes.

DR. TIKVART: Any other questions for clarification by either Jeff or Pat? If not, let's go forward with the AWMA presentation that will be led by Bob Paine.

MR. PAINE: I'm just going to give brief opening remarks and then call forth a -- five individuals who will comment on various aspects of modeling. Our committee -- by the way, I'm the chair of the Air & Waste Management Association Meteorology Committee, which is code named AB-3, and we have over 50 members that have various degrees of involvement.

The objectives of the meteorology committee within the Air & Waste Management Association have -- we have four bulleted objectives:

* One is to promote the understanding of meteorology and its importance to air pollution control within the AWMA and the professional community at large.

* The second issue is -- for our committee -- is to make available, within the AWMA, to the professional community at large, to regulatory agencies, and to legislative committees a ready source of information and expertise in air pollution meteorology.

* Our third charter item is to encourage the proper use of the science of meteorology in the field of air pollution control.

* And fourthly, our committee ensures the dissemination of the latest scientific information on the application of meteorological methods through sponsorship of sessions, meetings, and other events.

In keeping with these goals and our Charter, our committee is honored to have been asked by the US EPA to provide comments on the proposed changes to the modeling guidelines.

Now our comments have been compiled by several subcommittees, and the draft versions of the comments were provided to the entire committee for review over the past several days. These comments do not reflect an individual's opinion, but rather a consensus opinion of those committee participants who took part in the process. For this modeling conference, the use of e-mail and the coincidence of having an annual meeting last week in Salt Lake City, has provided the best opportunity ever, I would have to say, to have an open and inclusive process in the development of these comments.

We're going to have five segments of comments. The first will be on AERMOD with George Schewe. The second will be ISC-PRIME by Mark

Garrison. The third on CALPUFF by Gale Hoffnagle. The fourth on the use of meteorological data in modeling by Eldewins Haynes. And the fifth on regional modeling by Howard Feldman.

Final written comments will be provided to the docket by the close of the comment period, August 21st.

And now I'm going to give you George Schewe.

MR. SCHEWE: I'd like to thank everyone here, especially EPA for the opportunity to speak this morning and give some comments on AERMOD. I'll be giving these on behalf of the Air & Waste committee AB-3, and this was done in -- Bruce actually put these comments together. Also, I'd like to thank the various EPA teams and work groups for accepting the somewhat dubious honor of putting together these new models and laying themselves out here in front of everyone.

By the way, how many people, including John and Joe, have run AERMOD? Very nice, okay, so we have some hands-on users. What I'll be doing this morning, I only have nine minutes left already is just give you a summary of what our comments are. Joe will be submitting our formal written comments over the next several months. I, in fact, was elected secretary of the AB-3 committee recently, last week, and so I'll be putting the comments together for the whole AB-3 group and we'll be consolidating those for you.

Two major comments that the AWMA wants to summarize here before I go into our specific answers to EPA's specific questions that they posed. Those are, will AERMOD be used properly, given the wide diversity of sources, application scenarios in the skill sets of the users. And secondly, and this is a big concern that I think has been voiced all through the last two days, will EPA force-fed the combined use of both two of their best tools yet AERMOD and PRIME in a

manner that not necessarily is straight forward, or ... intuitively meaningful, and will they heed the plea of us users and in the scientific community, and please install PRIME in AERMOD soon. First slide, please.

I will probably read these because these were prepared by Bruce and I just had the opportunity to look them over last night about ten o'clock. We ... basically the six questions directly here, and we will try to organize our written comments in the same way.

Has the scientific knowledge of AERMOD been established? We thought about this saying yes, and then moving to the next slide, but we gave a little bit of comment here. The model is based upon improved characterization of atmospheric boundary layers, and we're very pleased that that is in a model, now, that's very good.

Is the model more accurate? Yes, it seems to show again, greater accuracy in the model evaluation studies. We caution that this improved accuracy is going to be associated with less bias towards the conservative, overpredictive tendencies that models had in the past, which the agency used in their decision making process. What that means is we'll have to make better decisions on some of the inputs that we use.

What are the regulatory uses? AERMOD, when fully implemented, should replace the ISC model for routine permitting applications. In the interim, use of AERMOD and ISC-PRIME can be supported, but the AB-3 committee believes that it should be done very cautiously without artificially combining the two models. Also we think that the latest deposition and downwash algorithms need to be included, again, as soon as possible. I think we'll emphasize this every other slide just make sure the point gets through. We do recommend that further tests possibly

do need -- are needed to assure a better performance of the model in a wider variety of complex terrain settings.

Do significant implementation issues remain? I think this gets back to what I said on an earlier slide. The model is going to be less conservative, better decisions are going to have to be made. The fact that the model can utilize greater detailed surface conditions and meteorology, means that users must be better equipped to make choices of input data. And this reflects, I think, the AMS' group's concern on the use of AERMET as well. Having used AERMET a few times over the last several months myself, I feel that that is a very valid concern. We're also again, concerned about the interim use of the combined AERMOD/ISC-PRIME results in some sort of a postprocessor.

Are there serious resource constraints imposed by AERMOD? Well, the documentation seems to be pretty good, but again, seems to be a need for increased experience in establishing input data and in running the model.

What additional analyses are needed? We strongly recommend again, that we incorporate PRIME in AERMOD. Bruce had this on every slide. So -- we agree that the model should be tested, then, and of course exercised for the structure -- for the range of structures, stack heights, et cetera that a new source review regulatory needs. Again, one last emphasis on the complex terrain is we probably need to see, or would like to see more testing in different types of terrain.

So we thank you for the opportunity, and that's I think all we can do in ten minutes. Mark.

MR. GARRISON: Good morning. I want to add my thanks to EPA for the opportunity to present some comments on the ISC-PRIME model. I'll try to go through this fairly quickly. It's been a challenge coordinating and organizing

comments from many different people on the committee, but I will do my best. I will, again, try to go through this quickly because time is somewhat limited.

I too have organized the comments along the lines of responses to the questions that EPA proposed in their -- in the Federal Register notice.

The first one is, has the scientific merit of ISC-PRIME been established? I think the committee believes that the model is indeed a significant improvement over ISC3. It addresses several well-known deficiencies in ISC3 that have been detailed yesterday by a couple of people. We do have some suggestions for further evaluation and analysis, and for implementation issues, but nonetheless, I think our bottom line is that ISC-PRIME provides a superior, more realistic, and more scientifically defensible approach to modeling the effects of buildings and structures on stack releases than is currently contained in ISC3.

Is the accuracy of ISC-PRIME sufficiently documented? We sort of looked at this in two different ways. The first way was in terms of the model performance compared to observations. And the second way was in terms of model -- how the model performs when compared to ISCST3. And in general, we believe that ISC-PRIME is very well tested against available data bases, and against other models; that overall, performance of the model when compared to observations indicates suitable performance for a regulatory model. We do have some concerns that some of the wind tunnel comparisons, including for the Lee power plant in a paper presented by Ron Peterson at the AWMA conference just a couple of weeks ago, suggested that ISC-PRIME does have an underproduction tendency in some situations in the near wake where maximum concentrations occur. And in some cases, based on Ron's paper, ISC-PRIME was nonetheless an improvement over ISC3, both models underpredicted, but there were some cases

where ISC-PRIME underpredicted more than ISC3.

Another comment on the Lee power plant evaluation: a scaling factor of 0.61 was used to convert wind tunnel five minute results from one hour concentrations. The committee, in looking at the way that factor was applied, believes that it might be inappropriate. Is that factor is removed, then the underproduction tendency is exaggerated, although -- for both models, although ISC-PRIME still does provide an better prediction than ISC3.

We would also note that specific evaluation of the performance of the model as it treats stacks close to buildings compared to -- directly to the same stack right next to a building has not been very thoroughly tested.

Committee members have a wide range of experience with ISC-PRIME and in this slide I would like to, well, I sort of take the liberty of -- P stands for ISC-PRIME and I stands for ISC3. And I think we're presenting these in terms of supplementing or adding to the observations I made in the consequence analysis prepared by Bob Paine to look at the effect of ISC-PRIME versus ISC3.

But just to go through this very briefly. ISC-PRIME is -- tends to predict much lower concentrations than ISC for one hour concentrations, but as the averaging time increases, the concentrations become more similar.

ISC-PRIME tends to predict greater than ISC for short, non-buoyant stacks.

ISC predictions decrease with height much more rapidly than with increasing stack height with ISC-PRIME, and there's a sharp concentration decrease at GEP, and we learned why that is yesterday, when Joe Scire told us that -- which I didn't know -- that the downwash effects are simply turned off at GEP.

PRIME predicts much -- tends to predict a much lower

concentrations than ISC for stacks removed from buildings.

And one sort of peculiar -- not peculiar, but unusual effect of running BPIP -- BPIP ... PRIME on L-shaped buildings, that you tend to get a very large effect of lengths and widths that may not be realistic.

We also think, based in part of some of our experience, is that ISC-PRIME performance in terrain is questionable. I'd like to amplify on that a little bit. The traditional approach of simply subtracting the terrain elevation from plume height is, at best, tolerable when the terrain is below the stack. But since ISC-PRIME forces a decrease in plume height downwind of the structure, there is a concern that the combination of that and the traditional terrain handling exaggerates the model ... their quality impacts. At present, the committee, AB-3, suggests that a temporary fix might be to limit the simple terrain treatment to half the stack height or alternatively, to limit the application of ISC-PRIME to flat terrain.

Based on these observations, the committee believes that the potential exists for situations to arise in the real world applications. The model performance would point to desirable changes in the model itself, or in the processing of building structure inputs into the model. The committee recommends that during the transition to phase II of this new model, EPA would be receptive to possible changes to ISC-PRIME and/or its preprocessor to insure that the model's superior technical formulation is not compromised by unusual performance characteristics in situations that provide compelling signals for needed improvement.

Next question is are the proposed regulatory uses for ISC-PRIME for specific applications appropriate and reasonable. Our response is a yes, but...

The -- I guess the 64 million dollar question with respect to ISC-PRIME is its relationship to AERMOD and when should the two models be used,

and I guess, what is meant by the term "important"? What are important downwash effects?

If you take a very prescriptive and inflexible response to that question, I guess it's our view that you're put on a slippery slope down to -- ending up with an hour-by-hour, source-by-source, receptor-by-receptor mixing of the two models, much as intermediate terrain was treated a few years ago. And the committee does urge EPA to avoid any recommendation to mix and match predictions from these two models.

I would like to amplify on that a little bit too. With both ISC-PRIME and AERMOD, the transition period is likely to involve a great deal of learning about the performance of these models in real world settings. It's our understanding that the future plans include incorporating the PRIME module into AERMOD, and I would like to add my two cents to -- I think the entire committee would like to add their collective two cents to encouraging EPA to go ahead and do that.

We are also considering a recommendation to pass around a collection plate once this -- in any event, it's the committee's belief that combining the two models is not just a clever programming exercise, not just a clever postprocessing exercise, and that the combination of these two scientifically advanced, but very different techniques requires careful evaluation and a lot of thought to avoid compromising the benefits of the model and also to hopefully, avoid producing a new model that produces concentration gradients.

Issues mentioned previously in our comments, such as treatment of terrain, the configuration of structures, et cetera, ought to be considered along with upgrading the underlying ISC model to include the improvements that have been

made in ISC3 since 1995.

A little bit further on that point, during the transition period, decisions on which model to use, AERMOD or ISC-PRIME, we believe is best left up to the judgement of the applicant and lead agency on a case-by-case basis. Some decisions will be clear. If the analysis involves only stacks that are at or greater than GEP, AERMOD only should be used. If the analysis involves only stacks that are subject to downwash, ISC-PRIME only should be used.

To address the in-between cases, the committee suggests the following approach: an initial choice or a primary model should be made between ISC-PRIME and AERMOD. The chosen model would be run both with and without downwash. If ISC-PRIME is the initial model and the run without downwash results in identical design concentrations, then the initial choice would appear to be flawed and consideration should be given to running AERMOD in that case. Is AERMOD is the initial model and the run with downwash results in higher concentrations, then it would appear again that the initial choice was flawed, and consideration should be given to running ISC-PRIME.

In another case, if the design concentrations resulting with and without downwash are acceptable, then the additional analysis should not be required. And if application of the second model is determined to be necessary, it should nevertheless be limited in scope and application and the approach should not include combining outputs from both models into a single run.

Next question is do significant implementation issues remain or is additional guidance needed? I think we've already discussed the regulatory niche and the relationship to AERMOD, and we believe too, that maybe the use and evaluation of BPIP preprocessor probably needs some further guidance.

This is the easiest question to answer. I think it's the committee's belief, and my experience, and most people's experience that the model itself is very easy to use. The BPIP preprocessor is also very easy to use.

We do have some general -- other issues and comments that didn't fit into one of the questions, before I present our summary conclusions.

In terms of the use and evaluating the revised BPIP, it is generally easy to use and understand. Its applicability to complex buildings and structures leaves some room for evaluation and possible improvement, and I guess we would suggest that continued work and evaluation be done on the preprocessor.

In terms of documentation of model algorithms, I think there have been a couple of very good papers -- a March 2000 paper by Lloyd Schulman is a good overall view of the model. I think Joe Scire, yesterday said that we could look in the source code and figure out all of the algorithms, and that's probably true if one were as smart as Joe, but I would -- I think it's the committee's feeling that a model formulation document along the lines of the MFD for AERMOD is -- would be a very desirable thing. I'm not sure how many of you have actually looked at the MFD for AERMOD, but it is a very impressive document, it is very thorough. And we also think that the BPIP user's manual needs to be updated with complete examples.

Okay, last -- almost last slide -- I tried to answer the what additional analyses or information are needed question by simply presenting our conclusions and recommendations.

I think our bottom line is that the -- that ISC-PRIME is ready to be included in the guidelines, with some caveats for the transition period.

The wind tunnel apparent underpredictions definitely deserve further

evaluation. We recommend that some sensitivity testing of the model. We think that a couple things should be avoided including application of ISC-PRIME in elevated terrain, and application of air quality for building downwash. And evaluation of the performance of the model for stacks removed from buildings is desirable.

And this is the last one, and we suggest that EPA be receptive to changes and improvements in the model that may be dictated by evaluation during the transition period.

And finally, that EPA be receptive to extending the transition period until the AERMOD/ISC-PRIME merging has been completed and evaluated.

Thank you very much. The next speaker is Gale Hoffnagle.

MR. HOFFNAGLE: IWAQM, IWAQM, IWAQM -- oh, that's the wrong talk. That was an earlier talk. We have also -- some remember that talk. We have also chosen to answer the questions in the same -- as the way Joe posed them, I guess.

Has the scientific merit of the model been established? Has the scientific merit of CALMET/CALPUFF been established? We think so. We agree that it has, and that it is a significant advancement. One of the real neat things about this model is it has a lot of flexibility: buoyant line and area sources, deposition, first order transformations, shorter averaging times, calm winds -- and Joe says he has rain hats and puff splitting, and boundary conditions.

We have a lot of many years of looking into this model and seeing how it works, as we worked through lots of different problems and we'll have to grow with it, obviously.

Comments on the accuracy. Is the model's accuracy documented.

Yes, we believe so. The testing is adequate for inclusion in the model, certainly for the 50 to 200 kilometer range. More testing for the short range is desirable. I think without more testing it may be difficult for people to demonstrate the generally more appropriateness without more testing. So in order to demonstrate use of the model in shorter distances than 0 kilometers, we're going to need more model demonstrations.

Regulatory applications. Is it appropriate for long range transport, for 50 to 200 kilometers. We urge specific guideline language about the appropriateness for short range, where puff modeling may be more accurate. And as John Irwin described so eloquently and usefully yesterday, there is a procedure here for use of CALMET/CALPUFF at less than 50 kilometers. We find that even this -- this is a reduced procedure from what was before -- you said there was five people that did this before, there may be ten that follow the new procedure. But we would urge EPA to reconsider and make that procedure even more up to the states, I think, rather than necessarily the regional modelers.

Next, implementation issues? Are there any implementation issues? Yes, I think the user's guide must be revised and corrected. One of the major -- just as an example -- one of the most important parameters is MDISP, which is the selection of the turbulence coefficients that you're going to use in CALPUFF. The user's guide and the model disagree about what MDISP5 means. That needs to be fixed. One of them's right.

More output modes of regulatory importance. I think the -- that means basically, we need, if we had multiple sources involved in NAAQS or PSP analysis, right now we have to make multiple runs with CALPUFF to figure out what those answers are, and there could be more ease of what the contributions of

each source are.

We -- add PRIME. I'm glad to see that Joe Scire went back -- Joe's not here, right? Oh, there he is. Well, go back and put PRIME in CALPUFF. We think that's a very good idea.

I think there needs to be more clarity on default options, as many people have said. Consider multiple grids or nested grids -- I think that's a quite useful recommendation that the AMS has made. We need a close -- in one situation that we're dealing with, we have close grid -- a ten kilometer by ten kilometer grid to get the terrain right, and then when we need to do 50 kilometers to get all the sources in the analysis for PST, we have to use a larger grid, and that means the larger grid in CALMET and CALPUFF ends up -- you know, glossing over the original work we did. Now which NEQS (ph) answer for my source do I give? The small grid one or the large grid one? So there are some issues there that need to be addressed. The accuracy is not the same, obviously, for the large grid versus the short grid.

We also, just like AERMOD -- CALPUFF needs some thing beside -- like SRDT -- besides total cloud cover, which always means that in CALMET you have to add an NWS station to the grid, whether you need it or not, to get total cloud cover, and if we had SRDT that would be -- that would work.

Are there resource constraints to the use of this model? Yes, well, computer storage and time limits limits the users although every day we go out and buy a bigger machine, and the PC we just bought is larger than the server we have at the company, so this is all getting fixed with time.

Certainly skills that are required limits the number of users -- I was glad to see that we have some people who have taken CALPUFF training, but

believe me, you're a small fraction of the users of these models, and there certainly does need to be as much training as possible.

I think EPA must supply technical support by EPA personnel to all of these models. This is a general comment. It is inappropriate for us to go back to Joe Scire every time that we need model support and know how to run the model. And there will be too many people doing that, and Joe's busy enough already. So I think we need, certainly with CALMET/CALPUFF, we need to have EPA personnel at the clearing house, ready to answer questions about how to run the model.

As with all regulatory modeling, emission inventories is the weakest link. In any PSD exercise, emission inventories is the weakest link. We know you can't do anything about that, Joe, but EPA can, so we're making the comment.

And I would like to see the Class I area packages -- meteorology and emission inventories for Shenandoah National Park that John Vimont talked about at the last modeling conference. And we need to make some further push to have that happen, so that when I'm a small source and I'm within 200 kilometers of Shenandoah, I can pick up the emission inventory and the analysis to do it. Right now, if I do that -- and I've done this -- you have to go to four different states for their emission inventories. Not great work, you know. It takes a long time. It's really not the states' fault, but somewhere in this system we need to get these things together.

The last slide. What additional analyses are needed? Is information needed? There is sufficient information already on CALMET and CALPUFF to include in the guideline. We have a question, and the question is, does CALMET need five years of -- even of National Weather Service data? Think about it. We are -- CALMET is providing us with a wind field that is probably reproduced most

of the time, that is, if you have -- especially if you have terrain or something like that -- we're reproducing a wind field many, many times during the five year period. That is the variability that led us to get regulatory analysis to use five years worth of data, is a variability that's related to straight line Gaussian modeling. And I don't believe that true puff modeling needs that kind of five years worth of meteorological data. Some work needs to be done on whether that's true or not, because I believe it could be a lot less.

It would be useful, and I think John led us through some of the minimum met requirements, but there could be some more effort put in by EPA on what the minimum requirements are for the use of CALMET/CALPUFF in various applications.

And as others have said, clarification of the dispersion coefficient treatment. MDISP is useful and we ought to work on that.

I'd like to thank John Irwin for his citations yesterday to the guideline with all the numbers and everything else like that that helped us -- help all of us understand how CALMET/CALPUFF will be used in the guideline. But you missed one.

MR. IRWIN: Ah-hah!

MR. HOFFNAGLE: 7.2.8(d) which talks about stagnation. Okay. 7.2.8(d) which talks about stagnation.

We are -- opposed is probably too strong a word -- but we don't know that there's any reason to keep wind valley in the guideline, specifically in that stagnation section. I know that there are some agencies and some people in the northwest that have used that, and that's part of their SIP, and that makes it more difficult to take it out of the guideline, but we think CALMET/ CALPUFF,

especially because of its handling of stagnation and calm wind conditions, makes wind valley obsolete.

We believe that 8.3.4, treatment of calms, should also include a paragraph on CALMET/CALPUFF, and that is, here's a place where we believe CALMET/CALPUFF should be in the guideline at 8.3.4, but it's not. Since CALPUFF does, everybody agrees, does a reasonable job with calms, then it should be in the paragraph on treatment of calms.

As a final note, the Air and Waste Management Association is planning a conference, a special conference in April in New England, on the guideline. You'll get the call for papers very shortly. Go to the AWMA website for the call for papers. If there's this many papers, we'll have a great conference, right.

PARTICIPANT: Tell them where it is.

MR. HOFFNAGLE: We're going to try to have it at the Foxwoods Resort and Casino, the largest casino in New England -- in Connecticut. Thank you.

MR. HAYNES: Good morning, I'm Eldewins Haynes, presenting some comments on meteorological data used for air dispersion modeling for the Air Waste Management AB-3 committee. I don't have any slides, I'm really, really low tech this time, but I gave the gentleman at the table the copy of my text. I'm not going to read the entire text, and I guess in the coming weeks I will provide some slides for the final record and the revised text.

The AB-3 committee does support EPA's proposal to consider representative rather than onsite meteorological data where we find modeling applications involve site-specific data. We believe it is appropriate to put the old policy behind us at last.

Regarding five year meteorological data basis, USCP has solicited comments on the use of the latest five years of readily available meteorological data for modeling. With the installation of automated observing stations, the ASOS, the cloud cover observations have been limited to less than 12,000 feet. There is also a concern with the quality of ASOS observations for low wind speeds. The committee does not see any enhanced value on requiring applicants to continually update a five year modeling data base every year, especially in light of the ASOS shortcomings.

As in the case of the CALPUFF data base initializations proposed by the Federal Land Managers, the committee recommends that a carefully selected five year data base, if we need five years, carefully selected five year data base for each site be established, and not be changed annually, as long as the requirements for dispersion models using the data are satisfied.

A note on the measurement heights -- for tower on a near terrain, slope flows need to be accounted for. Tower measurements within the slope flow would be representative only if releases within the slope flow -- this is for clarification.

Now, in terms of -- for distance of measurements from the proposed facility, we're talking about the onsite requirements. Field studies imply to indicate that the wind reporting error at a ten meter height, due to instrument separation on flat terrain, is comparable to the uncertainty for co-located instruments at towers within a distance on the order of about ten kilometers. For many modeling applications, receptor coverage and plume transport distances of interest span distances of at least ten kilometers. Therefore, our committee recommends in general, that site specific tower measurements for flat terrain applications can be

considered for towers within ten kilometers of a proposed facility. This separation distance would likely be less for hilly terrain, but greater for increased anemometer heights.

For -- concerning multiple tower and SODAR levels, for purposes of determining adequate data capture for ... network, multiple tower and SODAR levels for large, meteorological monitoring programs, if available, should be considered as backup co-located data on a case-by-case basis. For example, if winds for SODAR are missing for a particular hour, but 100-meter tower winds are adequate backup, then the data for that hour should not be considered as missing. The committee recommends that this be allowed if stated in the monitoring protocol.

Now, we have some other comments on measurements. The use of the correct title -- or I guess we should have a correction in title for reference number 98, which should read, "Meteorological monitoring guidance for regulatory modeling applications dated February 2000".

Should add specific mention of sigma theta and sigma W in the turbulence measurement material in Section 8.3.3.2(h).

Also should, in Section 8.3, some reference to siting, instrument exposure, measurements and data quality guidance information for wind measurements and a similar reference could be added for temperature and temperature difference measurements. I'm not going to read the documents, but they're going to be in the record. We believe that these are voluntary, consensus standards that EPA has applied to reference when applicable.

So that should be just a little update in the guideline there.

Now, for meteorological preprocessors. Advances in air quality

modeling in the last five years have been impressive. We believe that the guidance and process in developing meteorological input files for use in air quality models now needs to be updated. For examples, the preprocessors for Gaussian models such as EP's ... height program and NPRM need better documentation and error handling to reduce the likelihood of getting cryptic error messages. And some of you know what I mean. Or sometimes, when you get worse, you have the execution stopping without any message.

EPA should provide further discussion and guidance on the treatment of missing data, especially if there are any updates or precedents available since 1992 internal memo from Dennis Atkinson and Russ Week (ph). While it's pretty clear on what to do if there's a single hour of missing data in the met file, there's some uncertainty of what to do if you have multiple hours -- three, five, ten, 20 or more missing observations. There are various ways to handle that, but there probably should be some suggestions on how to do that.

Also should consecutive missing upper air data be treated the same way as missing surface observations, even though the upper air observations are 12 hours apart? Prognostic met models that generate the detailed meteorological data for regional air quality models such as CALPUFF or Models-3 or MAXSIP (ph) should undergo more sensitivity testing.

Certainly, some educated subjectivity is needed if the initial spatial and/or temporal data resolution is poor. However, a single successful run of the met model -- met preprocessor -- does not determine the quality of the met model results. Frequently the met data fields developed by prognostic models such as MM5 or RAMS have greater and more long-lasting impact on the air quality model results than either the choice of the air quality model or the emissions inventory.

EPA should encourage modelers to investigate at least a few alternative ways of running the prognostic met model.

The committee suggests your adding some text to 8.3.2.1 and then to follow that with a suitably worded portion in the recommendation section which would be 8.3.2.2. I'm going to read that just as a suggestion. Some dispersion modeling applications, particularly those addressing long range transport are suitably matched with gridded meteorological data supplied by prognostic meteorological models. Prior ... is required for this data source because model output is not archived by NCDC.

Another point that is very important is that EPA should recommend or suggest, or however we want to word that, but I'm going to say recommend, that prognostic met models use the enhanced data bases becoming available, such as the NSIP (ph) enhanced by the RUP-2(ph) procedures. For example, to provide better spatial and temporal resolution. The ... theory, ... sound data alone should not be used as input to models such as MM5 and RAMS.

The enhanced data bases may improve model meteorological inputs in three ways. The first would be to apply data extracted from NWS, gridded meteorological fields routinely produced in its daily operations. Second, the NWS gridded fields can themselves serve as a basis for reanalysis into much finer grids, on the order of -- below 40 -- you know, ten kilometers or even less, which can also incorporate additional local data such as aerometric networks, SODAR, profiler, all that kind of stuff. And third, these gridded fields can serve as initializing fields for prognostic models. If we use that -- this data which is now becoming readily available, we may solve a lot of issues in providing good quality data in the met models.

I'm going to end my comments at that point. Thank you very much for listening.

DR. TIKVART: You try again, huh, Howard?

MR. FELDMAN: I am Howard Feldman, and I have successfully put up a presentation which if you were at the Models-3 workshop, you would know that should not be taken lightly.

On behalf of the AWMA AB-3, I have the task of discussing regional modeling issues. Now, of course the first question is everyone's saying why are we talking about regional modeling issues? We've been talking about AERMOD and CALPUFF and ISC-PRIME and what's the deal with regional? Joe says, I didn't ask any questions about regional modeling. What are you talking about?

Well, anyway, I went through sort of the way John did, and I was given this task a couple of years ago when the modeling conference -- when it was three years from the last modeling conference, and AWMA said you'll be in charge of regional modeling issues, so I've got it, and it hasn't gone away, and no one took it away from me so I still have it. So I've got the lead on this.

There are a number of references in -- I didn't count them or document them in the same way John had gone through -- to regional modeling issues in the proposal, and I think it's things that people should be aware of and how -- what it all means for all of us.

In any event of course, AWMA AB-3 supports the state of the science of modeling. What -- some of the things that are included in this proposal are dropping UAM-IV, ROM and RADM and it's clear that the state of the science has passed them by. So that's good.

Of course, another comment from all of us is that regulatory models

and the inputs need to be in the public domain. We don't need to say much more about it, but I think it's clear to everyone what that means for all of us.

One of the things that's suggested for regional modeling applications is Models-3 and CMAQ. Models-3 is the framework, CMAQ is the model engine inside of it, so when you hear this terminology -- it took me a while to figure it out -- that's my understanding of how that works out. One other thing is that it has been suggested, and the sense of the group is we need some more analysis before this is -- we're sure that this is the way to go.

One of the things that's very important, we have seen model evaluations start to be presented. Model evaluation is traditionally, in terms of regional models, gridded models. What people do is they go back and they say can they replicate a historical ozone episode. Can we get the right maximum concentration for that day, hopefully nearby. That is how one validates a regional model.

The way regional models are used is to determine emission reductions. People go back and they say, can I get the emissions reduction if I reduce emissions by 30 percent, 40 percent -- does that -- what will be the ozone concentration then? Well, that's the -- regional models can actually replicate those changes in emission reductions, so it's not just enough to replicate the historic feat, but you have to make sure your model is working correctly and it is sensitive to the changes in emissions. So we want to emphasize that.

Rob ... said two weeks ago at the Models-3 workshop -- said that there's no reason not to use the model. Well, to me that left me a little bit less than comfortable in terms of an endorsement from the model developer and proponent when there's no reason not to use it. Okay, convince me that we should, please.

So, as I said, the ability to predict changes in air quality due to changes in emissions is what's critical. Models-3 or any other regional air quality model should be able to -- required to do this before it's recommended.

One of the questions that came up when the presentation was made two weeks ago was is the CMAQ responsive? Some of the models that we see -- one of the things that we saw a couple of weeks ago was that the models underpredicted the peaks and overpredicted the lower concentrations. Well, when you're underpredicting a peak and overpredicting lower concentrations means you're honing in a little bit more towards a median concentration, and I'm sure -- and I know Ralph is up there and he can probably even tell me all the main reasons why it happens. I know there are reasons, but I need to be sure that we're understanding that the model's going to respond to emissions changes correctly.

So I think that's something that we just need to think about as we look at these regional models. Because remember if we're not talking -- when we talk ... you saw people put up numbers with a factor of three difference, and that's how people were looking at the dispersion models. When we're dealing with these open models, we're looking at a peak that might be 150 parts per billion, and you might want to be -- you might want to be going over from 150 parts per billion to get down to 120. We're talking about much smaller differences here in terms of factors that people are dealing with here.

So have -- and then we have a whole host of new issues coming up, based on what EPA would like the standards to be, which some people don't want the standards to be, but there are a whole host of issues at eight hour concentrations. Can the model fit eight hour concentrations right? Eight hour is not simply getting the peak, it's getting the integral of the ozone during that afternoon.

So just peak to peak comparisons don't do it. You have to make sure you've got the right shape of the ozone formation during the day.

Now one thing -- I started to talk about ozone, we've got PM issues too. Now, built into these regional models now and into Models-3 are the PM component. How good is the PM component? We are well in our infancy in our ability to model PM concentrations relative to where we are for ozone. PM is just a much tougher nut to crack. So can the models get -- we have a PM standard, an annual PM standard. Can you -- could you conceivably run this model for a whole year and get annual PM level? We have short term PM peaks. Can we even get close? So we've got questions. When you look at annual PM concentrations, there's a bunch of non-episodic days to feed into those. What's the approach for that? So those are going to be questions.

The other thing is, as people are setting standards closer and closer to the background, as is the annual PM concentration, and as is the eight hour ozone standard that's been proposed -- those are tougher and tougher to predict. Your boundary conditions become much more important. They become much more sensitive to a lot of things, so to understand how a model performs against those types of issues becomes more and more critical.

The other thing that we need to do is, now we've got some uncertainty built in, and we will have models no matter what, and we will use models no matter what. But we need to be able to understand and convey to our decision-makers what -- what level of certainty we have. On ozone, we can do an ozone monitor for a long time, and I think we -- if you ask ozone modelers are you confident, if you made a change, is it directionally correct? You get a right answer for the right reason? I think people would feel fairly comfortable and say, yes,

certainly the modelers would say it. I don't know about the model critics, but certainly the modelers would say, yes, we think we've got it.

But in terms of PM, I've got three question marks -- PM and regional haze? There are a lot of complicating factors going on here and a lot of alternate pathways that things may be taking, so until we're sure about that, we just need to be able to convey to the decision makers and give them an honest level of our estimate of uncertainty with this.

So one of the things is that there is a notion of what kind of evaluation should we have? And there's operational, diagnostic, mechanistic, and probabilistic -- all a part of the whole measure of this. Christian Signor (ph) published a paper this spring that covered some of this and how he would evaluate PM models, and I refer you to that.

Another thing is how uncertain are the models to changes in the inputs. Steve Hanna published a paper on that -- had a paper presented at a conference this spring -- we'll include the reference on -- that looks at that, how you sort them to Monte Carlo analyses on some of these things.

In terms of regulatory applications, we know that EPA is taking great strides, and I'd like to commend them for the steps that they made in the guidelines in terms of the ozone attainment in terms of model and other analyses, which I think is a key part of this, that the world has not been just left to rest on the modelers. As bright as we all are, and as skillful as our models are, there are other analyses too that help make an informed judgement. So EPA's done a very good job on ozone. They're working on the PM. We're looking forward to seeing that.

And I just put down as a note, that Models-3 and REMSAD are suggested to visibility assessments as well, so that's -- I think it's part of the EPA

paradigm that they are thinking about. So I just want to call that out to everyone.

Another point someone made to me is that when running the model applications, we need to make sure that the grid is resolved fine enough to do the job. Certainly there are a lot of coastal situations where you've got -- if you don't get down fine enough in the grid, you're going to miss the location, the magnitude of the peaks -- of the peak concentrations. So you've got to make sure that you get down fine enough in the grid.

And then of course, there are the famous EPA questions. -- I don't know if I took them word for word, or if I doctored them, or I made them up, but it's one of those things. Are modeling tools and policies sufficiently developed to provide guidance on an integrated approach to ozone? Well, -- and then I looked at my answer, and I read it through a few times. It's not clear that the current models can describe the effects of emission changes on PM. Of course this asks up here about ozone, so I'm not quite sure if I can match the two, but it's clear that -- it's clear -- it's funny, but it's not because these models now work as a system, as a full system. Try to do ozone and PM. And if you're not getting all these effects correct, if you're putting NO_x out into particulate NO_x instead of keeping it in gaseous, you're going to screw up the whole thing. So I think that's -- maybe that's what I was thinking when I wrote it. Maybe not.

Can they address source specific assessments? It's clear that when we dealt with large sources of ozone precursors, these source-specific types of assessments have been done. But I don't know how certain those are or how uncertain those are. You know, this gets back to the whole notion of OTAG and what does it mean, what didn't it mean, and what -- how do things come out from there.

So that, I think, concludes my presentation here and I think that concludes the presentation from the AWMA, so thank you.

DR. TIKVART: Bob, anything else?

MR. PAINE: That's it.

DR. TIKVART: Okay, I do -- I would like to open this up for discussion, but we are running a little late and we do have an hour for another panel that we need to move on with, so I think I'll hold on any questions or discussions for the time being. I do have one question though that I asked Julie or Bob and Jeff -- and it has to do with AERMOD and since the two of you might have conflicts of interest on this, if you want to defer to somebody else, that's fine. The -- AERMOD-PRIME is a no-brainer. Everybody would like to see that happen. However, funding and timing for that is uncertain at best.

If that were -- if AERMOD-PRIME were indefinite for the future -- okay -- let me just leave it that way -- indefinite for the future, what would be the best option to follow? Should we allow the use of AERMOD or ISC? And should that be -- and should we go ahead and adapt -- not adapt -- adopt ISC-PRIME for the interim until we resolve the question? Should we go ahead with the proposal as is, namely both AERMOD and ISC, or should we go ahead with AERMOD and ISC, and specify, which we haven't done, how one interfaces the two if there's a downwash issue? Care to tackle that? Was that clear? I gave you a number of options. If AERMOD-PRIME is uncertain, what would be the best option?

MR. PAINE: I'll start with some comments. I'm Bob Paine. I think the Mark Garrison et al had useful suggestions in ways you would --

PARTICIPANT: Microphone?

DR. TIKVART: Yes, I think that one there's on permanently. No

it's not.

PARTICIPANT: Here you go, Bob. Use this one.

MR. PAINE: Mark Garrison had several suggestions in which you would look at the situation. For example, if you have tall stacks and terrain in which downwash may be insignificant and of course ISC-PRIME impacts on terrain were not evaluated, you would probably want to look at AERMOD first. And if you run AERMOD with or without downwash, find no difference in the design concentration, you could probably conclude, in that situation that AERMOD is the appropriate way to go.

If you find differences, you might be able to isolate those differences to specific sources and tackle those with ISC-PRIME, so I think we can -- in the interim, we can probably come up with a kluge (ph) to limp forward. But I think that ultimately the solution is to merge the two models, but I think it's worth working with the system as we have it and get along as best we can, utilizing the best features of both models.

DR. TIKVART: Okay, so struggling with the better science is preferable to sitting on our hands for the time being.

MR. PAINE: I would think so. Just my opinion.

DR. TIKVART: Okay, that helps. Jeff, do you want to respond?

MR. HANRAHAN: I would. I would say that we shouldn't hold back AERMOD, that we should go ahead with proposing both models. One of the things that we've talked about over and over again is gee, it's downwash or it's terrain, but I think we might be missing a point on that one, in that AERMOD has a new convective boundary layer methodology that can give you significantly higher concentrations than ISC in simple terrain, and that may actually be something that's

more important than the downwash. So I would say go ahead, but at the same time, I feel that you can expand the use of AERMOD, in fact I mentioned earlier, in that let's find out where AERMOD is conservative with respect to ISC for certain downwash conditions. I think we've already seen that it may not be conservative with buildings with short stacks, but it may be applicable to a number of other scenarios. So let's not just ignore the fact that AERMOD can give you better estimates of impacts during convective boundary layer conditions.

DR. TIKVART: Just for clarification, not to start anything here, but I believe that AWMA's comments specifically recommended against what Pat suggested. I just wanted to make sure I understand, and I think Mark said not to use AERMOD for downwash.

MR. GARRISON: I think that you are --

DR. TIKVART: I want to make sure that I understand if I have concurrence or dichotomy here.

MR. GARRISON: I think you are correct that there is a disagreement. This is Mark Garrison by the way. And I don't think I need to amplify on that.

DR. TIKVART: I understand there are two views and I could probably argue either one, depending on the time of day. Thank you. John has one question, then we need to move on.

MR. IRWIN: Yes, Gale, thanks for your comments. You don't really have to answer this right now, I just want to see if I understand, and when you give your written comments, could you write them up so I better understand and the rest of us do. You were urging specific guideline language on the appropriateness for short range applications in modeling, and we were trying to walk the gentle

tightrope of not allowing people to shop for two models on the same application, in other words, we didn't want to specifically recommend both CALPUFF and AERMOD, let's say, for the same application. So we were trying to use the case-by-case as a clever device to weasel in when appropriate.

I think you're trying to say can you make it more obvious for us, and if you have some suggestions along those lines, or if you can give us some specific hints as to how you think we could do that effectively, I think that would be very helpful in our response and adopting your comments.

MR. HOFFNAGLE: Okay.

DR. TIKVART: This is going to be kind of a stand up break, I guess, while the next panel kind of organizes itself. Let's say let's take ten minutes and come back.

(Whereupon, a brief recess off the record was taken.)

DR. TIKVART: We'll proceed with the panel discussion on the next generation, and I believe that has to deal with the next generation of merging meteorological data from prognostic models with air quality models. So, Dick Schulze, do I turn it over to you?

MR. SCHULZE: Well, really John Irwin here.

MR. IRWIN: Okay, my name is John Irwin, NOAA meteorologist on assignment to EPA for the record. We have really five people, I'm just sort of here to orchestrate the proceedings of today. Dick Schulze has done all of the background work, so he gets all the credit for all the right things that are said, and I get all the credit for all the things that go wrong today. We'll try and keep the presentation that we have here rather short and brief, to sort of -- really just talking points is what we want to get out from each of the speakers here today, get you

people sort of engaged into the topic, which is primarily, what are we going to do with the prognostic data that's available to us? Are we going to really use it in depth or are we just going to ignore it? And how much can we use it effectively?

To start the conversation, Jeff, if you would introduce yourselves, speakers. And speakers, you can either use the podium, if you would feel more comfortable, I'll flip slides for you or you can run with a portable mike, which is this one.

MR. McQUEEN: Thanks, John. I'm Jeff McQueen from NOAA Air Resources Lab. We are here in Silver Spring and we work with the EPA group down in Raleigh on air quality modeling. We're -- I'd like to just look at basically some of the physical features that are out there in the atmosphere and some of the features that we want the model to include in our assessments and our SIPs. I think down the road we're going to want to start looking at this, and we want to make sure that the meteorology model has it, and we want to be sure that the dispersion model is using it correctly.

So, that I think is where I'd like to go, and this is just an overview slide of all the different physical properties that we try to consider, which we should try to think about. We have sources being released way out in the atmosphere, as particulate matter and gas. Now these, obviously, as you know, they transport and they diffuse. They interact with things like clouds and rain, and then they eventually deposit onto mountains and oceans and different surfaces. So these are obvious physical properties that are out there in the atmosphere.

We probably should start to consider them, I think, especially beyond the AERMOD range, and maybe even in the AERMOD range, I'm sure there are pollutants that do deposit within the 50 kilometer box. So that is going to have a

big effect on your sources.

So meteorological model outputs can be the largest source of error in deriving atmospheric dispersion models. I think several studies have shown that, so let's look at some of these models. What I would like the user community to start asking the questions to their modelers -- what are the attributes that are in these models that you're using for the problem that you're using them for? If you don't know the answer, we should certainly ask the modelers.

Can the model resolve the important flow fields affecting pollutant transport? As we know about sea breezes, we live here on the coast. Some of you live in the Tennessee Valley. Typically, you need four to six grid points to -- are typically required to resolve an atmospheric circulation. Therefore, if pollutant transport around a valley is studied, enough model grid points must be included to resolve the topographical forcings which control the wind direction and wind speed, not saying anything about the other processes.

In this example, you probably can't see, but the drainage flow in the mountains is decoupled from the large scale flow that you see in the flat terrain. So how are we including this? Are we including it? At least -- even if we're not including it, that's okay, as long as we know, as long as we can define uncertainties.

So how are the boundary layer mixing processes included -- number two -- for near ground dispersion? Yes, dispersion is a three dimensional problem. The stuff doesn't just stay near the ground. It does go up. It does go down. As you can see there, that's a ten kilometer box simulation from a Large Eddy Simulation there. Using second order closure -- fairly complicated -- but that just shows you the picture of what the atmosphere is actually doing. It's much more complicated than a two-dimensional problem. So we need to ask our modelers, how

are you including this?

Number three, how is the air-surface exchange included for parameters which control the deposition? Is deposition even included in the model? Deposition of pollutants, obviously, can affect your concentrations, influenced by friction near the ground and the flux of heat between the air and the surface. Many of these ecological models are trying to include the effects of vegetation or water surfaces on air exchange. Therefore, depending on whether you're at coastline or if in a complex area, or even in a flat area, you probably should consider the effects that deposition has on your concentration.

The example on the right, up above shows the deposition velocity of nitrogen to the Chesapeake Bay at different grid resolutions, and you see the red line is the 20 kilometer model. Twenty kilometer would be the logical model. The green line is the ten kilometer meteorological model -- so even at 20 and ten kilometers, it might not be on the average, representing the deposition correctly.

Number four, how are the meteorological and dispersion model interfaced? Yes, I ran MM5 and I ran RAMS with 50,000 vertical levels, but by the way, my dispersion model doesn't use any of them -- just looking at the surface value. So there's no additional improvement really in that.

So that's where -- these are the questions that I would like -- I think we have a very intelligent group out there, and I think we can start asking these questions of our modelers.

There are some model data sets out there that are starting to address this. I've chosen -- of course we've got -- I've chosen a few of these -- what we call reanalyses data sets, basically, which would assimilate in the very surface and upper air meteorology from aircraft measurements, from traditional upper air

measurements, satellite, NIX-RED (ph), and some like number three down there and number four -- ETA and EDAS is running regularly, and I think other colleagues will be talking about this.

The spatial resolution in July is going down to 22 kilometer resolution for the ETA -- for the EDAS -- this is from the National Weather Service. And the RUC is going down to 25 kilometer. These data bases, you can get the output in three dimensions. You can get turbulence. You can get cloud cover. You can get U-star, basically anything you would want to get, and you would be able to use this information to drive your model. In fact, it's on the web -- you can just click on your point and you can get your data. It's that simple. It's right on the NOAA web page. And if anybody has any questions about that, please come and see me.

And then -- but again, that's going down to 20-25 kilometers in July -- and that's three dimensional data sets. That includes turbulent kinetic energy in the boundary layers, so you have some pretty sophisticated boundary layer mixing going on there.

This is being used by the Weather Service. This is being used by the Department of Defense. This is being used by almost every weather-related agency I know, maybe except for some portions of the EPA. So I really would start recommending just using what other agencies -- just kind of leverage what we already have out there to get the best answer for the public. It is a new century. So these models of course use sophisticated 3-D variational simulation. They're out there. You don't have to run them. The data is available.

And just to conclude, really, looking at the -- well, let's go back to the last one. Within the next five years, there are joint initiatives between NOAA

and EPA to go beyond the current 45 kilometer models that we have now and start looking at the one to ten kilometer range of circulations with -- and using variational simulation to try to get our best archive of this kind of data, so that we can start including sea breezes and terrain flows in. That's the one project -- it's called the Weather Research and Forecasting System, WRF. NOAA has initiatives coming up.

And what I think the future is going to start showing is not only like a CMAQ approach, Models-3 approach where the models are uncoupled, the MM5 is run first, but now I think you're going to start seeing in the next five to ten years, like with the WRF project, where the chemistry and the dispersion is directly coupled in the meteorological model. So that clouds and the effect that the particles have, the pollutants have on clouds will be incorporated.

And NOAA and EPA are involved in these initiatives, heavily, and we invite anyone in the air quality community who is interested, to really come join us so that we can really start getting at some of your needs even more completely than we have today.

And finally, just one slide, I think, this is just showing basically why we need to go down to this new resolution. The other guys will be talking about this too, but here's the EDAS at 32 kilometers for the Chesapeake Bay, showing you the northwesterly flow, and basically this is an analyses, right, for July of last year, typical flow in the Chesapeake Bay area. And then there's just a ... model, a RAMS model run at four kilometers, showing that the flows are in opposite directions, basically. You have northerly flow. It's not capturing the local scale effect -- the what we call, local scale channeling. That could be important, I think, in your day to day assessments to include these kinds of effects.

Thank you.

MR. SCHULZE: I have entitled my remarks "getting ready for the eighth modeling conference" because what you're seeing here now is what you're going to be discussing in great depth in another few years. Dispersion modeling has a great deal of impact on the national economy. There are probably well over 2000 modeling studies done each year for applications that are made to well over 100 permit-granting agencies in this country. The results of dispersion modeling can easily affect the investment by US industry by, I would estimate, somewhere greater than two billion, and possibly as much as 20 billion dollars, in terms of siting facilities, stack heights, air pollution control equipment, and other investments made to limit air quality impacts.

Thus the economic impact of our deliberations here are not trivial. I appreciate, by the way, the cooperation and the participation of NOAA in this thing -- in this conference. I think it's very, very valuable.

Starting 40 years ago, Bruce Turner pioneered the use of routine meteorological data to enable us to develop the high quality dispersion models we have today. Then about ten years ago, NOAA started to replace manned observations that had been the backbone of surface meteorological observations with automated stations, the ASOS network.

At the last modeling conference, Bruce and I wondered whether there wasn't a better -- there wasn't better data available for dispersion modeling. So we met with NOAA and produced an article in AWMA journal in March 1998, maybe some of you saw it. NOAA has been assimilating data from five types of sources -- ASOS and other surface observations, aircraft landings and takeoffs, NEXRAD and ... and geostationary satellites. This data has been used to develop

modeled atmospheric data, and we have to distinguish between dispersion models and models of the atmosphere, because we use the same word, model. Modeled atmospheric data each hour that is enormous in scope and available with the click of a mouse.

The National Center for Environmental Prediction, NCEP, operates two models called the ETA data assimilation system, EDAS and the rapid update cycle, RUC. At a minimum, they produce about ten million bits of data covering all of north America each hour. This data is modeled -- that's meteorological modeling -- on a 40 kilometer grid, ... reduced, as you just heard, to between 22 and 25 kilometers. At each grid point, 14 or more parameters are determined at each of 40 elevations. This prognostic data is used primarily for weather forecasting, and thus is only stored for about 12 hours. So it's not archived. It's put out there, left out there for about 12 hours, and then overwritten.

Nevertheless, some people have been diligently getting this data and storing it. Naturally, it's captured chronologically, which is satisfactory if one is studying ozone episodes or industrial accidents. But to use it for regulatory modeling, one has to invert the matrix and obtain the data for a specific site, and then store that data chronologically.

The key question is, when can this type of data be authorized for use with the models we have been discussing these two days? What the panel would like to do is to promote a discussion on the use of the data, seeking your ideas on what should be done. Presentations by the panel will continue for another 20 or 30 minutes. We'll make some comments ourselves and then we'll open the floor for discussion.

And I pose ten questions. The first one is, do we need five years of

data? Could only one or two years suffice?

Second, can the modeling committee make a convincing case that prognostic models produce more reliable data that onsite monitoring data, primarily because of the ability to characterize the atmosphere throughout its entire depth?

How can archives be established?

Who should manage the archives of data? Should the archives store the data by location or sequentially?

Should we start using this data immediately with AERMOD? If so, should it be from the nearest grid point or should an interpolation model be formulized to be based on the data from the nearest nine grid points or more, and if so, then how do we modify AERMET?

When is data sufficiently sparse -- this is a rhetorical question almost -- when is data sufficiently sparse that prognostic data should be required?

How will the missing data be filled in? Currently there are some data gaps, only about 98 percent of the prognostic files are recoverable, so we have significant data gaps, much greater than we have with the historical NCDC data.

What standard procedures can be used to reanalyze data to finer grids -- five kilometers, two kilometers, one kilometer, 500 meters, 250 meters -- when studying situations in complex terrain and at land/sea interfaces? See this procedure now is going to combine prognostic and diagnostic meteorological models, and we need to have some kind of standards or procedures for doing this.

Are prognostic models capable -- this is a fundamental question -- of characterizing the lowest two or 300 meters of the atmosphere, where most of the action that we're interested in is taking place?

How can consistent application of prognostic and diagnostic models

be assured across the 100 or more permit-granting agencies in the United States?

Are there any -- is there any potential for a set of screening meteorology that could be authorized for use on flat or gently rolling terrain, more than five kilometers from a body of water? This is just to simplify the use of this sort of thing.

Well, the other panelists will now take over and perhaps talk about these, and address them. Thank you.

MR. LYONS: I am basically going to say what Dick and Jeff just said, but I'm going to say it again slightly differently with some pictures. My name is Walter Lyons and I've been in this business for about 30 years also, so I guess you could call this, "reflections of the meteorologist upon reading the new guidelines." And here are some of the thoughts of myself and a lot of the colleagues that I've talked with.

And I had some prepared statements and the answer to that, and I had some slides. I'm going to mix and match here and hopefully not get too mixed up.

A regulatory air quality model consists of key components, including a representation of the physics transport, dispersion, transformation and deposition of chemical species released from a variety of source configurations; specifications of the physical and chemical characteristics of the emissions; and an adequate representation of the meteorological factors which influence these processes. To date, the majority of regulatory models have attempted to describe the atmosphere using data from single sites, that is National Weather Service observations, and/or onsite data.

Our comments here are primarily focused on the CALPUFF

modeling system, which enables the use of sophisticated, three-dimensional, time dependent wind, temperature, turbulence and moisture fields. Spatially and temporally varying meteorology on a variety of scales permits CALPUFF to treat previously challenging regimes such as calms, sea and lake breezes, recirculations, fumigations, and transport in complex terrain.

First, we would strongly urge that in additions to its long range transport applications, that is 50 to 200 kilometers, CALPUFF be more actively considered on a case-by-case basis, in the near field, that is under 50 kilometers, when dealing with sources in mountainous areas, and especially the complex mesoscale recirculating flows of the coastal zone.

I would just point out -- actually nobody has mentioned today so far -- a sizeable fraction of the nation's population and emissions are concentrated within about 100 kilometers of the coastlines, especially if you include those of the Great Lakes. It's ground zero. CALPUFF's ability to define meteorological conditions at not just one,but literally thousands of points within its domain, greatly magnifies the need to properly characterize the meteorology. Errors influencing transport and diffusion can now be introduced not only near the source but throughout the entire model domain. It's a two-edged sword.

I want to talk a little bit about the advances in the meteorological data acquisition, data fusion and modeling systems.

Paralleling the improvements in air quality models, there has been a multi-billion dollar investment in the nation's meteorological infrastructure, as evidence by the decade-long National Weather Service Modernization Program. This has resulted in many additional sources of meteorological data. Dick's already mentioned the ASOS stations, boundary layer and tropospheric wind profiles,

NEXRAD Doppler radar precipitation and wind measurements, GOES satellite cloud mapping system, data collection platforms from commercial aircraft, the ECARS that's been mentioned earlier.

Meteorological resources now extend far beyond the twice daily radiosonde ascents and widely separated surface weather reports. Curiously, the degraded cloud cover information from the ASOS weather surface systems appears to be a step backwards when you first think about it since it is completely oblivious to clouds above 12,000 feet. But maybe this has been more than compensated for by the application of satellite and radar data, which also map clouds on a much higher resolution.

Major advances have been made in the large scale prognostic modeling at the National Centers for Environmental Prediction, NCEP, and of course, on the regional scale, suing codes such as MM5, RAMS and ARPS by universities and private sector firms.

Now, as Jeff mentioned, and as Dick also pointed out, as a byproduct of its forecast operations, each hour NCEP generates a nationwide, 3-D gridded atmospheric analysis which combines all available data from federal meteorological sources. Just about everything. And this is a typical concept -- image that you can get. Just click on your machine and pull this right up. That's the graphical form and you can FTP the raw data from it.

This gridded output from the RUC 2 model, rapid update cycle, and there's another model called the ETA which is somewhat similar -- this happens to be a 40 kilometer mesh and 40 vertical levels. We can show the 40 vertical levels in the next slide, and as Jeff pointed out, that's going to go to an even finer mesh towards the end of the summer. Now we take these 3-D gridded fields in and of

themselves are a potentially valuable source of air quality model input. Perhaps it will need a little massaging, but this is a very valuable resource.

Just give you an example here -- here would be the grid just showing over Florida. This was some work that we did for the Kennedy space Center. That's all the data you have every hour on the hour. Next slide, some idea of what the surface winds would look like if you just took the 40 kilometer RUC analysis, and you can see there's a curved wind field going right through the Kennedy Space Center. This is based on the national analysis available every hour.

Now, one more slide there -- there's a lot of good things to be said about the NCEP gridded data sets. We've already said them. But as was pointed out, there is no federal archive system, though private sector groups such as ourselves are doing it and archiving them, and I'm sure other people are. At least as they are, the grids are not immediately compatible for input into CALPUFF, but it's a fairly straightforward thing to change. We've already done that. We can insert it directly into it. And it does not include local aerometric and onsite data. Ah, that's the one thing it does not have.

So, there is actually hope here too. There's many ways to do this. This is the approach we have taken. By using the RUC 2 as a first guess field, this allows sophisticated reanalyses using mesoscale model initialization routines. We happen to like the ADAS system which is part of the ARPS model, but there's other approaches too. These reanalyses can incorporate locally available information, including mesonetworks, sodars and profilers, what have you, and then convert them to the appropriate coordinates and mesh size, and these fields can be imported into CALPUFF. This provides an intermediate level of sophistication between using a few raw National Weather Service surface and ... observations and a full blown

prognostic model.

Just to show you -- I hope you can see it, but this is a reanalysis of the RUC data, putting in the dense surface air network at the Kennedy Space Center, and all the little wiggles and jiggles and the surface flow just show right up. An example, more relevant perhaps to this application here, if you want to use CALPUFF in say, New England, you take the RUC data, we have one year RUC data, you reanalyze it down onto a 15 kilometer mesh, include the appropriate terrain and land use, reanalyze ... and surface data and out the other end comes a very nice field, analyzed every hour, it goes right into CALPUFF.

We also have an improved analysis scheme for both the cloud cover and other meteorological parameters. I mean, it's being done now. And it's fairly straightforward.

NCEP gridded products are now available over the internet, as was mentioned, and while these voluminous fields are not being centrally archived by NOAA, they can be saved easily by end users. Historical libraries are also available from third parties. We've been saving it, I might add since about 1998.

Ideally, when meteorological regimes are sufficiently complex, and resources permit, CALPUFF should be driven by a properly configured mesoscale prognostic model. Major advances in computing technology now make it economically feasible, even at the field office level, to run prognostic models over longer time periods, larger domains, and increasingly higher resolutions. And even with last year's PCs, we were running a forecast model for ... and thunderstorms right on my desk, literally. Every day we have an eight hour forecast of the thunderstorms over the great plains, and every time you run a model you get what you our looking for, in our case, thunderstorms. You get a vertical sounding at

every grid point, every time step. We don't need every time step, but if you're striking that every hour, you can refine and define all the fields you need. I mean this is what prognostic modeling is all about.

But there are some things that we should think about. It appears that it's a reasonable presumption that as more sophisticated meteorological fields are incorporated into air quality codes such as CALPUFF, improved model performance should result. But, resources need to be allocated to quantify these improvements over a range of model applications.

Encouragement should also be provided to those entities wishing to develop suitably configured meteorological data bases, utilizing reanalyzed or regridded fields or prognostic models for general use by the modeling community.

You can put just the last slide up. If prognostic models are to be used, however, it should be done with the assistance of those experienced in the application of these advanced tools. For instance, the model should be run at the resolution appropriate for the weather patterns exercising control on dispersion within the domain. I would just point out that many still today fail to realize that the actual resolution that you have in a meteorological model is four times the mesh. So when you say I'm running a model of ten kilometers, that means you're resolving only 40 kilometer features in the atmosphere. Keep that in mind. It's a very basic, fundamental thing. And also, the proper specification of surface and vegetative cover, the soil moisture, and bioshperic/atmospheric exchanges is really essential. And the new models, as Jeff pointed out, will do that.

The EPA makes frequent references to the MM5 code in its documents, but we just like to note that there are other codes such as RAMS and ARPS which may have advantages in certain environments.

The availability of several prognostic models also creates the option for ensemble modeling, something which I've not heard mentioned today, and maybe it's for the tenth conference. I'm not sure yet. But it's now a common practice within the weather forecasting circles, to run the -- to assume that the various realizations of the atmosphere produced by differing model inputs and different modeling physics, yield ranges of physically reasonable answers for consideration. Something to think about.

And we would strongly urge that the 1990 MM4, 80 kilometer mesh, one-year model archive data base that's been used in the past be retired for use in CALMET. It's now seriously outdated.

And just to sum up. We urge the air quality community become aware of, and adapt for its beneficial use, many of the substantive improvements in the meteorological data resources, data fusion, data reanalysis tools and, of course with greater frequency, prognostic models for use within CALPUFF. We would also point out that these RUC 2 soundings and other similar things could also be used as input for ISC-PRIME and AERMOD, as other people have mentioned too. Thank you.

MR. VIMONT: I'm John Vimont, National Park Service. Yes, it really doesn't show up, that's what I was afraid of. This was a snapshot out of the 1980 80 kilometer data that was just destroyed here. Actually, what you can't see on here is that in general ... observations are out there, and the arrows that you can't see are the model wind field. It generally did a nice job at this resolution of what was going on.

Now what I was going to try to highlight is you get in complex terrain, particularly right here, this vector is oh, about 30 percent greater than the

underlying model vector, and 180 degrees out of phase. So in complex terrain situations, such as east of Rocky mountains there, sometimes the modeling system doesn't always work as well as it might. There are a lot of things that go on in the front range, I'm sure Walt will go into a lot more detail on that, that why, you would not expect these things at 80 kilometers to not be resolved.

But in general, this field isn't too bad and in Class I analyses like you like to see, we've been recommending using this and then going into the diagnostic mode of the CALMET system -- and I think Joe's going to talk about it a little bit more about that, I suspect -- in terms of trying to usually do some of these terrain effects back in.

One other thing that we have done several times when we've had the data, or done some of it ourselves, I know that the state has been asking people to do it as where there is a good set of data which there happens to be a nice set of data around the front range there, rather than initialize with the meteorological models, to actually use the DEMS network. I think that's going to probably do a much better job, if you have that kind of a data set. However, we don't generally have that.

So, in general, until we get something better, this is from the tests that we did back when we were developing the IWAQM recommendations -- I'm still feel very comfortable with this kind of a data set for now, but I would certainly encourage, and I'm hoping still like I said yesterday, to get the 36 kilometer MM5 runs done, and then if these ADAS data sets are indeed readily available, that is certainly a viable option that I would encourage people to look into, because obviously there are a lot of features, particularly in the western US, that are not resolved at this kind of -- with an 80 kilometer run.

The actual motivation for this particular chart was originally that we were looking at how well it was doing over the southwestern US. Now at 80 kilometers we seem to be capturing some of the mesoscale flows you get over the Mojavi Desert. The NGM model, when we were doing ... commission work, was found to totally miss this entire effect over here because of the grid size on it.

Initialization focused here on the San Diego sound, which in this case is coming inland a little bit there, but very frequently you get a northwest flow off the coast here, which then translates completely inland. Now, at 80 kilometers, ... resolve some of this and it was coming out okay. That was really the focus of this particular analysis, originally.

So you know, there's things you do pick up. Certainly we could stand the high resolution data, and since it's being generated routinely, I would really recommend that the community work at trying to get that data readily available to all of us.

MR. SCIRE: I guess I'll just do it from here. I want to make some of the same points, but also a few different ones. The use of prognostic meteorological data is clearly a valuable tool now, and more so it will be in the future. When you look at these spatial and time resolutions, the observational data sets, with surface stations you have hourly measurements, but you have a resolution, typically, of the order of tens of kilometers between stations.

The EPRI ... data, you only have twice per day, and -- typically from the routine stations, and the resolution might be several hundred kilometers -- three to 500 kilometer resolution.

With the use of a model to -- that does assimilate those data, but then also other sources of information -- satellite information and profiler and other data,

you can develop data sets routinely that go down to five to 20 kilometers, potentially even down to one kilometer resolution with the current computing technologies. And what that's giving you is maybe something of the order of five to 10,000 data points per level, and you might have 18 to 20 levels or more. So it's a tremendously rich data set.

And then I want to emphasize I think there's a -- this cost effectiveness in this approach as well. The cost of producing a single profile in the vertical in the ... system is about two to three times higher than running the prognostic models. So you're getting 10,000 data points times maybe 15 or 20 levels for less cost than using a single profile.

However, it's very important to realize that the resolution of the prognostic model is critical. If were to use that directly into the dispersion model, you can get very, very poor results, worse that just using observational data, if you haven't adequately resolved the flow field with the prognostic model.

I want to give an example here, this is a domain that was used in Wyoming, southwest Wyoming, and looking at the Salt Lake City area in the lower left hand corner. We'll take a look at a couple of wind rows from that site. This is Salt Lake City, the observed wind rows at the surface, and it's showing pretty clear channeling terrain effects here. This is at six meters.

At 600 meters above the surface, this is the observed wind rows, so still you're getting strong channeling effects. And then the next one is at two and a half kilometers, so you're back to the typical westerly flow. So very strong effects near the surface and above the terrain, the westerly flow.

MM5 was run here at 20 kilometer resolution, and the data that you see from the previous wind rows was assimilated so it's being used in the analysis.

And MM5 at 20 kilometer resolution, didn't really get the terrain properly characterized. And so we're not really seeing this strong channeling.

And at 600 meters, it's more approaching the westerly flow regime rather than the strongly channeled flow that we saw at the observations. And then at the next one, is at two and a half kilometers, and there it's doing very well. So at 20 kilometer resolution you would not want to put MM5 data directly into CALPUFF because you wouldn't get this channel flow correctly.

One of the things that we're proposing here is -- next slide -- is to use a hybrid approach in the interim, until the -- it's practical to run a prognostic model at the final resolution that you want. Another method is to use a diagnostic model, such as CALMET, to incorporate the prognostic model data as its initial guess field, similar to the reanalysis that Walt talked about, except with this we'll do is -- it will also include the fine scale terrain effects at the CALMET scale, which might be at the order of kilometer or 500 meters, or in some cases, even 250 meters.

If you do that, this is using MM5 data as the initial guess field, you are getting the channeling effect properly characterized in Salt Lake City, because CALMET is seeing the fine scale terrain that MM5 couldn't see at 20 kilometers.

At 600 meters above the surface, it's also doing quite well. Next slide. And at two and a half kilometers, you're seeing -- all the models are doing well there.

So it is -- and there's one more slide -- This is taking the Salt Lake City station out of the analysis -- we can't take it out of the MM5 analysis because it was used in the assimilation, but taking it out of CALMET, even without Salt Lake City in there, you're still getting the strong channeling effect. So there is utility even in the absence of observations to making the diagnostic terrain adjustments in

the wind field.

I did want to talk a little bit about a project that's been ongoing with the state of Alaska and EPA Region 10, and what this is meant to do is to quantify some of the things that I've been talking about, in particular, the utility of a hybrid approach, prognostic model, and a diagnostic model. And there's -- part of this also goes towards one of the questions that was asked earlier regarding the generation of data to go into AERMOD and ISC.

One of the products of this project is a converter that will allow you to run MM5, use MM5 data, run CALMET to fine scale terrain, and extract out of CALMET site-specific wind that will go into AERMOD. So it's preparing site-specific AERMOD files for -- out of the CALMET simulation.

I think one of the things that has been clear in the analyses that we've done so far is that, for example, taking the nearest ... grid point, if it's not adequate resolution, it won't work very well at all, because if MM5 doesn't see terrain properly, you're just simply not going to get the right answer.

Also there's an option now that allows the model to run in the complete absence of observations, it allows CALMET to run. So in other words, using MM5 much more to drive the whole system. And I'll also point out we're doing our tests with MM5, which is readily available, but it could be done with any of the models as well.

This whole matrix of different types of tests that have been done using CALMET with NWS data only; CALMET at 20 kilometer resolution, driving -- I'm sorry -- MM5 20 kilometer resolution driving CALMET on a four kilometer grid; MM5 as the initial guess field down to four kilometers with no NWS data, MM5 only. There's been an annual simulation of MM5 done in Alaska at four

kilometer resolution where -- we're using that input into CALMET to see if there's additional benefit of getting a finer scale MM5 run to drive CALMET, going to 24 kilometers; then running CALMET at one kilometer resolution, in fact, doing the test down to 250 meters.

And then looking at the effect of that on design concentrations, and also looking at the effect of that in terms of AERMOD and ISC. So we're running -- we're generating site-specific data with CALMET to drive AERMOD as well as ISC in this particular example, in addition to CALPUFF.

This is the terrain area. This is Juno up here in Alaska. This is -- a met tower is located here. There's an industrial facility located in this little side valley right here, and at one kilometer grid spacing, you can see you're resolving the valleys pretty well.

But if we go to the next slide, at four kilometers, this is what the terrain looks like. You're eliminating a lot of the detail. In fact, that's a real paper mill site, and in the higher resolved terrain, that was in an east-west oriented valley, but in a four kilometer resolved terrain, you're losing that valley completely. Grid resolution is extremely important, and you should never compromise on that in order to use a more sophisticated model. You have to get the resolution done properly.

And then of course if we go to 20 kilometer meshing, all of the details are gone. So it's really important, if you're using course scale prognostic products -- and some of the products are at 32 or 25 kilometer resolution -- that, I believe, is not suitable for direct input into the dispersion models. You really need to refine the local effects if you're in a terrain situation.

Okay, let's show that. This is Hawk Inlet site. This is one of the met

towers in that particular domain, showing strongly channeled north-south flow. Let's show the next one.

This is, even at four kilometer resolution, MM5 is showing the typical south-easterly flow, easterly to south-easterly flow situation. It was very important to get the highly resolved data down to one kilometer resolution in order to get the channeling.

So I think in the interim, in the near term, the hybrid approach offers a lot of options. You can get the very good resolution out of the prognostic model, in terms of full vertical profiles at hourly intervals, in resolutions of the order of ten, 25 kilometers, but combining that with a diagnostic adjustment to get the fine scale terrain effects when you have a terrain situation is critical for the modeling.

MR. IRWIN: Well, everyone's had a chance to talk. It seems like there's lots of obvious benefits. There may be problems. And for the benefit of some of the users, I want to ask two questions of the panelists and then I think they're just going to talk amongst themselves for a while, and once they've started the conversation, then I think what would be fun is try and engage some of you in the audience. But the two questions I have, one for Walt, and one for Joe, is sort of what's the investment that they've made? You've tried to store off a lot of this data. It might be interesting to some of the people for them to understand what kind of investment you made and the problems you ran into.

And Joe, you've been learning how to run MM5 and you were talking last night when we were discussing this of some of the investments that you've made in training staff and things, and equipment. And I think the user community would be interested in some of the things that you've been doing so that you could stay abreast with all of this.

So I'll leave this mike on this side so you can pass it back and forth, and they have mikes.

MR. LYONS: Thanks, John. It's not trivial, but compared to the difficulty of trying to do something like this four or five years ago, it's much easier. We have been routinely, now, archiving -- actually have two sites, one in Fort Collins, and one in Minneapolis where we archive just about everything that comes across from the National Weather Service -- all the RUC fields, the ETA analysis, the digital satellite data, which is a rather large file, of course the met towers and all the usual thing.

And yes, we have a pretty large bill for CDs, but at 88 cents a pop or whatever they are, it's pretty minimal when you consider the amount of data you're storing. We're off-loading about two gigabytes a day. And so we have it. It requires a lot of babysitting and the communications systems that the Weather Service have are not 100 percent perfect, and sometimes the RUC doesn't run, things happen, computers burn down at NCEP and a few minor details -- so I mean it's not 100 percent data file -- no data or very few data are really 100 percent, so we will have to deal with that issue, what do you do when you have occasional gaps?

But we found it entirely doable. We're archiving the NCEP products and also driving MM5 on a daily basis, running it operationally and we're going to get to the stage where we're going to start archiving, as a lot of other people are in the state of Washington, archiving their local MM5 for ARPS or RAMS or whatever model you're running output.

The forecasting community has the mentality, well, we do all this work, we look at it and say that's great, and 12 hours later they do it all over again

and just throw the old stuff out. I mean for a forecaster, 12 hour old data is like a three day old fish. It's not something they're interested in. So there's not been a mentality of archiving this. But we've been trying over the past several years to get people to realize, hey, this is very useful stuff, and just as Joe has pointed out, you might now want to take the raw RUC or the raw ETA, but once it gets down to 22 kilometers it starts getting kind of appealing.

You have to reanalyze, but that is not a big deal. And that's essentially what CALMET does. You can put your local data in -- we have another scheme called ADAS or TARS, but it's all the same basic process, and it's a lot cheaper than running your own mesoscale model. Or, you now have at least very good initialization fields to retrospectively run your mesoscale models.

I mean there's a lot of benefits for doing this, so we've got it, we're using it, we're happy to make it available.

MR. IRWIN: What's the cost to the group ...

MR. LYONS: Well, the equipment is just a broad band link to the internet -- we happen to be in a remote area so we have to go with a microwave hop, but we need that anyways. In terms of a computer, we're just study it with a standard PC. Like I said, we're burning two CDs a day, but that's coffee money, essentially, and about a half time person to do it. So it's manageable.

We're doing it so that other people don't have to do it too, because there are times when you pull your hair out a little bit, but it's within the realm of feasibility and doability and I think in the long run it's going to be a very worthwhile effort.

I think there's a lot of agreement here about what we should do, the question is we have to establish the procedures on how to do it and to make sure

that when people decide to go down that route that they feel it's sort of being blessed, if you will. I mean I think that's what we need to hear back. Like, this is a good thing to do. You're not going to have to do it kicking and screaming your way through the system to pull it off.

MR. SCIRE: John wanted me to say a couple words about the -- how we got into the MM5 modeling and the resources, and essentially, MM5 is -- was the logical choice for us because it's a public domain model. The code is available at NCAR. You can download it, you can analyze it, you can look at it. They also offer training at NCAR at quite reasonable cost, twice a year. And so we started off by sending one person, then another person, and essentially every year we send one or two people to get trained, and now we have a good number of staff that are capable and proficient in running MM5.

In terms of the hardware -- the investment -- it's not really designed for a PC when you're running the annual simulations that we use for modeling purposes, so we have UNIX workstations -- and the low end of those, I would say, it's probably about a $15-20,000 investment. What we really use most, though, is a $40,000 machine, a dual processor machine. So you're talking about a non-negligible investment, but it's not tremendous compared to some other things.

The staffing time is minimal in terms of the training investment, the cost of the training, and the presence at the one week training course is not a very large investment to make either.

MR. LYONS: Can I just say one thing. I think you're being a little pessimistic because the new gigahertz machines, dual processor machines -- we've been running operationally, the group I'm working with -- 35 domains of MM5 on standard PCs running under NT -- operationally at 10 kilometers mesh for 35 cities

around the United States. Daily. Actually twice daily. So -- two years ago that's what you needed, but today, with the PCs, I think you'll be able to run MM5 with a wristwatch in about three years. Not quite, but I would say $5000 is about the hardware investment you need to do some of this on the ten kilometer scale, on a semi-operational basis. The trick is to do it every day for a year, not try and do one year in a month.

MR. SCIRE: That's typically how we do do it, though. There's a project where you want an annual data base, and you spend two months to do it, and the other element of this, is don't short change the need for a lot of disk space. We have a half a terrabyte and we run into disk problems on the UNIX machine, so you need a lot of space.

PARTICIPANT: Jules ... from Science Consultants. I would like to know the cost to produce the one year of MM5 run for -- like domain at ... kilometers.

MR. SCIRE: An annual data set with MM5, a customized run is probably between the order of $20 and $40,000 to produce.

MR. McQUEEN: And if I can pitch in, I think you can do a good reanalysis starting with RUC as your initial field rather than MM5 for half that or less.

MR. IRWIN: Okay, Walt, you sort of hinted that there may be some things that EPA needs to do and maybe the people in the crowd here have some other ideas of how does EPA bless the use, or what are the steps that we need to get there to use this, so I'm going to open up the floor to questions, but panelists, you chime in and answer -- you have to answer all these questions. Please identify yourselves.

MR. CHEN: K.C. Chen from Argon National Lab. A lot of people are archiving these MM5 data for different domains and for different years. Is somebody going to set up a repository so people won't have to repeat and do the same thing that somebody else has already been doing?

MR. LYONS: I should say the EDAS and the RUC data are being archived at NCAR, for one, at the National Center for Atmospheric Research, and they're actually getting funded to do this. But it might not be in a format that is very usable. And we're also archiving, and we have it on line, the NOAA -- NOAA puts on line the EDAS data on line so you can drive or run your dispersion model on line. But the idea of archiving all these different model runs at different locations is an interesting idea, but --

MR. CHEN: (off mike) I mean those which have already been done, somebody went through putting together --

MR. LYONS: Yes, I think right now we're doing it at 45 kilometers -- well, we're going to start doing it at 25 kilometers, but as far as running -- to bring all the four kilometer in and the five runs together, you'd have to have a lot of faith in all the different agencies and groups that are running it. You know, they might be making some assumptions that you don't like, so the data might not be real data.

MR. CHEN: Well, those things have to be described and it seems like -- you know, everybody has to invest $5000 or $4000 to do all this thing, if somebody else has already done, it's ridiculous to let other people repeat it.

MR. LYONS: Well, that's exactly the point I've been trying to make. I mean we've been very interested in precisely this idea of running these models operationally and archiving them so each year you build up another year of data, but

the thing that has stopped me is I need to know, -- as a business person in this context as well as a scientist, I have to at least break even to do this. So I have to know 1) is there a market place? Are there people out there who would be interested in using these, either reanalyses or RUC files, or MM5, or ARCS or whatever the output; and the other thing is, once we all say this is a good idea and march into the regional meteorologist whoever finally has to bless it, they're going to say yes, that's a good idea now get out of here. I mean in simplest terms, I mean are we all sort of on the same page here or are we going to have to fight trench warfare to be able to take this approach to apply it to CALPUFF or other models.

MR. LYONS: You're talking about forecasts? Are you talking about forecasts? That's not data, right? I mean everybody's -- you're doing daily MM5 forecasts -- a prognostic forecast. This is not the same as this reanalysis data set, right? Because you're going out in the future so there's a lot of -- a lot more uncertainty in the mesoscale model forecast than there is in a mesoscale model reanalyses.

MR. McQUEEN: Absolutely.

MR. HOFFNAGLE: Well, it's sort of a question. The question that's up there is does PST and this sort of review require real observations? The short answer to that is sometimes it doesn't even require the real stack height. No, the point of PST in this source review as far as the law is concerned, is that we're trying to demonstrate a priori, that a new stack or a new source will not cause a violation of the National Ambient air quality standard or the PST increments. And it's left to EPA in the model guidelines to interpret what needs to be had to make that demonstration. So my answer is no, it doesn't require real observations to make that demonstration as long as the regulatory agencies can agree that the data from

the prognostic models, or the reanalysis of the prognostic models is good enough data to use to make the demonstration that the NAAQS or the PST increments won't be violated.

MR. IRWIN: Another question here.

MR. HOFFNAGLE: Very much a ditto on that. Because the truth is not really needed as it relates to that question of PST, it's overwhelming in terms of the cost to do that, and even in fact, what I would make as a general comment is even the interim discussion we're having about AERMOD and the like, would be out on the table. Is it worth us all learning this process in the interim until everything is worked out with AERMOD and PRIME, given the fact, again, for the discussion, of where we are now and the fact that information presents a lot of the case that ISC is conservative? But actually, I wasn't getting on that.

What I would point in -- to this particular panel is actually in other forms of data sets that are available, my understanding is some states are setting up mesonets. The state of Oklahoma has a mesonet across their whole state -- of surface measurements. Agriculture is getting much more interested in the surface measurements that may help the situation -- of course that's in simple flat terrain, simple terrain. But individual states -- Texas is now talking about a mesonet on the order of 50 to 80 kilometer resolution, surface measurements, and possibly even sodar ... state.

And so I would just pose that as another potential data source that some states are talking about now, in fact the mesonet for Oklahoma is on the web and available. Crop moisture indices and other things, -- we were talking about Bowen ratios and things of that nature, that's something else that people could potentially tap into.

MR. McQUEEN: Yes, I think you're dead on on that. There's a tremendous amount of meteorological data out there, other than the standard National Weather Service resources. I think it's very important to understand this. And what Joe and I have been talking about is that you can either use the mesoscale model, if you've got a budget, or something like the RUC 2 or ETA if you're a little more modest, and use that for your first guess field and then take all of this, plus you might happen to have a sodar right by your facility, what have you, blend it all in, and then you get the best of both worlds. You have a better, large-scale, 3-D representation of the atmosphere, plus you can preserve the site-specific information that is obviously most important near the source.

MR. LYONS: And remember that every Weather Service office with 4.2 is now running a local analysis -- a LAPS system and that's at ten kilometers, and they are putting in -- I know the guys at NSSL are putting in the mesonet into that LAPS analysis, so I would also contact your local Weather Service office because they are running a reanalysis on -- using the LAPS model.

MR. HOFFNAGLE: Let ... long time on this topic. Dick started out asking do we need five years worth of met. My question is do we need to have all these data sets archived in the next 30 years? I think the situation -- I mean, meteorologically you're always talking about five years worth of representative met. Can't we get to some point and say that, you know, globally we're not changing or tilting on our axes and our climate's not changing drastically, although that's a matter for discussion, but you know, the issue is what's reasonable and get that situation out there and let it last for a certain time. For one thing, it would create consistency, so if you're trying to evaluate a bunch of different sources using a consistent met set would be a very good way to start.

MR. IRWIN: For the reporter, this was Gale Hoffnagle. Did this gentleman ever identify himself?

MR. STRACONGAS: This is Arnie Stracongas at U.R.S. Radium in Austin, Texas.

MR. IRWIN: Okay, well, one second, Gale, we have one more question over here.

MS. DIOSI: Phyllis Diosi (ph) with Malcolm Perny (ph). I guess there's a more -- I'd just like to get back to that PST-NSR issue. As a more nuts and bolts type modeler, I see this is as kind of shopping for met data. Met data is so critical to the output, I just think that we really need to have some kind of standard and consistent approach for permitting, and not go into this -- well, I use RUC and I use this, and I use this set of acronyms to develop the data met set. I think that's a -- you know, that's a -- you're putting a lot of, I think, stress, on an already overworked regulatory -- state regulatory agencies, and it's not leveling the playing field. I mean before the guidelines came out people were developing their own models. Now I see the same thing is happening with the met data coming in, and I think it's creating a potentially serious situation from a regulatory perspective, in protecting ambient air quality.

MR. LYONS: I think it's best to -- whatever you can use the best science that you have available, rather than what the regulator might dictate what you must use. I mean some groups might have more access to better data, better models.

MS. DIOSI: Yes, but I think you should say this is not data. This is not data. Now we're talking about using --

MR. LYONS: Are you saying that ISC is data?

MS. DIOSI: Prognostic -- no, but it levels -- it's a model that levels the field. Every --

MR. LYONS: We have to use the lowest common denominator, I guess, is what we're saying. Is that in the regulatory field, we have to.

MS. DIOSI: And if we're not talking about ISC, now we're talking about going into AERMOD or doing into CALPUFF -- those things are improving, but in that time that it gets improved, we still have to go forward with something, and I think this somewhat -- facilities, let's say, that have the dollars available, $20 or $40,000 may be nothing for them for an air permit. But you're talking about other smaller facilities that also want to get permitted, that's a significant investment. And I think we need to have something a little more standard for those kinds of situations.

MR. LYONS: Okay, but I guess the guys that have the $20,000, they can use something better.

MS. DIOSI: I don't know if that's -- I don't know if I agree with that -- I don't know that that's necessarily state of the science, number one, and I don't know if that's necessarily the right approach in any case.

MR. IRWIN: Okay, the debate's good, but we are running short of time. Joe wanted to finish this off at 11:30. How many more people wanted to jump in?

MR. HOFFNAGLE: This is Gale Hoffnagle. The question --

DR. TIKVART: Gale and Doug and -- okay, if you all can make it quick. We need to get on and get one more speaker in.

MR. HOFFNAGLE: To make a wonderful reference here. There's a paper by Tikvart et al that used, for steady state models, ISC, an evaluation that's

had 14 years -- 17 years worth of data, was sufficient to get the higher range of meteorological data, and that's the genesis of the, if you will, compromise of using five years worth of data that's in the guideline now. What we don't have is a similar analysis for a puff model. That's what we need in order to answer the question about do we need five years worth of data? My guess is that we don't need as many years of data, but I think that analysis has to be done.

MR. BLEWITT: Doug Blewitt, Air Quality Resource Management. I would -- I want to speak from wearing a second hat that I have, which is being part of the Colorado Air Commission, and speaking as a quasi-regulator, states have an obligation here in the near future to develop regional ASIPs, and especially in places like Colorado, there is a real void of meteorological data, especially in many of the Class I areas -- there's very limited data. This whole concept of using prognostic models like MM5 is very attractive, and I think EPA and the states need to start working this issue far more aggressively.

But I also think we have to address issues such as grid size, as Joe was saying, and some work that I'm doing, I'm seeing that grid size is very important at how you apply these things. I think we need to start really, in preparation for regional ASIPs, in preparation for the next modeling conference, we need to start forming a group of participants to really help the states, because they're going to be very short of staff to do these types of analyses. It's time to start doing our homework is what I'm trying to say.

MR. PAINE: Bob Paine, ENSR. Just a few observations from the AERMOD point of view. The use of these profiles of meteorological data would be helpful to enhance AERMOD in terms of its temporal resolution of Bowen ratio and moisture fields. Applicants, if they didn't have to go out and put out a full year of

meteorological observations and wait a year to get their permit, if they grab a profile of data from these fields, would be greatly encouraged and thankful.

How do we know that these are going to work though? I think we have to take existing onsite measurement data bases, extract these fields that would be consistent in the location and time, and run them in parallel and see if we get modeled concentrations that are consistent, more accurate -- and we have to see if they're more conservative, less conservative, and evaluate them against conventional data bases to see what is the regulatory impact, the consequence analysis, if you will, of using these data bases.

So, if we have archived data bases in the time frame -- I guess, Walt, you said you can go back to '95 with some of these fields -- if we have conventional network data or towers and profilers back to '95 and we can use those data bases to evaluate the impact of these new fields, we can get an idea of the regulatory impact.

MR. IRWIN: Well, thank you all. Joe, we'll turn it back over to you.

DR. TIKVART: Thanks to you and all the panelists for the spirited discussion and I think it was very informative for everybody to consider the challenges these numerical meteorological models ...

We do need to move on, though, with the public hearing part of this conference, to take comments on the specific proposals from both government agencies and the public in general, and in that vein, I'd like to move on and Pat Hanrahan has agreed to hold his comments until after lunch, representing state and local agencies, so I'd like to move on with Peter Lunn, speaking for the Department of Energy. So Peter, if you would come forward, and let us have your comments.

MR. LUNN: Apparently what happens when the EPA sends a letter

of invitation to the Secretary of Energy is that eventually it filters down to someone like me, and I'm tasked with giving a presentation without fulling appreciating the context of the meeting. So, I'm here representing the Office of Science within the Department of Energy, which has a primarily research mission in support of the rest of the Department of Energy and the energy needs of our nation.

So in the next few minutes I'd like to address three questions in very general terms. One is why is the Department of Energy interested in air quality modeling, if that's not already apparent to you. What do we think, in general terms about the current state of the science? And what scientific research are we conducting in support of better air quality modeling?

First of all, the DOE context. These are excerpts from the strategic plan. Part of our mission is "to foster a secure and reliable energy system that is environmentally and economically sustainable". What this means is that we care about the environment, but we also care about the economic impact.

So "to protect our living planet with scientific understanding of energy impacts on people and the biosphere".

The American industry needs greater scientific understanding to track pollutants through their complex interactions with the environment if we are to find ways of dealing with the technological challenges and the environmental issues.

In effect, we use a hierarchy of models, all the way from first principles chemistry and physics, through integrated assessment models, and what I understand of air quality models, they're sort of in the middle between these two extremes. So we are a user of these models, but in the context of this hierarchy.

This is what we feel about the state of the science at the moment. We're somewhat impressed with the advances that have been made in the state of

the science in recent years, with support from the federal and state agencies, the scientific community has responded to the earlier challenges and the current generation is much improved.

However, significant scientific uncertainties -- significant scientific issues have not yet been resolved and considerable uncertainties remain. Of course that's what I'm supposed to say because I'm a program director for a research program. But it turns out that this is true. Although we rely on the current air quality models for assessment purposes, we find ourselves increasingly turning to research-level models for the deeper understanding of the limitations of the various models that are more popular.

And of course, when you go to the level of an integrated assessment, all of the science that goes into something like an air quality model is just a very tiny part of an integrated assessment model. And integrated assessment model, then, is used to impact energy policy and a whole host of consequences.

We do remain concerned about a number of things: relationships among pollutants; about transport and diffusion issues; relationships between urban, region, and global scales; relationships between air quality and human health; and relationships between air quality and climate change. In fact, there is a growing interest in both the White House subcommittee on air quality research, and the subcommittee on global change research in seeing how climate and air quality are influencing each other.

We have a number of research activities that are addressing these issues. One is our Atmospheric Chemistry Program, which is concerned with the chemistry of energy-related air pollutants. Activities include field measurements, lab measurements, modeling, and instrument development. Current emphasis is on

urban and regional scales. In fact, we have a couple of field programs underway this summer. One is part of the central California ozone study. Another that we're involved in with other agencies is the Texas 2000 air quality study.

We also have an Environmental Meteorology Program that is concerned with the atmospheric transport of energy-related materials. The current emphasis there is on vertical transport and mixing. And we have a field campaign underway in October in Salt Lake City basin.

We have a new program called Tropospheric Aerosol Program. It's not actually funded yet, but it's in our Fiscal 02 request, and we're still hopeful of some seed money in Fiscal 01. In a sense, this is in response to the growing -- at least federal and national interest in the PM issue and the various relationships of air quality and PM and health effects and so forth. This program has been developed and we hope to implement it soon.

We also have a Research Aircraft Facility that provides G-1 aircraft in support of Department of Energy and other agency research programs. This aircraft is fully instrumented with the kinds of things it would need to do air quality studies.

And finally, for more information, I have on this slide a list of these programs, web sites, lead scientists, and e-mail addresses if any of you are interested in more information. The last address there is for the Department of Energy research announcements. Periodically we publish requests for proposals and you can get the most recent list from that web site. Thank you.

DR. TIKVART: Peter, thank you very much. Any questions or comments for Peter? Okay, thank you. That does leave us with a little free time, if there's anybody that would like to jump ahead from this afternoon, and your

comments are limited to ten minutes you can go ahead now if you wanted. Otherwise, we've been so busy, you might welcome the additional time for lunch, a more leisurely lunch than we had yesterday.

Pat Hanrahan has slipped into this afternoon, and I've had one additional request from Stanley Vasa to speak this afternoon, so I think we are going to three o'clock for sure. And if there are no other comments, we can go ahead and break now. I would ask that -- for John and for Dick Schulze, if it's possible to compile all the slides that were presented, that would be helpful because there's a lot of good information there that was talked through that the reporter got, but the slides, I don't think were made available, so if we could have those, that would be good.

We'll go ahead and break and start again at 1:15.

(Whereupon, at 11:50 a.m., the hearing was recessed, to reconvene at 1:15 p.m., this same day, Thursday, June 29, 2000.)

AFTERNOON SESSION

1:15 p.m.

DR. TIKVART: We move now to the comments on the regulatory proposal. We have a holdover from this morning. The holdover from this morning is Pat Hanrahan speaking for state and local air pollution control administrators and the local agencies. Then the other speakers for this afternoon in order are Doug Blewitt, Ken Steinberg, Andrea Field, Bob Paine, Maidhila Shararan, Stanley Vasa and Dick Schulze. Anybody else needs to speak, please let me know. So Pat, let's go ahead with you.

MR. HANRAHAN: Thank you. These here are draft comments. Two weeks ago there was a state EPA local modelers workshop, where we got together all of the local modelers with the EPA modelers to go over a number of state issues. It was at that meeting that I began formulating these issues. I had a chance to talk to a lot of the state modelers to get these issues. But I want to emphasize that these are draft comments, and that we will be refining these and submitting these by the end of the -- before the end of the comment period.

Overall, we in general, welcome the addition of the new models as they have significant new improvements. However, I know of at least one state that would like to hold back on one of the models because of a feature that's not in there -- well, I won't say what feature that is. But in general, we would like to get our hands on these models as soon as we can.

Here's another interesting feature you probably haven't heard about before -- is let's put PRIME in AERMOD, kind of a novel approach. In terms of NOND, Federal Register, EPA invites comments on the possible burden of jointly using the two models -- AERMOD and ISC-PRIME. I think you've heard already,

this is quite a burden, and I would say that the idea -- if we had to do an hour by hour approach, probably the first application that came into the first state would probably have paid for putting -- the addition of the PRIME algorithm into AERMOD. So if there's any way that we can do that, that's really the tool we need to do that correctly.

And the next thing -- this next one is something I was trying to emphasize this morning, and that's -- well, if we can't have PRIME, let's expand AERMOD's use to where the current downwash algorithms are shown as being conservative. Now there's two ways of doing this. We've already seen that there's some cases where you look at ISC versus ISC-PRIME, that the downwash algorithms, we can look at those directly -- apples versus apples -- and we should be able to see that there are certain scenarios, probably quite a few scenarios where the current downwash algorithms will look conservative.

But then there's another way we could do it, which is apples versus oranges, but may even be more applicable. We're not really interested entirely in what ISC does with and without downwash, but we may find that there's a number of applications where AERMOD by itself comes up with a higher concentration than ISC in spite of the fact that it may have a downwash algorithm for those applications that are less conservative. And the reason for that is that, in general, a number of the simple terrain concentrations are higher in AERMOD than they are in ISC and if we can identify those situations, we can certainly expand the application of AERMOD so it can be used more now. I strongly encourage that that be done.

The next thing is we're looking for some kind of a hammer to use on EPA to get PRIME into AERMOD. We'd like to say, well don't issue it until you can do it, but that kind of throws the baby out with the bath water, and we're

looking for something that we can hold EPA to to get that in, and we're asking for a delay, and how long you can have application of both ISC and AERMOD.

The next comment deals with a screening model for AERMOD. And this here gets into the point about a comparison between screen-3 and AERMOD. Again, I mention this application that Ralph Staciana (ph) has done with San Diego Air Pollution Authority, and he found that using screen-3 versus AERMOD where you have a full year's worth of meteorology, he was finding that AERMOD came up with answers that were on the order of one or two magnitudes higher, just because of the difference in the conductive boundary layer predictions from AERMOD. Nothing to do with downwash. So screen-3, we may be having a -- we may be missing something pretty important if we rely just on screen-3 as a screening tool.

I want to get back up to the green mark there. Jim Haywood with the state of Michigan, has volunteered to form a workgroup to look at this, and possibly come up with a tool that may be usable by the time that AERMOD comes into general use. And if anybody else is interested in helping in that particular workgroup, it is a workgroup, go ahead and get in contact with Jim Haywood with the state of Michigan. I don't have his phone number, unfortunately, but I do have his e-mail address that I can mail you from home.

Next thing is with respect to CALPUFF. We welcome CALPUFF. We're fairly familiar with CALPUFF as we've been already using it for several years with respect to Class I issues. There has been no recommended model for long range transport, and CALPUFF certainly seems to be the first model that seems to fill those shoes fairly well.

Also with respect to CALPUFF, the CALMET model -- right now the way it's written is that it requires National Weather Service data. I believe the

way our rules are proposed is that we're requiring the use of National Weather Service data. There may be some applications where either National Weather Service data isn't available, or we're trying to maybe do something on a finer scale where onsite data may not be available, and I question the value of the National Weather Service data on those applications. And after this I'll show you a slide as an example of that.

And my last point is something that I'd like to stress here with respect to the use of CALPUFF. Right now in the proposal, there's -- it speaks of complex winds where CALPUFF may be used on a case-by-case basis. But it says, if you read the fine print, it says that EPA shall make this decision on a case-by-case basis. And I'd like to see this, and I'm sure most -- well, I'm sure that most of the states would like to see this become a state decision, not a federal decision in that we would like to use the best science on this and not get into have to deal with politics and having to deal with different regional authorities on this. I think it is important that I feel the states have the PSD delegated authority to do the modeling. We're proposing to put CALPUFF into Appendix A. Why can't the states make a decision as far as when CALPUFF can be used on a case-by-case basis in the area closer than 50 kilometers?

This here is a CALPUFF run -- or excuse me, CALMET run where this area here is Oregon. Here's the Columbia River coming in. Here's Puget Sound up here, Seattle, and the water going out into the ocean. There's a buoy up here that measures wind speed where Puget Sound dumps into the ocean, and it is measuring something that is fairly local. So if you put in your local data, you'll get this region here where it shows it's really strong winds. But the winds around here don't seem to be affected that much by the local data that went in. You don't see a

smooth transition of these big arrows becoming littler arrows. It's pretty well defined by a radius that comes in with R-1 and R-Max-1, and I don't see a lot of importance of those winds beyond that radius, and it's similar with other sites, but this one here happens to be rather obvious.

Other comments on CALPUFF. It certainly needs the PRIME downwash algorithm and hearing from Joe Scire, it sounds like that's work in progress. We're really encouraged to hear that that's happening. We also need to see the aqueous chemistry module in there and tested. And it sounds like we're very close to having that in there very soon. We're also encouraged by that.

But one thing we'd really like to see is this one here. With this proposal for the complex winds, we need to have a better idea of how well does CALPUFF compare with AERMOD in this near field? And that's in the area less than 50 kilometers. And we'd like to see it for a number of different scenarios.

The first one I have there is how well does it do with respect to the convective boundary layer? That is, the unstable conditions. Impacts on simple terrain? Another scenario would be how well do the two models compare in complex terrain?

There's two different solutions that are used in the two models. AERMOD has an approach where you put in the DEM data and it comes up with the critical dividing streamline height, based on an estimate of what it sees as the highest terrain around there. Where I understand CALPUFF uses a CTDM approach, where it's trying to fit ellipses to the hill and I would like to see how different the predictions are from those.

And then for a number of cases, how much different is the overall design concentration between CALPUFF and AERMOD. I think that would help us

a lot in what I was pushing for in one of the previous slides, and that's for the states to make their own decisions as far as whether CALPUFF is appropriate to use in the near field area.

The next thing is something that is a bookkeeping type thing. I know with the current versions of CALPUFF and CALMET, a number of users will find out after they get a file that's two gigabytes large, all of a sudden, the model will quit running when you're running under Windows. And at first I thought, well, gee, it's a problem with Windows 95 versus Windows 98, or it's a problem with the BIOS -- all of those things really are important, those things have to be fixed in order to get beyond the two gigabyte file size, but basically what it comes down to is that the Fortran compiler that I understand is being used for compiling CALPUFF, won't allow sizes greater than two gigabytes. But anyway, it would be nice to have a warning to the users that this problem does exist with the Windows version.

ISC-PRIME. When -- yesterday when we had the introduction to the models, we've seen that there are a number of empirical equations in there, but there's very limited documentation. Yet these are rather important, and often journal articles -- they -- they're spread out over a number of different places, and they don't really usually have the space that you need to explain an equation in detail. And, Joe Scire, I'm sure, is fairly good at reading source code -- better than the rest of us -- but I find that difficult to use as documentation as well.

And further, there aren't that many empirical equations that are used in ISC-PRIME. We'd certainly like to see nice model formulation document for all of ISC-PRIME, but if we could at least get documentation on the empirical equations, what some of the assumptions that were used in coming up with some of the variables in the empirical equations, I'd say that would certainly be a step in the

right direction. And we are requesting that this documentation be available before the rules are finalized.

This is something I mentioned this morning. I do have this other format up here for National Weather Service data that we will be seeing, I think, some time this fall. I forget what ISHD stands for, but it's the new format that we will be seeing for surface data. We would like to see all of these models be able to handle the new format.

I won't go into any more about how unfriendly AERMET is.

The next question is -- when National Weather Service data is available, what should we use? This is pretty much for the Gaussian models I'm talking about here. Should we use the most recent five year data? We're talking contains ASOS data, misses the clouds, misses some of the lighter wind speeds. Or should we use a set data set? Or should we put a five year data set on SCRAM like there is now and have that be the default set? I personally would like to see something standardized that excludes SCRAM.

This is something I discussed this morning with respect to AERMET. When you do have good onsite data, why do you require us to use National Weather Service data? It's a coding thing.

Next thing is with respect to AERMOD and CTDM-Plus. We're quite impressed with the performance of -- that we've seen so far. But there's something else as far as objectivity of input on AERMOD versus CTDM-Plus, and I'll show that in the next slide.

Steve Perry might recognize this particular hill. This is out of the user's guide for the topography module of CTDM-Plus. And basically, what you have a hill -- actually it isn't all that complicated, but it's kind of elongated in two

different ways. And how do you make ellipses fit this best? And what the user's guide came up with is this here.

This, as I mentioned, is a simple hill, where he's got some of the ellipses going that way, and some of them going this way. But that does lead to a little bit of subjectivity. You'll get different answers as far as which way you put your major and minor axes on this. It seems to me like some of these contours, lower contours could have had ellipses going around that way as well. And getting around this kind of subjectivity is very handy. We're curious that we see that in AERMOD.

Okay, this is just a reiteration of the comment I had about AERMAP -- your domain can affect what it sees as the highest elevation, and again, if we can get a preprocessor that comes up with this maximum -- what is it -- the receptor -- the small h_c whatever that's again -- the receptor height scale I think it's called. If we can get that processed by wind sector for a single source, that certainly would be very, very helpful.

Getting away from CALPUFF and the other models, with respect to regional models, we see an aggressive promotion of Models-3 and we see things like UAM and other less complicated regional models go by the wayside. I've heard from a number of states that have used UAM in the past. They'd like to continue using it in the future, and it sounds like they'll still have that option. But -- and also with respect to Models-3, I know even within the western states, the WRAP -- the Western Regional Air Partnership -- they're intending to go aggressively using Models-3.

But frankly, there haven't been that many performance evaluations of Models-3 up until now and it may be a little bit premature to be recommending

those as something that we need to be using in a regulatory approach.

The last thing I have here is for the timing and application of these models. I know that a number of the states would like to begin using these now, and because they feel they do have better science, and we would like to see some type of a mechanism that would expedite the use of these models as soon as we can.

This is dealing with -- getting back into CALMET -- there is a section there, there is a section in there that deals with how long of a -- or how many years of met data do you need, and it speaks of either using five years of National Weather Service data or something less than five years of mesoscale data. And we generally support that language in that it's flexible. It's up to the states to decide what something less than five years is. Right now we're probably looking at one year when you have that kind of data because it takes a lot of effort to develop the mesoscale data. But we have seen, from work that I've seen out of North Dakota with Steve Webber, that there can be quite a bit of variability from year to year, when you do have five years of National Weather Service data used with a puff model. He has data to show that.

Next point is with respect to the ozone limiting method. Especially from Nebraska I had comments that they're welcoming that back as a third level tier for modeling how much NO_x is converted to NO_2, but they do ask for some expanded flexibility as far as how you treat that with modeling multiple sources.

And the final point on this one is that we're encouraged by the section which allows consideration of more refined techniques for modeling NO_2 on a case-by-case basis. I happen to know one that has my name on it.

With respect to DEM data and AERMAP. These are the same things I mentioned this morning. I'll include those by reference.

And the next part, down in green -- oh, I want to emphasize that the one thing that we would like to see out of this is that if we could have some validating processing in AERMAP so we could get warning about sudden changes in elevation and data gaps. I think that would be a really big help, because if somebody's out in the plains in Kansas and all of a sudden they see their elevation's increased by a factor of 3.28, they might understand what might be going on.

With respect to AERMOD, this here is based on ISCST2 -- is where they got their start for this. That does need to be updated to bring in some of the other features that are in ISCST3, the event processor, the Fortran-90, and to have it compile so that we have executables that allow more sources and receptors. And we probably could increase those by a factor of ten real easily, and it would still fit in to a modern computer.

And this is the last thing that I wanted to talk about. We're now having as many as four models that we would be proposing for the near field area -- ISCST3, our existing model, ISC-PRIME coming in, AERMOD, and CALPUFF on a case-by-case basis. From a state level, we certainly don't want to see people taking advantage of the idea of running all four of these, see which one comes up with the lowest answer and then giving that to us.

And how we do this? There's probably -- I've heard a proposal from New Jersey with Alan Dresser. He had proposed that we should require the applicant to run two different models and to give us the higher of the two. Well, that's maybe one way of doing it. But maybe a better way to do it would be for the states to come up with their own protocols to figure out what is the best niche for each one of these models, and to be recommending just one of them to be used for that niche.

And whatever way, I would like to see that we -- states do have the flexibility so that we can use the best science to get around the problem of gaming. And that's all I have, and I thank you.

DR. TIKVART: Thanks, Pat. Any questions? I have none. Okay, thank you. Then I presume there are no other federal, state or local agencies who wish to make comments at this time. Yes.

MR. HOLMES: Yes, I'm from the National Research Council, and I'm like Peter Lunn, I'm required to be here because of my agency affiliation. And I guess my only comment is --

DR. TIKVART: What was your name?

MR. HOLMES: John Holmes, from the National Research Council. My only comment, I guess, is in the summary of the rule, where it characterizes most of the modifications to Appendix W as being associated with AERMOD and CALPUFF. But when you read -- and the editorial changes being the replacement or the delisting of the urban air shed model. And I think it's significant that you have delisted that, and potentially replaced it with Models-3. And not listed that in the summary, and characterized that as a minor change. I think most people would think of that in the long range transport air quality modeling as a major change that probably deserves a conference like this to be discussed a little bit more fully.

DR. TIKVART: We agree with you and in Models-3 and REMSAT are not recommended models, they are not in Appendix A. We've identified them as tools that can be used for regional scale applications of ozone and PM.

MR. HOLMES: But now there's no ozone model in Appendix A.

DR. TIKVART: That's correct. That is the proposal. Right. And we did not feel that we could actually propose a model because of the state of

evolution, but we did want to recognize that Models-3, CMAQ and REMSAT seemed to be the tools that were ready for use.

MR. HOLMES: Okay, but REMSAT is not associated with the ozone proposal.

DR. TIKVART: It's PM, that's correct.

MR. HOLMES: It's PM. And I think some of their developers would think it should go in the ozone side too.

DR. TIKVART: Okay, thank you. Any other questions or comments from governmental agencies? Okay, if not, let's go forward with the rest of the comments. Next, I have Doug Blewitt speaking for the Gas Research Institute.

That reminds me, Pat, can you get a copy of your slides to the Court Reporter? And Mr. Blewitt is doing it exactly correct. He has given me a copy and the Court Reporter a copy of his presentation. Thank you very much.

MR. BLEWITT: Thank you. My name is Doug Blewitt. I'm a Certified Consulting Meteorologist with Air Quality Resource Management. I'm here today on behalf of Mr. Jeff Panek, an air quality project manager with GRI who is unable to attend these hearings. These comments were collaboratively prepared between AQRM and GRI.

I am here to testify here today on behalf of GRIs membership regarding proposed changes in the air quality modeling guideline. My comments today will focus on the inclusion of CALPUFF into the modeling guideline. Our written comments will focus on a broader perspective to changes to the guideline.

Let me begin my comments by describing the modeling needs and

requirements of the natural gas industry. I will next describe the attributes of models to fill these needs and how CALPUFF compares to these attributes. I will make recommendations and suggestion to address our technical concerns regarding the CALPUFF model.

We are recommending that EPA conditionally approve CALPUFF as a guideline model, pending further analysis and investigation. GRI acknowledges that EPA has not conditionally approved a model before, but we feel that this is a unique situation. We're defining conditional approval as allowing the use of CALPUFF in regulatory applications, but allowing additional analysis to be conducted and funded by EPA and peer review of these additional analyses. We think this is -- we're really concerned that we don't freeze technology at this point, because this is an emerging tool.

GRI believes that this is a very promising model, but further analysis needs to be conducted.

Let me just describe the needs of the national gas industry with respect to air quality modeling. The natural gas industry currently requires air quality models that accurately predict primary and secondary pollutants of SO_2, NO_2, nitrates and sulfates at downwind distances of up to 200 kilometers -- typical Class I type analysis. Over the last five years, our industry in the inter-mountain West has been required to estimate potential cumulative impacts of new natural gas development in adjacent Class I areas. These analyses have required examination of Class I impacts with respect to acid deposition and visibility impairment.

To put the national gas industry in perspective, a recent forecast of the National Petroleum Council expects our industry to have an increase in production of 33 percent over the next 12 years. This increase is even more

dramatic when one considers that this increase is in addition to the natural decline of existing production, thus the true number of wells in production will increase above this 33 percent. And the majority of this increase will occur in the deep water Gulf of Mexico and the inter-mountain West.

This increase in production will occur at a time when the regulatory framework is changing. The recently promulgated Regional Haze Regulation will change the way analyze and document impacts from natural gas production. So while we have a need today for doing such analyses and needing such a tool, the need will increase even more so in the future. Thus it is important that the model that is finally incorporated into the guideline for estimating potential air quality related impacts be demonstrated to be accurately applicable to natural gas sources.

In trying to address the issue of natural gas impacts, it's important to understand what questions are being faced by the natural gas industry. The question that we're being asked is, what is the potential reduction in visibility associated with the operation of natural gas fields in combination from other sources that are not reflected in background conditions?

In attempting to answer this question we must address impacts on the cleanest days, days with maximum visibility impacts, as well as the number of days of which visibility impairment could occur above a defined threshold. In order to answer these questions, it is necessary to use an air quality model in a sequential mode and predict impact for all hours of the year. It is not possible to answer these questions by simply looking at episodes. Further, it is important that such model simulations be run on engineering workstations as opposed to a super computer.

It is important in framing this modeling problem that we do not freeze technology and preclude the use of other models as technology advances.

However, models must contain attributes to simulate time and varying wind fields in regions of complex terrain, such as the inter-mountain West, and the Gulf of Mexico, deal with atmospheric chemistry of sulfate and nitrate, wet and dry deposition, and estimating accurate concentration estimates over large modeling domains.

While CALPUFF -- while the CALPUFF model satisfies these conceptual requirements, it has not been adequately tested or evaluated for Class I air quality analysis. GRI's concern about the inclusion in the model -- of this model into the guideline without supporting analysis. This places the natural gas industry in a very undesirable position. We need a model today that can accurately simulate impacts, but the tools need additional verification and testing.

In addition, EPA needs to develop guidance on the application of the tool beyond the current user's manual, and such guidance needs to go through the public process.

I'd like now to address specific issues that we've addressed with the CALPUFF model.

The first is the overall model evaluation of the system. We don't believe that the model has been adequately tested or verified or evaluated. There have been a number of very limited comparisons of model predictions to observed data. In reviewing the documentation associated with CALPUFF, there are a number of limited evaluations that have been conducted. From the information in the docket, it appears that EPA evaluated the model against the Great Plains Tracer Experiment in Norman, Oklahoma, and the Savannah River Laboratory Experiment. In addition, an evaluation was conducted using the INEL Tracer Test. We are also aware of other model data comparisons have been done, for example, CAPTEX.

While these model comparisons show that CALPUFF can replicate to some extent the observed data, there are significant limitations in these studies.

The information in the docket provides no overall statistical calculations to quantify model performance. Both the Great Plains and Savannah River experiments did not involve any test of the model's ability to simulate concentrations in terrain. Thus, there is very little information regarding model performance over large distances and complex terrain.

In addition, EPA has not provided any testing of the accuracy of the CALPUFF chemistry models. GRI believes that this is a serious omission on the part of EPA. Our overall conclusion is that CALPUFF has not been evaluated for the actual conditions where it will be applied. Class I impacts with full chemistry, complex terrain over large dispersion distances.

GRI acknowledges that the data used to evaluate a model like CALPUFF is very limited. We're always faced with data base limitations. We would suggest that the Mount Zirkle study that was done a number of years ago and the Southwest Wyoming Technical Air Forum may be possible data bases that can be used for further evaluation. In addition, the upcoming San Joaquin PM 2.5 field program may be another possibility for further evaluation.

So there potentially are three new data bases that could be used to try to evaluate this model in the context that it will be used, and I think that's very important.

CALMET -- the next point is the accurate development of wind field. CALMET has the ability to use meteorological data from onsite stations, National Weather Service data, and output from prognostic models such as MM4 or MM5. The use of such a wide assortment of input data is a very desirable attribute of a

model such as CALMET. However, there is no guidance on what is a minimum amount of data needed to obtain an accurate wind field. The answer to this question is very site-specific, and there has been no guidance issued by EPA or the model developer, EarthTech.

This problem is further complicated by the fact that it is difficult to determine how accurate a calculated wind field replicates an actual wind field flow, especially in complex terrain. Part of the problem is that modelers tend to use all data in wind field simulations, and therefore there is no independent data for model evaluation. To address issues associated with wind field development, model sensitivity analyses need to be conducted along with evaluations where meteorological are withheld from simulations so that model performance can be judged.

It is also important to evaluate how changes in wind field can affect predicted concentrations. For example, if the wind field model underestimates the true wind speed, what effect will this have on plume dispersion and the conversion of primary pollutants into secondary pollutants. EPA should develop guidance and understand CALMET sensitivity to various input parameters.

Chemical mechanisms. The CALPUFF model has two basic chemical systems, the MESOPUFF II and RIVAD chemical mechanisms. Both of these mechanisms are highly parameterized simplifications of the actual chemical processes.

GRI believes that it is necessary to make such simplifications regarding chemical mechanisms in order to enable efficient simulations. However, we believe that it is imperative that both of these chemical mechanisms, as well as other chemical mechanisms, be carefully tested and evaluated in the overall context

of the CALPUFF modeling system.

Background ammonia concentrations. Background ammonia concentrations play a very important role in the formation of sulfates and nitrates. IWAQM suggests a background concentration of 10 ppb be used for grasslands, and five ppb for forested areas. Recent research in the inter-mountain West suggests that sulfate and nitrate formations may be limited by the amount of ammonia present in the atmosphere. Previous applications of CALPUFF have assumed uniform temporal and spatial concentrations of ammonia. Because ammonia emissions are released close to the ground, it is very likely that ammonia will undergo substantial deposition and conversion.

Further, ammonia levels have strong seasonable composition and will not be uniform with height. Thus, for elevated plumes, there may be very little ammonia available for reaction until the plume is uniformly mixed throughout the mixed layer.

GRI believes that additional research needs to be developed on what background levels should be used in the model, as well as the distribution of ammonia within the mixed layer. And I think, as a side note, the San Joaquin PM 2.5 study will start to look at the issue of distribution of ammonia within the mixed layer, so there may be some additional data coming out of that.

Next issue is modeling partial inventories. This is one that people have not really touched on. When the model is used for natural gas sources, what we're really trying to ask is the question, what are the changes in visibility of new sources plus sources not reflected in the background? But this requires modeling of a subset of the total inventory. And when you have competing reactions between sulfate and nitrate, how do you deal with the partial inventories?

The model developer, EarthTEch, has developed a process for recomputing nitrate levels using a repartitioning technique. This portion of the model has not been available for review at this time, and the use of this repartitioning technique would require the modeling of entire emission inventories, and then creating separate modeling runs to simulate the changes in the partial emission inventory, and recomputing nitrate levels.

While this is a promising technique, further evaluation and study needs to be done, and we need to ensure that the implementation of this procedure will result in truly more accurate estimates of visibility impacts.

Visibility calculations. The method that CALPUFF utilizes to estimate the change in visual range needs careful review. The IWAQM Phase I interim report is the basis for estimating the change in visual range in the CALPUFF model, with modifications to deal with recommendations in the FLAG draft FLAG document.

One assumption of this approach is to calculate the changes in visibility as a function of relative humidity for sulfates and nitrates. The effect of relative humidity on particle growth is based on experimental data collected by Tang. Review of his original data indicates that all his experiments were done at an ambient temperature of 25 degrees C. Justification of the use of this procedure for temperatures other than 25 degrees C is needed.

GRI believes that the application of the CALPUFF model to estimate changes in the visual range needs to be carefully evaluated and revised and subject to peer review.

As previously stated during my testimony, GRI has concerns regarding the inclusion of CALPUFF into the modeling guideline, but does support

conditional approval. We urge EPA to form a cooperative stakeholder peer review committee to address additional studies and analysis in support of this model. We feel that this committee should be comprised of EPA, state agencies and industry. We, as GRI, would welcome the opportunity to participate in such a forum.

In conclusion, GRI is recommending that EPA conditionally approve CALPUFF into the modeling guideline, and final adoption would be pending additional analysis. Since many costly control strategies will be selected based on modeling estimates and implementation of tools that are being proposed today, it is important that EPA fund and support continued testing and development of these tools. Interim review by the broader community, not a case-by-case modification of the tool is required to support small, independent companies with limited resources. EPA needs to approve a process by which these tools can be maintained in the current regulatory context, and scientific knowledge in between modeling conferences. Thank you.

DR. TIKVART: Thank you very much, Doug. Any questions for Doug? If not, let's move on. Next we have Ken Steinberg, speaking for the American Petroleum Institute. Well, another adventuresome computer presenter.

MR. STEINBERG: In some ways I feel like what I'm going to say you've already heard, so I'm just going to be adding my voice on many issues, I guess, as representing the American Petroleum Institute, and while they're trying to get the overheads to work -- I've been sitting around and making some observations that I just will share with you for a moment.

I think it's worth noting that the AERMIC committee has really done a very fine job with AERMOD and certainly I think the EPA staff has been working very hard to assure that sound science is brought into regulatory modeling. And I

just wanted to mention that certainly some years ago I had my doubts whether that was going to happen, and I personally, and certainly on behalf of the American Petroleum Institute and the representatives of the air quality task force -- I think that we all share in those comments, those thoughts.

However, just like everything else, I think that there has been a lot of labor of love is the impression that I've gotten, a lot of sweat equity is maybe another way of mentioning it. I get a perception that within the agency there's been quite frankly, too low of a priority with regard to -- so the point I wanted to make was the perception I have is that the agency itself needs to place a higher priority on some of the work that's been going on.

Certainly I think it's a no-brainer at this point that AERMOD-PRIME appears to have been something that in the five years since the last conference just hasn't happened. It's hard for me to understand why, other than it just being a matter of funding and priority, and I think one of the messages that maybe needs to be stated succinctly is that EPA management needs to get its act together for the user community.

I guess another way of looking at it, as I was listening to some very interesting talks and maybe some that weren't so interesting -- and maybe you'll decide when my mind was drifting here -- I was thinking of trying to put it in some kind of analogy -- and I'll give you my poor analogy of what this all seems to be.

Maybe this is going to be for some of the younger people in the audience maybe not quite so clear, but, I see a number with gray hair and I think most of those will remember the bicycles of the fifties, where they had fat tires, and the better bicycles sometimes had these springs in front so they had all kinds of shock absorbers and sometimes mud flaps and they were all gussied up, pretty much

like the cars of the fifties, with fins and what have you.

In a way I kind of look at the Gaussian modeling that we've been using of late, really harking back to that area, and now we're being brought into the exciting eighties and nineties where we're being shown this brand new ten or maybe 20 speed bicycle, and we're almost like teenagers again, chomping at the bit to get that bike. It's nice and new and it's shiny, and we all want to use it.

But the salesman tells us, you know, it has ten gears but only six of them work. You can only use this on a sunny day because if you try to ride the bicycle after it's rained, there's no fenders, and so you're going to get all wet and spoil your shirt. And so you're still enthusiastic and you still want that new bike, but you kind of wonder whether the manufacturer is really serving his customer, his constituencies by offering something that's really incomplete.

And I guess that's the feeling that I have as I sat out there for the last day and a half. And I guess I want to throw my voice, and maybe API's collective voice for the air modeling task force behind what some of these people have been trying to do and pretty much kind of supporting the notion that Doug was saying.

I think that there's an opportunity -- again, if you'll permit me one more analogy -- I remember years ago, I think we've all, at one time or another, on Labor Day turned on the Jerry Lewis telethon, and again, about maybe 15 years ago, Jerry Lewis, right in front of the TV kind of had a realization that he was beating his head against the wall. He was bringing in one million, two million, three million dollars, four million, making small incremental changes and all of a sudden he said, you know, there's a lot of industry out there that if I can only tap into and try to draw their support into what we're doing for the common good of these children -- all of a sudden his horizons changed, and today he's collecting $55 million dollars

for essentially the same cause. He got the other stakeholders involved.

I think that's really the challenge that I'm going to conclude with, but I'm kind of giving you an overview in these silly analogies, of what I'm trying to say while I can keep your attention.

Because soon I'm going to go on here, and I'm sure some of you may well doze off and I'll understand. Okay, well, let's try and get on with that.

For the overview comments -- well, as you might have imagined by this point, the API does support the use of sound science. We have for a long time, and we're encouraged to see that this may actually move its way into the guidelines. We do have some concerns about the lack of guidance for applying these new models, and I'll get into that a little bit too.

And as I guess I'm implying, and maybe saying flat out now, API and the air modeling task force is certainly willing to step up to be a participant in this process in an appropriate fashion with the other stakeholders. And again, I leave that as a challenge that I hope will be picked up and run with.

I'm going to talk a little bit about some of the API model evaluation work and maybe some of the improvement work that we've been sponsoring, and where I think it fits in. I also, in the course of doing that am going to compare outside of the paradigm here -- I view that a lot of what we've been asked to look at are these particular made in US kind of models, and sometimes I feel like -- I guess I'm going to go into another analogy.

I think we've all seen these little puzzles where you have nine dots and you're given the assignment of drawing a line through all of them, but you can only pick up the pencil once -- or beg your pardon, twice, and you struggle and struggle to try to stay within the dots to do it. Well, as soon as you work outside

the dots it becomes a trivial problem.

And again, I think looking beyond the paradigm of made in USA could very well help us, at least in the short term, bridge some of the problems, because as I said in that earlier poor analogy, the bicycle doesn't have fenders.

The evaluation study that I'm talking about is a study that looked at AERMOD, ADMS, the ISC model -- this is a study that was presented at an AWMA meeting. A precursor of that was also a study that was presented in Europe at one of the modeling framework homogination studies or conferences. And the basic bottom line is that both AERMOD and ADMS showed improvement over ISC. We looked at the five similar studies that -- three of the studies that API looked at -- Kincaid, Indianapolis and Lovett, and then we looked at two studies that API had funded, which were somewhat unique because they tended to look at point -- sources that were affected by downwash that were more from fugitive emissions, multiple point sources at near ground level in and around process areas or tank farm areas.

I really apologize for doing this because this really puts me to sleep when I'm out there, but let me just try and go through it a bit. As in all of these studies, you end up with some performance measures that you're trying to gauge how well or poorly these various models perform. And we've used four. We looked at the maximum of the peak to observed; we looked at a measure of the systematic bias; and we looked at a measure of the scatter -- random and systematic bias; and then those that are within a factor of two.

And I put up here in the column to the far right as you view it, what represents a perfect score. So as you look at a perfect model for the maximum predicted to observed, you can see that AERMOD and ADMS are doing pretty

well. A value, of course, under one is some underproduction, whereas over one is overprediction.

In the systematic bias, what we're seeing a reasonable systematic bias for both ISC3 and ADMS. AERMOD seeming to show a little greater systematic bias. From a standpoint of scatter, ADMS looks to be the best. And within a factor of two, it's almost, well again, AERMOD and ADMS look better.

On this next page it gets a little simpler. This is -- I think David Carruthers presented some of this yesterday. Here it's just kind of looking at the six separate experiments and four performance measures and kind of tallying, assuming that they're all equal weight, which model does best how often. And again, the fact is that ISC3 does best some of the times, but far more often you see AERMOD and ADMS doing better, and similarly, you see them doing pretty well in the middle range, and very seldom being the worst model for these performance measures.

So this is kind of a more simplified way of saying, you know, we really do see that the improvements in this model are shown when you actually compare it to data without getting captured into the details of the statistics.

But one of the conclusions that we come away with is that ADMS is certainly equivalent to AERMOD, and that was also borne out by the scientific evaluation of what's actually inside the code. And we could say it should be considered as an Appendix A, and after hearing some of the concerns in the audience and going back to my looking at the nine dot analogy, if we get outside of the box, we should try to see how we can allow applicants to use state-of-the-art models that are fully featured to try to avoid some of the problems that we've been talking about for the last day and a half. I think that's possible, although unlikely.

For the refinery site study, in this case ISC3 actually performed best,

and it performed best with the urban dispersion coefficients, so -- while we did not use ISC-PRIME -- again, ADMS is somewhat similar in its downwash treatment, there's still some work that needs to be done, at least as we apply these models to refinery type sources.

Then there's the question that's been brought up by --yesterday -- as well about some of these other models -- SCIPUFF which has gone through pretty extensive peer review, and of course -- where's AERMOD-PRIME, as mentioned earlier? We're looking at cost to run multiple models and maybe we're going to have to use experts as John has alerted us, and those costs are going to be real, and we really would rather have our cake and eat it too, if you will, but let's get the models done and get them done so that we can use them.

Now from a standpoint of -- switching gears a bit to the guidance -- just again, more of a qualitative statement. There's a lot more knobs in these new models. For example, surface roughness and some of the met inputs. We really need to have more guidance, better guidance, on exactly how these things should be set so that we can take advantage of the science in our applications.

And we need guidance in applying them in a regulatory context where we're looking at even these transitions from one model to another, and sometimes even within a given model where we have a variety of met options. I know in these evaluation studies that EPA did with AERMOD, I don't believe that any of the vertical profiling parameters were a part of those evaluations, yet that is a key feature of these new models. So we do want to make sure that we understand what's the right meteorological data to use when we have choices. And we're not interested in gaming. We're interested in trying to find accurate predictions of what the impacts are.

Of course -- this is putting a bureaucratic hat on. You know, there does need to be appropriate rulemaking procedures and processes to avoid confusion, inconsistent judgements and of course the corresponding potential higher costs.

Now another area where API has done a lot of work, and certainly John is aware of some of it because he's been involved, has to do with how do we estimate surface roughness. Refineries are located, are fairly massive in the space that they occupy, and it's very difficult to categorize them as an urban center if you look at many of these tables. And so API has gone through some efforts to help characterize what are appropriate values of z_0 around industrial facilities and we think that this can update some of the earlier approaches that have been available, and have been used and are rather subjective.

The AICHE's Center for Chemical Process Safety is continuing the efforts to provide guidance on how to apply this in a book that they're producing in their concept series. And this is an area that we've been, as a group at AICHE, very anxious to get EPA more directly involved, and I'll be talking to John and Joe later about trying to make that happen.

Finally, kind of comes back to the challenge. Kind of summing up that we do need guidance, and there are clear cases of additional evaluation. API certainly is willing to work with EPA and other stakeholders. This isn't a new idea. The Petroleum Environmental Research Forum recently concluded a very successful dispersion modeling project looking at accidental release models, where CRADAs were established and EPA did some work and industry did some work, and we set up a team and a management structure, and it worked quite nicely. So I just mention that, that this can be done and we hope that it could be incorporated in this

area as well.

That concludes my comments. If there's any questions, I'll try.

DR. TIKVART: Pat?

MR. HANRAHAN: I have a question regarding your underlying sentence up there. How can we get this cooperative effort going between the various stakeholders to get this going? This is something like -- I can see the first few applicants going through more costs than it would to put -- than what it would cost to put PRIME into AERMOD. How can we get this going? Do you have any suggestions?

MR. STEINBERG: Well, actually I thought that question was posed to Joe. Well, certainly in the PERF project it was a rather informal beginning. We held a -- called and asked various people in EPA if they were interested in the work that we were proposing, and were they doing any work that was related to that? We held a meeting. We established that yes, there were areas of mutual interest and by pulling the stakeholders together, we brought in different organizations from government, from this government as well as Europe, as well as Canada. It's just a matter of networking and it's a matter of having a common interest to do it. But certainly API is well known and have been working with Joe in areas in the past and I think we're at a point where maybe we have to refresh that and I think that some of these problems that we've been talking about can be solved rather quickly.

Although I would add that one of the aspects that the industry would be a little bit reluctant to do, and this is kind of coming from the very top of most of the corporations, they feel like they're being taxed enough, and so the notion of lobbing money over the fence and then saying we'll be back in two years and tell us what you've done with it -- that's not going to make it. It needs to be stakeholder

participation.

And in some instances, what that will mean is we establish what the work is to be done, and we go out and select the different contractors and set up a CRADA, which is an instrument that EPA has used any number of times, very, very successfully to make that happen. And industry walks away with a responsibility to deliver, and our contractors work with the appropriate study teams that have been identified by the various groups, and you'd be surprised, it works very nicely when you get it to a working level, to deal with the specifics, as opposed to the rhetoric.

DR. TIKVART: There was a question in back. Go ahead and use the mike that's there. Hopefully it's on.

PARTICIPANT: Yes, just one quick comment on the ADMS model. I just think that models like that would have a better chance of getting in the guidelines if the developers were willing to put the source code in the public domain, which I think David said it isn't in that case yet. And I think that's an important -- I know, case-by-case, theoretically you can use almost any model if the regulatory agencies approve it, but I think if you want general approval from the modeling community, it needs to be put in the public domain.

MR. STEINBERG: Certainly, from an API standpoint, we agree with you that that is a far superior way of doing things, but you know, I think there's some very creative ways of working around that. I took from David's answer to a question whether you or someone else posed, that as that type of thing would be required, they would be happy to do that, revealing it under a confidentiality arrangement with the appropriate regulatory agency. It's the sense I got. Whether that's sufficient, I don't know.

But the only other aspect is we have the EPA in their RMP

requirements for modeling, making no such requirement about the codes being totally made available, and that program seems to be working very well. So, I think it can be dealt with, but I'd be reluctant, personally, to say at this point that we should be using that as a reason to throw the baby out with the bath water. I think that we ought to be able to work that out.

DR. TIKVART: Okay, Ken, thank you very much. I think we should move on. If you don't want to damage your knee, there are steps over here. Let's move on with Andrea Field, speaking for the Utility Air Regulatory Group.

MS. FIELD: My name is Andrea Bear Field. I am speaking today on behalf of the Utility Air Regulatory Group, UARG. I am going to be addressing three policy issues and then ENSR's Bob Paine is going to be presenting UARG's comments on CALPUFF.

I'd like to talk first about the need to maintain a level playing field in model selection. EPA says its modeling guideline is designed to recommend air quality modeling techniques for assessing ambient standard, or criteria, pollutants. Of course the criteria pollutants that most of us are interested in right now are ozone and fine particulate matter, including secondary PM 2.5.

EPA has made it clear that it prefers to use multi-state, regional approaches for developing control strategies to assess ozone and PM 2.5 concerns. Because the proposed modeling guideline now contains no official preferred models for regional approaches, we would like to think that the choice of model for use in regional ozone and PM modeling situations will be determined on a case-by-case basis, where participants will be allowed to consider and use any of a number of models that have been proposed as alternative models. For example, and not exclusively, CAMx or UAM-V. Language in the proposed guideline, however,

suggests that this may not happen.

In particular, the proposed guideline says -- and this is on page 21519, that agencies with jurisdiction over areas with ozone and secondary PM 2.5 problems are encouraged to use models such as the Models-3 CMAQ system. Our experiences with multi-state regional efforts have taught us that decision making in such groups is not guided by the views of individual states whose roles tend to be minimized, nor is it guided by members of the public, whose roles are virtually eliminated. Rather, the groups' decisions are firmly guided by EPA, which is now on record as endorsing Models-3/CMAQ, even though that system has not passed the tests that must be passed before a modeling system can be considered for guideline status. And even though that system may be more difficult to use than alternative modeling systems.

We agree with EPA that it is not now appropriate for EPA to list any model as an Appendix A model for use in these situations. That being so, though, we believe it is inappropriate for EPA to encourage the use of any one modeling system over others. We thus request that EPA delete its endorsement of Models-3/CMAQ from the modeling guideline unless or until there is a sounder justification for such an endorsement, and we ask that EPA not use other means in regional or other proceedings, to press for the use of Models-3/CMAQ unless and until that system is indeed ready for prime time.

Next issue I want to address is new source permitting: what is in the guideline versus what is in informal agency guidance.

The modeling guideline provides that it is to be used to determine if proposed or new or modified sources can be built and operated without causing or contributing to violations of the ambient standards or PST increments. The

proposed guideline, appropriately in our view, indicates that many decisions about the sue of modeling techniques in new source permitting situations are to be made on a case-by-case basis, taking into account unique characteristics of each situation.

And I can give you an example. The guideline says in one place that the selection of receptor sites should be a case-by-case determination, taking into consideration the topography, the climatology, monitor sites, and the results of the initial screen procedure. We think that's appropriate.

Our concerns are not now about what is in the proposed guideline, which does promise reasonableness and flexibility. Rather, we are concerned about what happened in the real world because we have seen what has happened in the real world in past new source permitting situations. Specifically, instead of encountering flexibility in the design of modeling analyses, permit applicants have been forced frequently by EPA's regional offices, to adhere to the much more rigid terms of informal agency guidance documents, including, for example, EPA's draft 1990 New Source Review Workshop Manual, which is reference two in the proposed modeling guideline.

If EPA intents informal draft documents to be binding on all new source review permit applicants, we believe it should say so now and allow the public to comment on the problems posed by such documents. If EPA does not intend all new source permitting decisions to be based on what is in draft manuals or other informal guidance, it should clearly say so now and then take steps that are necessary to ensure that those responsible for implementing the new source permitting program understand and adhere to the more flexible approach as promised in the modeling guideline.

Finally, I'd like to address the need to encourage the development

and use of new modeling tools.

In both SIP revision and new source permitting situations, time is frequently of the essence. In the case of SIP revisions, states now face increasingly more stringent deadlines for the development and submission of adequate implementation plans to address attainment and maintenance of existing and new ambient air quality standards.

The Clean Air Act gives states three years, and only three years after nonattainment designations take effect, in which to develop their plans, and many groups are on record as saying that EPA does not have the authority to alter that statutory deadline. Therefore, affected states do not have the luxury of being able to wait until modeling techniques can be developed for use in assessing specific SIP revisions. They need to have those tools developed and identified ahead of time so that they are available and can be used when needed.

Similarly, when source owners seek permits to build new sources or make modifications to existing sources, time is of the essence. Utilities and utility customers facing power shortages cannot afford lengthy waits before permit applicants get the permits needed to build additional, sorely needed, and very well controlled, new electric generating equipment. Similarly, competitive industries cannot afford to wait a year or more to get the permits they need before they can produce products for sale in the international marketplace.

The new source review process, however, including the modeling portion of that process, can frequently take more than a year. That is particularly true where a proposed new source is to be located within 200 kilometers of federal Class I areas, and special modeling must be done to evaluate the impact of the proposed new source on air quality related values in those Class I areas.

A major problem with the current and proposed versions of the guideline is that they do not contain enough off the shelf models needed for use in the SIP revision and new source permitting situations likely to arise in the next few years. It is not surprising that the guideline does not list models for use in these situations, but because such situations are not uncommon, we believe more should be done to encourage the development of the tools needed in such situations.

We are heartened by the fact that EPA acknowledges in the guideline, that there is a pressing need for the development of models for a wide range of regulatory applications, and that EPA is soliciting the submittal of additional refined models that more realistically simulate the physical and chemical process in the atmosphere and that more reliably estimate pollutant concentrations.

EPA has also added a new section to the guidelines, Section 3.2, Use of Alternative Models, in order to establish clearer rules for the tests that alternative models must pass before they can be used. And yesterday, an EPA representative stated clearly that it is not EPA's intention to eliminated the possibility of the use of alternative models, even when an Appendix A model is available for use.

We are glad that EPA is publicly declaring that it wants to encourage the development and use of a broader range of modeling tools. If that goal is to be accomplished, however, EPA must help create the climate in which model developers and model users will be encouraged to develop and use sound alternative models.

Historically, such a welcoming climate has not existed. Even when parties have tried to get models added to the guideline, or when parties have in individual cases have tried to get permission to use alternative models. Looking first at the -- how you get models listed in Appendix A, we believe it has not been

impossible to get other models into Appendix A, but it has been extremely difficult to do so.

Most of the models previously and currently listed in the guideline for widespread use are those that have been developed by EPA, and not by those outside the agency. And there are not many opportunities to get models into Appendix A. With few exceptions, EPA has not added models to its guideline except during the regularly scheduled guideline reviews, which occur no more often that once every three years, with actual addition of models to the guideline occurring less often than that.

Also, EPA has occasionally been unwilling to include a model or modeling technique during its triennial reviews because the agency has been in the process of developing an alternative model or technique that it wanted to include in the next version of the guideline. We know first hand about these problems because of the difficulties we went through in urging EPA to include the rough terrain dispersion model, RTDM, in the guideline. While it's not useful to go over our battle, let me just say that we thought that sometimes the hill that had to be climbed to get RTDM into the guideline was an excessively steep and unnecessarily treacherous one.

Also, UARG members have run into immovable obstacles when seeking permission to use alternative models in individual new source permitting cases. Obstacles in defining the tests for the alternative model that it'll have to meet; obstacles in evaluating the results of those tests.

UARG believes that those who have spoken here on behalf of EPA truly want the modeling guideline to be a flexible tool. And we -- we certainly believe that EPA does not want its modeling selection process to be systematically

weighted against outside developed models. The fact that we and others, though, think that is a problem suggests that if EPA truly wants to encourage the development and use of sound alternative modeling systems, it has to do something more than simply say it would like alternative modeling systems to be developed.

We believe that there are affirmative steps that EPA can take to create a more welcoming atmosphere for the development and use of alternative models. One thing that EPA could do would be to have a clearinghouse on the SCRAM website, devoted to listing instances, both past and current, in which alternative models have been accepted for use. The existence of such a source of relevant information will help EPA, permit applicants, and model developers. If such a clearinghouse includes many instances where EPA has approved the use of alternative models, that will demonstrate EPA's commitment to the development and use of a broader range of models.

Permit applicants will also be helped in situations where Appendix A models are not appropriate for use, a clearinghouse would let applicants determine if a technique exists that has been used by others in similar situations.

And the existence of a clearing house will help model developers. If the systems they have developed are approved for use in one or more situations, that information would be widely disseminated and could lead to more widespread use of the modeling system, and work for the system developer.

UARG is not asking EPA to lower the standards it imposes for what is an accepted model or modeling technique. Indeed, UARG does not want to see in the guideline models or modeling techniques that have not been peer reviewed and that have not passed tests to ensure that they are appropriate for use in regulatory situations.

What we do want is for regulators and the regulated community to have the tools available to do the air quality analyses that are demanded by the Clean Air Act. And we believe that to accomplish this, EPA must show itself to be more willing to review models developed outside the agency, and to act more expeditiously to allow the use of sound alternative modeling techniques.

DR. TIKVART: Thank you, Andrea. Any questions -- Ian, why don't you just go use -- okay, we'll help.

MR. SYKES: I am just sort of wondering that what you're saying like opens the possibility of allowing people to shop for models and do you have any suggestions on how we might avoid that happening?

MS. FIELDS: I'm not quite sure what I said that suggested we were talking of model shopping.

MR. SYKES: Well, you said you wanted to encourage the use of alternative models. And I know if anyone came to our region with an alternative model, we would give it a serious consideration. There have been cases where we've allowed them to use models other than those in the guideline, and I think the problem we have is we just open -- the one thing that you'll always get when this happens is are you letting them shop for models? And I think there is a problem if you make it too easy for people to use an alternative model, then they're going to run, say, three or four models and give us the one that gives them the best result. So I'm wondering if you have any suggestions on how we might prevent that happening?

MS. FIELDS: Well, first of all, I think if there's an Appendix A model that's appropriate for use, then people would use the Appendix A model, unless for some reason, a reason that they could convince you of, that model is not

appropriate.

MR. SYKES: That has happened.

MS. FIELDS: If it is truly the case that EPA Region 1 is that flexible, I think that's great. But I know from having sat around the table, talking with the members of our group, that not everyone has fallen into that situation, and indeed people have run into situations where they've sat down with agency representatives, worked out what they thought was the appropriate protocol for evaluating an alternative model, and have problem after problem after problem with getting the protocol finalized and then getting the results of the protocol evaluated.

When the new source reform subcommittee -- I think that's what it was originally called, eight years ago, got together, one of the suggestions was that EPA have a centralized system where it could list instances in which alternative models have been used in specific cases -- why they had been used, what had to be done in order to get approval to use it, and what the results had been. And there was consensus that that would be a good thing. It would help people. That hasn't been done. We'd like to see it done.

DR. TIKVART: Okay, one more question, and then we'll go on with Bob.

MR. STRACONGAS: I'm Arnie Stracongas with URS. I think this relates to the same page as your question. Judging by the comments the last couple days and they are no longer recommending an ozone or PM model, I was wondering if you had an opinion about the situation with now there being K-max, UAM, potentially Models-3, CALPUFF and what the situation holds for the utility industry and what their opinion is about now there being so many models, and how that adds confusion to you trying to get your permits?

MS. FIELDS: Well, I'm sure it's fair to say that there are as many opinions on what models ought to be used as there are utility companies who have to deal with this problem, probably more than there are utility companies. The concern is not so much that there is no model out there. It is obviously, the difficulty in reaching consensus on what is going to be used. The OTAG process was one that spent just about a year, I think, almost a year and a half, before everything was resolved -- I say everything -- before the modeling protocol ... was developed and was agreed upon. And we recognize that there are going to be difficulties in regional modeling and doing that in the future.

We'd like to see things as open as they were during parts of the OTAG process, where indeed, people were able to make suggestions and to have those suggestions listened to. During parts of the OTAG process it wasn't quite that open and we did have problems with that which I alluded to here and will go into more detail in our written comments.

DR. TIKVART: Okay, thank you, Andrea. Bob. Bob Paine also speaking for Utility Air Regulatory Group.

MR. PAINE: Well, good afternoon. My name is Robert Paine. I'm a senior air quality scientist at ENSR Corporation in Acton, Massachusetts, but on behalf of the Utility Air Regulatory Group, or UARG, I'm commenting on the proposed use of CALPUFF for assessing impacts of air emission sources on PSD Class I areas. My comments include aspects of the implementation of CALPUFF in the context of the Phase II recommendations of the Interagency Workgroup on Air Quality Models, or IWAQM, that are cited by the proposed changes to the modeling guideline.

It is important to note that CALPUFF is the first dispersion model

formally proposed by the United States Environmental Protection Agency as a guideline model for assessing long range transport impacts, that is, covering distances of over 50 kilometers. At the same time, IWAQM and the Federal Land Managers, FLMs, have been working on a consistent approach to assessing impacts relating to air quality related values -- or another acronym, AQRVs, such as visibility and acidic deposition in Federal PSD Class I areas.

The use of CALPUFF to determine impacts of proposed new emission sources on PSD increments and AQRVs is likely to be an increasingly important and resource-intensive aspect of permit applications. It is also evident that the role of the Federal Land Managers in new source permit reviews has been increasing over the past several years. Therefore, in its comments, UARG is addressing, as a whole, the sue of an advanced model such as CALPUFF in conjunction with the IWAQM Phase II recommendations and PSD increment and AQRV significance thresholds.

I'm going to address three issues in my comments today. They will be, first of all, what are the modeling procedures to be followed in addressing impacts in PSD Class I areas, and are there impediments for applicants inherent in these procedures?

The second bullet, basically, is CALPUFF suitable for model applications for which it is proposed, and what are the limitations in using this model?

And the third item will be, in light of the IWAQM and FLM proposals for addressing impacts in PSD Class I areas, what are the appropriate ways to implement CALPUFF for long range transport applications?

Let's start with issue number one, modeling procedures for PSD

Class I areas. For most of the modeling situations discussed in the EPA modeling guidelines where a refined modeling technique is recommended, a screening-level approach analysis is also provided. The screening analysis is meant to be easy to conduct and to provide a realistic yet conservative estimate of the maximum impact. If the results of the screening analysis show compliance with the regulatory criteria, then no further modeling for compliance with standards and increments beyond the screening analysis is required.

The IWAQM's suggestion for CALPUFF screening analyses involve the use of five years of ISCST3 input meteorology. This approach is reasonably easy to implement, although IWAQM also proposes the use of a ring -- or in this case, three rings -- of receptors surrounding the proposed source at the distance of the PSD Class I area. Now in this case, here's a case of a source in a PSD Class I area and it's about 200 kilometers away. This scale, every tick mark is 50 kilometers, and basically the model all the way around this ring -- these three rings according to the proposed procedures.

So IWAQM proposes that the maximum concentration anywhere on the receptor rings, even if 180 degrees opposite to the direction of the PSD Class I area, should be used, rather than restricting the analysis to receptors only located in the PSD Class I area of interest. As I will explain later, this requirement for modeling with a full circle of receptors with screening meteorological input data to CALPUFF is likely to be overly conservative, especially if the prevailing winds do not direct the plume toward the PSD Class I area most of the time.

For example, if the winds are channeled towards the northeast or southwest, and hardly ever blow over there, we're going to treat this area here as if it were a PSD Class I area. I think that's outside the jurisdiction of the Federal Land

Managers.

The undue conservatism of the proposed screening approach is especially critical in the light of the extremely stringent thresholds for a project to show an insignificant air quality impact. For example, the thresholds for an insignificant PSD impact are only about four percent of the applicable PSD increments for Class I areas. Experience has shown that it is quite difficult to achieve an insignificant modeled impact even for a very well controlled source that is on the order of 100 kilometers away from a PSD Class I area, especially for 24 hour SO_2 and PM-10 thresholds.

The significant threshold for visibility impacts is also extremely stringent. A given project has to demonstrate an impact of less than five percent of the background extinction coefficient, when a value of ten percent is considered by the FLMs is considered the lower limit for perceptible visibility changes.

If these proposed conservative procedures for determining a significant air quality impact are incorporated into the modeling guideline, there will likely be an increased need to conduct a refined modeling analysis with full CALMET processing because the results of the screening method will be too conservative. UARG acknowledges that the use of full CALMET processing is preferable to the use of ISCST3 meteorological data, but the effort required to conduct a refined analysis, as you have heard by now, is substantial and very complex.

IWAQM proposes to implement a regional strategy we heard five years ago, whereby meteorological data required for running a refined CALMET/CALPUFF model will be provided to permit applicants to promote consistency. And conceptually, UARG agrees with this approach, but is not aware

of the existence of such data bases. In addition, most permit applicants are aware of the incomplete status of PSD emissions inventories from a number of states that would be required for a full increment analysis. Until reasonably accurate and complete meteorological and emissions inventories are made available by reviewing agencies, the use of CALPUFF as a screening tool should be redesigned to eliminate excessive conservatism. With the eventual and more widespread use of full CALMET/CALPUFF, there are specific modeling issues that I would like to comment on at this time.

I'm going to go to issue number two, but not a new slide yet, but soon.

The use of very stringent thresholds for showing insignificant air quality impacts in PSD Class I areas and for showing acceptable visibility impacts has the results that any arbitrary conservatism in the full CALMET/CALPUFF model is very burdensome to the regulated community. Since IWAQM and the FLMs, and I guess there's a FLAG procedure or group -- since they are imposing such tight restrictions on modeling thresholds for a realistic assessment, it is necessary for the regulated community to very closely scrutinize, to perhaps a greater degree than ever before, the way that CALPUFF is to be sued.

Generating the wind fields with CALMET using multiple sites for surface and upper air data introduces a number of technical challenges, which I'm not going to go into, but I will sort of flash up on the screen basically details of switches in the CALMET input. And most of these choices have default selections, site-specific considerations are often necessary, and there is relatively little guidance available on the considerations that need to be taken into account in making these selections.

Technical issues concerning the CALPUFF dispersion model user are also numerous. Although there are default values for most parameters, the user has the discretion of site-specific choices and the array of choices is potentially so large that between CALMET and CALPUFF, the range of possible modeling solutions is large as well. A significant investment in sensitivity runs may be needed for some applications before the user commits to a given combination of technical options.

A number of evaluations of CALPUFF have been completed by the USEPA and the following summary information form the Sixth EPA Modeling Conference is provided in Appendix D of the recently-released IWAQM Phase II Summary Report. It indicates distance and time travel limitations regarding CALPUFF applications. And I flash up here -- I'll read it --

"The IWAQM concludes that CALPUFF can be recommended as providing unbiased estimates of concentration impacts for transport distances of order 200 kilometers or less, and for transport time of order 12 hours or less. For larger transport time and distances, our experience thus far is that CALPUFF tends to underestimate the horizontal extent of the dispersion and hence tends to overestimate the surface-level concentration maxima. This does not preclude the use of CALPUFF for transport beyond 300 kilometers, but it does suggest that results in such instances be used cautiously and with some understanding."

As I said, they set the following information five years ago, but it's worth repeating.

One aspect of mesoscale dispersion modeling that could help to explain the potential CALPUFF overpredictions involves vertical wind shear and its effects upon pollutant dispersion. In a paper delivered at the Eighth Joint conference on applications of air pollution meteorology in 1994, Moran and Pielke

suggested, or discussed the importance of wind shear effects on enhancing, or even dominating, the horizontal dispersion during long range transport. These authors showed with a numerical particle model -- and show various time sequences going from left to right -- that vertical shear of the horizontal flow can result in pollutants at different levels being advected at different speeds or in different directions. This situation is most likely to occur during the night time hours when the vertical mixing in the atmosphere is often suppressed by stable thermal stratification.

After the shape of a pollutant cloud, as we initialize it with a vertical and very narrow horizontal extent -- after the shape of that cloud becomes distorted by wind shear effects, and then it gets mixed during the morning by convective activity, subsequent or delayed vertical mixing will greatly enhance the horizontal spread of the cloud when it is mixed to the ground versus what it would be if it were not sheared.

Moran and Pielke conclude that the neglect of wind shear by mesoscale atmospheric dispersion models can result in significant errors in the prediction of tracer cloud size, shape, centroid location, and surface footprint if the cloud has experienced a sequence of at lest two stability regimes. Note in the figure that CALPUFF might tend to assume a plume spread as depicted in the left panels when the actual plume spread, after the morning inversion breakup may more likely represent the right panels. Since CALPUFF may not have the capability to fully characterize the vertical shear effects, it is subject to the effect of underestimating the plume footprint and overestimating the concentration.

However, as we have heard, CALPUFF does have a puff splitting algorithm with is designed to respond to vertical wind shears across a puff. In effect, and I don't know if I have up all the latest details and the latest puff splitting

algorithms, but the algorithm causes puffs to be subdivided -- and this is from the CALPUFF user's guide that was on the web site -- the algorithm causes puffs to be subdivided and I'll call them daughter puffs to be sent in different trajectories. leading to increased effective rate of puff dispersion and lower ground-level concentrations, as we would have wanted to see, judging from the last slide.

Now recently, John Irwin told me that the puff splitting algorithm should alleviate -- at least partially alleviate the concerns noted by Moran and Pielke, but UARG is still concerned about the complex interactions between various parts of the CALPUFF modeling system and their cumulative effects upon this -- I would call it -- critically important algorithm. For example, the way in which surface and upper level winds are weighted, extrapolated, or otherwise manipulated within CALMET could have a significant bearing on how effective the puff splitting algorithm works.

In principle, the algorithm should split the puffs into three parts -- or how many parts it is now -- send their puffs in a number of different directions and effectively increase the plume spread. But the complex interactions between the construction of the wind field, the turbulence, and the dispersion model, may be such that some seemingly unrelated processing decisions in CALMET, such as extrapolation of surface winds aloft if you artificially reduce the wind shear in CALMET, you could significantly affect the dispersion results in CALPUFF, as they pertain to how effectively this puff splitting algorithm could work.

UARG is not aware of a convenient mechanism to track how many puff splits occur, and the IWAQM Phase II document does suggest that CALPUFF results are relatively insensitive to the selection of dispersion options. In my personal use of CALPUFF, I also found that turning on and off the puff splitting

didn't seem to make too much of a difference for a 200 kilometer case, using refined full CALPUFF. In general, the insensitivity of the puff splitting exercise to this turning it on and off and making a difference in concentrations is cause for concern, because that option has the potential to be a partial solution for the distance and time limitations that were mentioned about due to delayed shear enhancement.

Okay, I want to go to the recommendations of UARG for implementing CALPUFF. First, get rid of the receptor rings. It is unnecessarily conservative to expand the Class I area to be protected to an entire ring or rings of receptors at the same distance as the source is from the PSD Class I area. This proposal arbitrarily expands the jurisdiction of the Federal Land Managers to areas not under their control.

From a technical point of view, it is quite possible that, in fact, a high frequency of winds could often direct the plume from the proposed source toward an area not under the jurisdiction of the Federal Land Managers. Furthermore, the reasons put forth in the IWAQM Phase II document for using the ring of receptors are not well founded.

This is an example of a table -- you probably can't see all the details, but it has ratios of the concentration of the screening versus refined and there is a concern that there are some numbers less than one. Therefore, the solution is to require the rings of receptors. However, if you look at this closely, you'll find that most of the numbers are for averaging periods and distance ranges that are not relevant for long range transport of pollutants of interest.

For example, there are no one hour concentrations regulated for PSD Class I areas for the criteria pollutants. Why be worried about these rows here? We aren't worried about distances beyond 50 kilometers, most of the other rows are

eliminated from consideration. When you get down to it, there's hardly any numbers in the rows that count, below one. So -- as for SO_2, the critical concentration averaging time seems to be 24 hours anyway.

Therefore, UARG contends that the IWAQM recommendations to use a ring of receptors just for CALPUFF screening calculations is without foundation and it arbitrarily extends a jurisdiction to the Federal Land Managers to areas not under their control, so basically, get rid of the rings.

Limit the distance and travel time applicability of CALPUFF. At least for now. The ability of CALPUFF to adequately simulate conditions that tend to effectively disperse plumes over large travel times and distances is limited. Influences such as delayed shear enhancement and multiple terrain features above plume height can rapidly disperse the plume beyond the ability of CALPUFF to effectively simulate these features. The effectiveness of the puff splitting algorithm has not been clearly demonstrated for overcoming these effects.

Therefore, UARG contents that CALPUFF should not be used for simulating long range transport beyond travel distances of 200 kilometers or travel times of 12 hours, especially if multiple encounters of complex terrain features are involved. For such large travel distances or transport times, or those complex terrain features, the plumes of interest will likely be very effectively dispersed, thereby being of little threat to PSD Class I areas.

Develop regional meteorological data bases and emissions inventories. UARG recommends that the Federal Land Managers develop regional meteorological and emissions data bases and make these available to all permit applicants on a consistent basis. In fairness, the development of these procedures should not be the sole burden of the first permit applicant to come along. In the

interim, while the Federal Land Managers are developing such regional data bases, the sue of screening procedures for the full PSD inventory impacts should be allowed, and currently they are not.

Implement refinements to visibility impact calculations. There are several aspects of the regional haze calculations as recommended in the IWAQM Phase II document that are overly conservative. UARG proposes that three changes to the screening procedure be implemented before the full use of the CALMET/CALPUFF option should be required, as noted below. These changes would apply to the application of the full CALMET/CALPUFF procedures as well.

Significance threshold for regional haze impacts. A technical paper coauthored by senior National Park Service and National Oceanic and Atmospheric Administration scientists, Pitchford and Malm, 1994, states that a change of one to two deciviews -- and that's about ten to 20 percent of the background extinction coefficient -- corresponds to a "small, visibly perceptible change in scene appearance", and that smaller changes are imperceptible. Therefore, UARG considers the use of a five percent threshold as excessively conservative, especially in light of how conservatively the background extinction is estimated and the fact that the highest point concentration -- I can't emphasize this enough -- the highest point concentration is used, rather than the regional average prediction that would be more appropriate for a line of sight, regional assessment. A revised value of ten percent threshold for significance, and 20 percent for the perceptibility limit for all PSD sources should be adopted instead if we're going to keep using those peak modeled point concentrations. Otherwise, if you want to keep the five percent and ten percent, allow regional or line-of-sight path averaged concentration calculations should be adopted.

Rather than using the background extinction that is consistent with transport conditions, IWAQM suggests that the use of the best ten percent background visual range irrespective of meteorological conditions for determining this five percent extinction threshold should be used. By definition, since it's the best ten percent, such good visibility conditions occur infrequently, and they in fact may never occur for the wind flow that is required to transport the plume from the proposed source to the PSD Class I areas of interest. However, these practical issues are not considered by IWAQM in their selection of the background visual range, which UARG considers to be a screening value, subject to refinements as discussed below.

We're going to do relative humidity extinction adjustment. That adjustment of the CALPUFF particulate concentrations that is used to compute the plume-related extinction results in an overestimation of impact. The secondary particulate concentrations estimated by CALPUFF are adjusted according to an arbitrarily high 90 percent relative humidity in the screening calculation, and by hourly relative humidity values in the refined calculation.

For high relative humidities, a difference in relative humidity of as little as ten percent can result in a factor of 50 percent or more in the modeled extinction. Even the use of hourly values of relative humidity introduces significant overprediction biases for the purposes of visibility impairment because periods of very high relative humidity often correspond to fog or rain when visibility should not be a legitimate AQRV -- you can't see anything.

In addition, the relative humidity increases near the surface during the night, when the visibility also is fairly poor, I would have to say, when the air in contact with the earth cools is an issue. So in addition -- this was mentioned earlier

today -- relative humidity values aloft, where the buoyant plumes are, are generally lower than near the surface, but lack of measurements aloft or any corrections of the surface-based measurements means that highly conserved surface values are implied for plume heights.

All of these several biases, added up, result in the degree of extinction due to droplet growth being significantly overestimated by the current recommended modeling procedures. UARG contends that to determine the effective relative humidity to determine extinction, it is appropriate to exclude the period from dusk to dawn, and for periods of precipitation or fog. Such refinements should be made, should be incorporated into the CALPUFF and CALPOST modeling system as soon as possible, prior to any use in determining regional haze impacts.

And going on to selection of background/visual range. The currently recommended so-called clean 90 percent best background visual range most likely represents air masses that invade an area with westerly or northwesterly winds following a cold frontal passage. The meteorological conditions required for impacts on the PSD Class I areas from specific sources are often not consistent with these wind directions and dispersion conditions. In those cases, the use of the typical conditions associated with the 90 percent clean days is counter to EPA guidance in Section 8.2.2 of the proposed guideline which states "the meteorological conditions of concern accompanying the concentrations of concern should be identified for background". That is, the background visual range should be concurrent with the model predictions.

As a refinement, UARG recommends that the use of representative, concurrent IMPROVE visibility measurement, analogous to the short-term

concurrent background ambient concentrations be used in routine permitting activities. The use of the IMPROVE data has not been mentioned in the FLM guidance probably because such data has only recently been made available. The IWAQM guidance and the CALPOST code should be updated to take advantage of the very data relied upon by Federal Land Managers in characterizing the existing visual conditions in protected areas. UARG recommends that the meteorological data and IMPROVE visibility measurements for concurrent periods be used in regional haze modeling.

And that concludes my remarks.

DR. TIKVART: Okay, Bob, thank you very much. Given the lateness, I think we probably should move on to the next presenter, and that's Maidhila Shararan, who has some personal remarks, representing himself.

MR. SHARARAN: Good afternoon everybody. I'm just going to talk to a different issue. I'm just going to give some ... conditions, how to keep them ... taken into account in the CAL model. I am from ... India, and I've just visiting currently at the John Hopkins University.

As far as the ... conditions are concerned, they are presently around the world, particularly in the tropics. They are associated with ... The ... study from the ... in India which took place under highly low and ... conditions. Hourly ... are likely ... through the lack of well defined center line. Conservation field normally has a large plume spread and multiple peaks. ... distribution non-Gaussian. Plume ... in the ... conditions. At distant ... and then it becomes low. What we get out of it, hourly average concentrations from the ... models gives ... peak values, and underproduction for plume spread.

In addition to that, the ... now identified in ... low conditions. ...

especially in the sterile conditions. Next slide.

... in the dispersion model ... primarily ... and the second application ... parameters ...

This is the experiment we conducted ... in the ... conditions in 1991, ... and we have the circular arc ... 50 meter, 100 meter arc ... experiment and you can just see that we have ... are very large, and in addition to that the ... are also ... on the circular arc.

Now we just see that the conditions ... wind ... Next slide.

... if you understand the wind direction, you can see during the night ... wind ... 15 meter ... you can see in the wind direction that you get very ... in the wind direction too.

And now we have our model predictions -- just ... one. You can see in most of the cases they are within a factor of two from the model.

This is the other experiment. This is the ... 1974. Here we have our circular arc, ... and the samples were ...

You can see that -- I'd like to emphasize here -- you see this solid line ... concentration ... compared to 360 degree, and you can see if you're going to use the hourly ... wind conditions, ... classical ... what you will get, just the ... concentration ... big, really high predictor, and the plume is ... less. And if you just see on the bottom line -- you can see ... this one, we are able to predict ... along the complete arc ... two degree ...

The summary ... this is from run number five, you can see ... simple lines ... Gaussian model ... overpredicting the peak and underpredicting the ... spread and ... shows up ...

Now if you go ... model ... as you can see this is again ... hour ..., and

if you can notice here in the ... quadrant, the plume ... deflected to the first quadrant. What that implies, the vertical ... that you can add to the ... concentration of the 100 meter arc, and there's low concentration on the ... meter arc and the 400 meter arc.

Now if you go with the ... model like they do, next slide please, what they will do ... concentration of the ... meter arc and the 400 meter arc. You can see it -- this is the ... solid line ... concentration in the ... quadrant, and ... concentration in the 200 meter arc and the 400 meter arc. ... compared ... same to other models. Next slide.

Just to summarize ... dispersion ... conditions ... most of them have been published over the last five years ... Thank you very much.

DR. TIKVART: Okay, thank you. Next Stanley Vasa from Southern Company Services wanted to make a few remarks. Yes, it would be good. Could we have copies of your slides?

MR. VASA: This is a low key, short presentation, and I'll take only a few minutes. As Dick Schulze mentioned this morning, as part of his presentation, the implications are very important in modeling, and American industry could run into billions of dollars. A statement, our deliberations here in the last two days here are not trivial as far as American industry is concerned.

John, on his presentation on behalf of Bruce ... had mentioned AERMOD and the inclusion of PRIME into AERMOD. And if you noticed, he was even joking about repeatedly the same thing said in each slide, to insist on the point.

The reason I am speaking today is to express our concern to EPA at large, and also the people that have attended today's meeting. My concern is to bring this point to the attention of EPA, and also it's in the form of a comment too.

My name is Stanley Vasa. I am a senior research specialist for

Southern Company, based in Birmingham, Alabama. Southern Company, has a service company called Southern Company Services, which is a subsidiary of Southern Company and provides engineering, research and other services to its sister companies, namely, Alabama Power, Georgia Power, Gulf Power, Mississippi Power, Savannah Power and Southern Energy Incorporated. Southern Company is an international energy company that operates more than 50,000 megawatts of electricity worldwide. It is the largest producer of electricity in the United States, and one of the world's largest independent power producers.

Southern Company's generating facilities are located in simple and complex terrains, and some of their stacks are subjected to the building downwash phenomenon. For this reason, it is imperative that scientifically sound and as-realistic-as-possible regulatory air quality models are available to apply to our facilities.

The conclusions and recommendations of the First Peer Review Report of the AERMOD model state that "The development of AERMOD by the AERMIC committee has proceeded over the past several years with minimal funding. The scientists who composed the primary boundary layer and dispersion algorithms have done so on nearly a volunteer basis. The budget for evaluations and documentations was relatively small." The peer review panel found, as we all have heard in the last two days, that AERMOD appeared to represent significant scientific advancements over ISC3, with various inadequacies that could be fixed through some additional work if funding was available.

The original development of the CALPUFF model was sponsored by California Air Resources Board. Later enhancements were incorporated into this model as a part of the work for IWAQM, US Forest Service, Environmental

Protection Authority of Australia -- Victoria, Australia, private industry in the United States and Australia and the USEPA. Thus, the current state of the CALPUFF model is a result of many contributors that funded improvements based on individual needs.

The ISC-PRIME model was developed under the management of EPRI and funded primarily by a few electric utility industries, including my company, that felt a need for a better downwash algorithm to address the industry's anticipated installation of combustion turbines with relatively short stacks. In reality, however, all industries that have existing and anticipated stacks less than GEP will be the benefactors of this model.

As we know, AERMOD, CALPUFF, and ISC-PRIME have reached a stage of recognition because of their proven superior science. In the next few months, EPA will receive helpful comments that may call for either improvements to some portions of these models, as we have heard so far, or providing additional information in the form of documentation, testing, et cetera.

At this juncture, it only prudent that EPA comes up with the needed funds to complete the process. EPA has the responsibility of providing not only the guidance needed to use appropriate tools, but also to provide those tools to the user community. In all practicality, this may be equal to providing salaries for a few people on EPA's payroll for just one year.

The time has come for EPA to take this responsibility seriously, pick up the tab for it, and complete the task without any further delay.

Southern Company intends to comment on the specific technical aspects on which the comments have been welcomed by EPA in the next few weeks, on the models that we have discussed in the last few days. These will be provided in

the form of written documentation. Thank you very much.

DR. TIKVART: Thank you, Stanley. We have one last individual who wanted to make some comments, and that's Dick Schulze.

MR. SCHULZE: I just have two quick comments for you all -- I suppose, more for the record. The first one. Sometimes I've seen firms build structures on their property or neighbors build structures that create or acerbate a downwash situation. And I don't know if it's within the purview of this group, but it seems to me that there would be benefits, both to industry and to EPA that would require the review of the construction of new buildings. These are not emitting structures -- it could be a warehouse which would cause downwash.

Perhaps a simple statement entitled permits saying that the agency reserves the right to review the construction of nearby buildings in excess of 40 percent of the shortest stack height, would be sufficient to achieve this. I've seen this happen several times and the issue of course is that they build a new warehouse or an office building, it's real hard to overcome the effects of downwash that are created by this new structure.

And the second one, I just want to underscore a comment made by Andrea Bear Field, and that is I think it would be beneficial for all of us if EPA tracked the use of models in permits, perhaps in the same way we track the determinations on ... layer -- just put it in a clearing house and say this particular applicant did do modeling, they used this model. That way, EPA would have some information on the utilization of models by various types of applicants. And I think this would be very useful to the modeling community to have that information.

That concludes my comments.

DR. TIKVART: Okay, thank you, Dick. I -- yes, Tom?

MR. COULTER: This is a quick question and comment. If we can rewind the tape back to AB-3, earlier, we didn't have any comments on that, so, in that we heard Gale Hoffnagle mention something about some errors and enhancement requests for the CALMET/CALPUFF users guides. And as sort of being one of the few people in the room that actually has read every single page of the CALMET and the CALPUFF users manuals, I compiled all the errors I could find and passed those along with an enhanced request answer to Earth Tech, and I think they incorporated those in the version they uploaded to their website. So I guess my question to AB-3, is could you provide a detailed specific list of errors? I'd like to see that and I'd also like to know what they had in mind for the enhancements and improvements. That would be nice to see.

Comment is also back to Eldewins mentioned about some suggestions for Section 8, I think, in Appendix W for what I -- I think I understood him to say is some matters of quality assurance and siting considerations to be placed in there, and I guess I would say back to that that I think we have relegated those off to the site specific met program guidance document. I'm going to look more closely at his comments, but I think we've put that material out there -- that's where we keep it. We've tried to make linkages, where appropriate, back out to that other document. So if we need some additional linkages put in, that's what I think we should perhaps do, but we've tried to keep those kind of details, and that kind of information not in Appendix W, but in that site-specific monitoring guidance. So I wanted to kind of help clarify that or mention that.

DR. TIKVART: Thanks, Tom. Does anybody else wish to make comments, or do they have a burning question that they haven't had a chance to provide? If you would go to the mike, and if we have to start moving the mike

around, we'll do that. Name and affiliation, please.

PARTICIPANT: Jules ... from Science Consultant, Toronto Canada. It looks like to me that we are underlying all the time that we should put PRIME into AERMOD, and nobody's mentioning that if you want a complete model you should also put the position in that system and have a complete system that we can use it, not just add the PRIME and then what again are we going to do with position? Use ISC-PRIME or something else? So I think if we are updating this model, it should be finished and be a complete system.

DR. TIKVART: Okay. Thank you. Yes, I would assume that that's the intent, that if AERMOD-PRIME comes to pass, ISC-PRIME is dead. And I don't know if that's what you meant to say, but that is a consequence. If PRIME goes in to AERMOD, there really wouldn't be a need for ISC any more. Go ahead.

MR. STRACONGAS: This is Arnie Stracongas with URS. My comment relates to consequence analysis of what we're talking about here over the last couple days. We're talking about implementing AERMOD -- or that's the proposal -- and my comment is that I'm not sure of the full consequence of this is really known. We talked about it from an individual source standpoint, but I would remark that this very limited situation where limited sources themselves face certain problems with, their own individual sources. Usually it is the multiple source aspect of things that drives an analysis, whether it be increment or Max. All of the studies we've talked about are usually on individual source that added features into AERMOD which include individual selection of urban versus rural mode, but no talk of the effect of that in a multiple source environment.

My suggestion would be if individual states would take on the task where they are in locations where they think they are near nonattainment, what is

the consequences of them switching to AERMOD versus staying with ISC right now? That could be a multi-million dollar affect that is not known. So if you think of where we are now, grandfathering the analyses as they are now, reopening the analysis with a different model, we could be talking about a complete oranges versus apples result of that. I've heard nothing to talk to that effect in the last couple days.

The last thing I can think of at the moment is -- there still is no inclusion in ISC or AERMOD of any blant (ph) line source situation which does come up, and Joe talks about that in CALPUFF. I'm sorry, I'm trembling, I'm a nervous guy. So I still that as special source type that has been overlooked in the models for a long time and we have no regulatory model now, it looks like, that can handle that situation and so there's creative requirements to deal with that. So I would, I guess, pose that there's still should be some way of handling the blant (ph) line source situation.

DR. TIKVART: Doesn't BLP do that? Is there some --

MR. STRACONGAS: That is true, but then you're talking about a model that is probably equated with ISC-like, so yes, it's true it does that, but now we're talking also about the AERMOD situation, ISC-PRIME, downwash, et cetera. So there's different models that do a different aspect, but then there's weaknesses of those within that.

DR. TIKVART: Okay, other questions or comments? Anybody else have a burning point that they didn't have the opportunity to make?

PARTICIPANT: This is ... from Bechtel Power. I have one question and one suggestion. My suggestion is the current screen-3 approach, I don't use it any more, but you can print out cavity height and length. I really would like to see the future PRIME or even the AERMOD would print out cavity height

and length, I mean the rational dependent, and I believe that piece of information is very useful because nowadays our projects hardly see any ... height any more. In our studies, I would say, during the past three years, everyone is ... height, so know not just for air quality purposes, also for stack height determination and sometimes I can move the stack a little bit to my own benefit.

And my question is regarding CALPUFF. Can that model really do fogging, icing for cooling tower? That's my question, I'm not saying it can't.

DR. TIKVART: Joe, do you want to handle that? Does CALPUFF really do fogging and icing --

PARTICIPANT: Because the plume rise is different for a moisture plume and the dry plume from the stack.

DR. TIKVART: Yes.

MR. SCIRE: It does do fogging and icing, together with postprocessors, so the special information it needs to make those calculations are included in the model.

PARTICIPANT: So is the relative humidity already considered?

MR. SCIRE: What the model does is, it has an event process to calculate emission rates for different operating scenarios -- type of towers, dry or wet towers -- wet towers primarily, and then emissions relative to the source -- hourly varying source input file, the CALPUFF model will use -- will computer the water vapor effects, including the effects of plume temperature on the humidity/saturation mixing ratios, effects of downwash on that, and also the effects of multiple sources contributing potentially to a line of cooling towers.

PARTICIPANT: But can it take care of the plume abatement? Because currently I'm using CEPTI (ph) to do cooling tower impact studies.

MR. SCIRE: It'll do both. It'll do a abated towers as well.

PARTICIPANT: Okay. Because ... is not able to do that.

MR. SCIRE: This model can. That's why it was developed.

PARTICIPANT: Just for your general information. Two months ago I attended NCR's panel meeting regarding control room habitability analysis, that's more of the air quality side of the nuclear -- for nuclear plants, and in the panel people are agreeing, including having NRC's approval to use three years of met data. I don't know if it's good or bad, but it's just another reference.

DR. TIKVART: Okay, thank you. Anyone else? Okay, looks like we've run out. I'll remind you that the public comment period is open until August 21st and I would like to thank you for your attention and bearing with us here and we'll close the Seventh Modeling Conference.

(Whereupon, at 4:40 p.m., the meeting in the above captioned matter was adjourned.)