U.S. ENVIRONMENTAL PROTECTION AGENCY

10TH CONFERENCE OF AIR QUALITY MODELS

DAY THREE

U.S. ENVIRONMENTAL PROTECTION AGENCY

109 W. ALEXANDER DRIVE

RESEARCH TRIANGLE PARK, NORTH CAROLINA

MARCH 15, 2012

8:30 A.M.

GUEST SPEAKERS:

Mark Bennett, CH2M HILL
Andy Berger, Tri-State Generation and Transmission
George Bridgers, US EPA - OAQPS
Pietro Catizone, TRC Environmental Corp.
Tony Colombari, Trinity Consultants
Allen Dittenhoefer, Environplan Consulting
Nicole Downey
Tyler Fox, US EPA - OAQPS
Ryan Gesser, Georgia-Pacific, LLC
Steve Gossett, Eastman Chemical Co.
Sergio Guerra, Wenck Associates
Steven Hanna, Hanna Consultants
Qiguo Jing, Trinity Consultants
Ashley Jones, Trinity Consultants
Eladio Knipping, Electrical Power Research Institute
Cindy Langworthy, Hunton & Williams, LLP
Mike Lebeis, DTE Energy
David Long, American Electric Power
Stephen Mueller, Tennessee Valley Authority
Robert (Bob) Paine, AECOM
George Schewe, Trinity Consultants
Lloyd Schulman, Exponent
Joe Scire, Exponent
Justin Walters, Southern Company

GUEST SPEAKERS:

Richard (Chet) Wayland, US EPA - OAQPS

Dana Wood, BP America Production Co.

Anand Yegnan, ERM

U.S. ENVIRONMENTAL PROTECTION AGENCY

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MARCH 15, 2012

MR. GEORGE BRIDGERS: Well, welcome back, everybody. Hopefully everybody got a little bit of rest. I know I did last night. I think the amount of rest that I got last night was -- let's see, the magnitude of rest was the same level of decrease in the, or the increase in stringency of the NO2 standards, so tenfold increase of sleep, so it was great. Today is -- we're going to change gears a little bit. Today is where we get to listen to everyone that has requested to come and speak. This is a public presentation session. The format is a little bit different because we will not offer or entertain questions and answers so, hopefully, we can make it through the thirtyish presentations somewhat on time. We will have, I don't want to call it open-mic but we will have, after all of the presented presentation -- or the requested presentations, we will have time for others in the audience that, if they have prepared comments, they can come up to the microphone and offer those. But I also interject that at any point that, you know, comments can be submitted to the docket. They don't actually have to be verbally read today.

Just to go through a little bit of logistics. I did this yesterday morning without the aid of the slide. I will do it this morning because we have a few new people in here today. Just a reminder that this is a public hearing, as I said. Everything that's presented and said is part of the official docket. Things are transcribed so be careful what you say if you don't want it repeated years from now. We also ask that as you come to the microphone, when you're offering your public comment or during the presentations, the presenters to announce themselves. So, for the record, this is George Bridgers from the U.S. EPA.

We're scheduled -- we're packed today. We've had a couple of changes in the schedule that we'll work with as we go along because of some -- someone could not attend today because of a family emergency, but they will be submitting their comments to the docket. Let's see, I've already said there's no Q&A and the other thing, this was mentioned yesterday during the afternoon sessions, that we will be extending the public comment period for the Conference through April 30th. We won't do that through a Federal Register printing. What we'll do is we will have a memo that's signed and then add it to the docket. So that's how it becomes official, but I also encourage everyone that after the Conference that just because April 30th has come and passed doesn't mean that we can't continue to engage. I mean, that's part of what we're trying to do moving forward is to have this collaborative effort as a community as we look for - forward to the different developments and changes that may need to happen between now and the 11th Conference which, if everything goes beautifully, would be - what, in October, 2014. And that's all I have to say there. Let me back up a little bit because I think Chet wanted to come say a few comments and then after that, we'll launch right into the public presentations.

MR. CHET WAYLAND: Great. Thank you, George. Unfortunately, this afternoon, I have to run out for some other meetings and so I won't be here when things wrap up at closing, so I wanted to take this opportunity in the morning to thank everybody again for coming. I've been able to sit in for most of the last two days and I'm going to sit in for as much of today as I can, and it's really been phenomenal to hear all of the issues that have been raised, the comments that have come in, suggestions, presentations. It's been great, and I really appreciate everybody taking the time to come. I heard a couple of things yesterday that I just wanted to kind of, you know, elaborate on and that's, you know, people are all coming up with specific issues they'd like us to address. I think Tyler was correct in saying we do have limited resources and, you know, we have limited budgets and different things like that, but if the community as a whole through their comment process that comes in from this conference can help us to prioritize which items are the ones that are the most important to you, then I promise you we will take that prioritization and work on it in that order. So, if folks can kind of, you know, somehow gather around and try to come look at it like this is the most important issue and this is second and this is third, then that will help us a lot to say, okay, this is where we do need to put our resources here at EPA and work on those specific issues. And we're happy to do that and I promise you we will do that, if we can get some kind of a semi-consensus of, you know, what are the highest priority issues to be looking at.

The other thing is the field study issue that came up. I think that's a great idea. Unfortunately, EPA does not have a lot of resources to go ahead and do big field studies like we maybe had ten, twenty years ago, but if there are others who want to pull together and do field studies and work with us or work together, I think that's great. The more data you have, the better tools that we will always have. So, I would encourage folks that are interested in that to maybe collaborate with each other and, you know, come up with some proposals and some ideas and some protocols, and I think we'd be very supportive of additional field studies for any of these tools.

So, and just to reiterate, I know some folks made notice yesterday but for a little while in the afternoon, Janet McCabe, our Deputy Assistant Administrator for OAR, joined us. She was here for several other meetings and an SAB Panel in the morning here and she just wanted to slide in. She had seen the agenda and was very interested, and she stayed for about a half hour or so and heard a lot of the Q&A on NO2 and SO2 and then had some several -- several meetings with her afterwards. She's very interested in this. She wanted me to let everybody know that she and Gina McCarthy, our AA for OAR, are both very interested and aware of the NO2 and the SO2 and the PM2.5 issues. They were thrilled that we were having this and that this many people were coming. She got a lot out of the short amount of time she was here yesterday, and I just wanted to let folks know that she is generally watching this and making sure that we are addressing these issues that you guys are raising. So, with that, I hope you guys have a good day today. I'm looking forward to the, you know, presentations and again, thank you all very much for coming. If today is like the last two days, it's going to be great, and I really appreciate all of your attendance. So, thank you.

MR. GEORGE BRIDGERS: Thank you, Chet. Well, we are going to actually transition right into the public presentations so we can, hopefully, get a little bit ahead of schedule. So, the first handful of sessions are from the AWMA AB-3 Committee.

MR. PIETRO CATIZONE: Good morning. Home stretch is here, I guess. I'm Pete Catizone. I'm the Vice Chair of the Air Waste Management AB-3 Meteorology Committee and first of all, I'd like to thank EPA for inviting AB-3 to be part of the 10th Modeling Conference and also inviting us to be part of the Attendee committee that has been conferenced in the last couple of months, to put together and help out with the agenda. We're happy to do that. As you might have seen from the references to AB-3 in the last couple of days, AB-3 has been involved all along. I know that we've been in most of the EPA conferences, but we've also had specialty conferences, which I'll talk about a little bit, typically the year after the Modeling Conference to further focus and iron out some of the modeling issues that face the group in the industry. The AB-3 Technical Coordinating Committee is part of the Tech Council of AWMA and we are very active. We have more than 200 members currently in our committee. To be on the committee is not that difficult. You just need to attend one of the meetings or ask to be part of the meeting and/or be a member of the AWMA. We have a wide, broad representation on our committee. We have government folks that participate, industry folks and, of course, consultants are always there.

The basic objectives of AB-3 is actually very simple. We want to promote the best science we possibly can and apply it as best as we can to the issues and concerns that face us all. So, to that extent, we try to provide technical support in peer-review of papers for our annual meetings. We participate in and support specialty conferences and workshops, contribute to technical programs whenever we are asked to or can, and provide comments on regulatory and relevant technical issues as we doing here today.

For this particular event, we assembled a group of AB-3 members. They're all listed here.

Just for the record, the current Chair of AB-3 is Mark Bennett. I'm the Vice Chair and David Long is the Secretary. I believe every one of those folks listed here are here today and have provided in some way comments to this 10th Modeling Conference. I'd like to thank them for their time and effort to put in the comments that we have already presented and the ones we're going to present here today. It's purely a voluntary organization, so any time effort that's spent is truly out of our own time.

We got involved talking about the 10th Modeling Conference actually about a year ago and that resulted in meetings internally with AB-3 and then, subsequently, we had a meeting with EPA staff OAQPS -- a conference call on August 4, 2011. At that time we, I think, discussed approximately fourteen or fifteen different topics and issues that the membership was interested in either addressing or participating in discussions with at the Modeling Conference. At that time, we all thought the Modeling Conference would be in October. We actually were able to put more information and provide better comments, I think, due to the fact that it was delayed but, nonetheless, out of those comments, about five different topics and I believe seven or eight specific presentations came out.

A couple of those topics were asked by George to be brought into the main agenda of the 10th Modeling Conference, and those have been presented already. Ron Peterson presented on Tuesday on the use of equivalent building dimensions in AERMOD, and Gail talked a little bit about the system availability -- the modeling -- the CALPUFF modeling system and the availability of such also on Tuesday. Today nearly following my introduc -- my introduction is going to have Joe Scire and Lloyd Schulman. Joe's going to have actually two presentations, first on new developments and evaluations of the CALPUFF model, basically folks in the Inversion 6.4 work that he's done, and then also an assessment of EPA ETEX evaluation study. Lloyd is going to further discuss the issues related to the building downwash modeling with AERMOD. Later on today, George Schewe is going to also have a presentation on AERMET versus AERMINUTE issues that he's been working on.

There are other topics of interest. As I said, in August we had talked about fourteen different areas we were interested in, and some of those have -- are still on our radar screen. Some of these are leftovers from the last conference, so you might have seen some of these comments and recommendations before, but to the extent that they're still of interest to the membership, we thought we'd repeat them here. There are three listed here and I'll go through each one on subsequent slides. The last one here relates to long-range transport database that we know has been available but other than I believe Joe, who got it through a FOIA request, we have not seen it. I think the community would like to know whether that information or database is available and, if so, how do we go about getting it.

Regarding the Model Clearinghouse, we are very happy that George has been brought on to address and manage the Clearinghouse, and we look forward to that, but traditionally the concerns of the AB-3 committee members has been that the Clearinghouse process does not allow for technical input by the affected parties, the applicants and the general. Therefore, we believe that permitting authority just states their opinion and ask for Clearinghouse approval without much involvement from the applicants. AB-3 would recommend that the affected parties be able to provide comment along with the permitting authority's correspondence to the Clearinghouse. I think that getting involved would result maybe in a quicker and faster resolution of the issues and, hopefully, the resolution would have a common consensus on what the result is as opposed to just having handed down a requirement or a mandatory resolution.

Regarding the status of the modeling guidance, we know that issuance of modeling guidances have been lengthy at times and a lack of final guidance sometimes for the user committee can result in the viability of projects and the projects' schedules can be affected. Specifically, we're talking about SO2, the draft guidance that was out since September 22, 2011. However, the final guidance we haven't seen and we heard yesterday I believe that we don't have a specific date for that to be issued. Also, the secondary PM2.5 guidance that has been promised several times and expected to be out as of this conference, we have not seen yet. We are very interested in seeing that. AB-3 believes that a collaborative effort might help in expediting the process of some of these issues, and we just want to know how can we help? How can we get involved to help EPA recognize the limited resources you do have, to not only set the priorities as Chet said, but also provide support and technical input whenever we can.

Finally, I'd like to announce as Tyler mentioned the other day that, in fact, AB-3 has been asked and is considering and planning the 5th Modeling Conference. We are -- we actually discussed this last night at our meeting. We are planning to have it late this year, early next year as Tyler suggested. It's likely to be more the beginning of next year than late this year due to the schedule that we already have. As the last one, I think to accommodate and encourage the participation of EPA staff, we'll likely have it here in Raleigh. If there's a particular venue that seems appropriate, let us know. Otherwise, we'll find one or maybe the same as we had last time, and for information on that and for a call for papers for that, you can stay tuned to wma.org. Thank you.

MR. LLOYD SCHULMAN: Good morning. My name is Lloyd Schulman with Exponent in Natick, Massachusetts. Today I'm going to be sending some comments on building downwash and AERMOD. I'd like to acknowledge my co-author with these comments, Joseph Scire. The outline of the comments is going to be talking about the recent change. I guess a year ago Version 11059 to AERMOD, which we found is leading to significant but unverified increases in predicting concentrations for, mostly for wide buildings, but also we want to talk about, even without this code change, this recent code change, there have been documented over-predictions for very wide buildings and I'm going to present some slides about that also.

I won't dwell on the GEP stack height. I think we all know this quite well. The only point I wanted to bring out was that people talk about the 40 percent increase for excessive concentrations but that has to be paired with contribution or causing a violation of the standards, so you have to avoid that. That's the definition of excessive concentration. The change that was put forth about a year ago with Model Change Bulletin Number 4, basically what it does -- you don't have to read all the italics -- basically what it does is it says that you have to model downwash now for all stack heights, whether they're below or above the GEP formula height, and the previous policy was to only model downwash effects for stacks that were less than the GEP formula height. Now we heard on Tuesday that the change was made to eliminate discontinuities for stacks that were straddling the formula height and that the change will be justified by a pending clarification memorandum. But as far as I know, we haven't seen any consequence analyses. There hasn't been any public comment or review and we feel that this is a very significant change to the model that should warrant more than a clarification memorandum in our Model

Change Bulletin. Now, I guess I'll start off by saying, yes, there is downwash at formula height. I think that's well known. It's part of the definition of excessive concentrations, but that's not really the key question. The key question is does PRIME model it correctly for stacks above formula height, and we think that it doesn't for many cases, and I'll be talking about that today. The PRIME algorithm was developed mostly for buildings that width to height ratios that were quite small. I'm going to list on the next slide of what those were, but they were all less than 4.4 and it was sub-GEP stacks. Now we're applying the model to stacks and building shapes that are outside of that range. There was already a problem that everyone's been well aware of for very wide buildings because of the unrealistically long projective lengths for BPIPPRM. Roger talked about that on Tuesday and proposed an effective length remedy that looks like it's a step in the right direction. We're very encouraged by that, but we know it needs further testing and confirmation. But this change now in the allowing downwash for all buildings, including these wide buildings, is only going to exacerbate that problem, and I'll demonstrate that with some case studies.

This is the width to height ratio for the evaluation databases. The one that we relied on most in developing PRIME was Bowline Point. We had half the data for independent - for developmental data for evaluating the model and half the data was for independent evaluation. We also relied quite heavily on the Snyder's wind tunnel data and Alaska north slopes. So, most of the data that we used to develop the model are for buildings that were not very wide at all, and we got quite good agreement at least for the sub-G, the stack heights with the data sets.

So, what I'm going to do now is talk about a case analysis that's based on an actual source. The numbers are roughly about what they are for that source. It's a buoyant source. It's a very low building. It's twenty meters high, 220 meters wide. So, the width to height ratio is about eleven. The existing stack height is above the formula -- the formula height, of course, would be two and a half times the twenty which would be fifty meters would be the formula height for this structure. It's a little bit higher than that but because it's below the sixty-five meters, they get credit for that stack height and have to model it now with downwash, whereas before there was no downwash model with that stack height.

And what I'm going to do is vary the -- both the stack heights for that structure between something sub-GEP and up to a number that's sixty-five meters, which would be three and a quarter times the stack height, and vary the width as well from a very narrow building, which is half the height to one that's twenty times the height. And we did a one-year simulation, calculated the maximum concentrations without the buildings and with the buildings and those are the results I'm going to present in the next couple of slides.

This slide's a little busy but I'll try and explain it. The -- on the vertical axis is the concentration. The ratio -- concentration that the building -- with the building and to without the building and on the horizontal axis is the ratio of stack height to the building height. So, the excessive concentration threshold which is 40 percent is right here going across because this is the concentration ratio is the 40 percent, and the vertical dashed line is the formula height for this structure, which is two and a half times the building height. So, everything to the right of this vertical line with -- previous to the change would not have been modeled with downwash, and everything to the left of the line would have been modeled with downwash. So in the first slide, I'm showing just the narrow structures which are kind of in the range of what we tested PRIME for and you can see, for example, that I'll look at the width to height ratio of three and at two and a half times the building height, you're looking at about a 25 percent increase with the building as opposed to without the building. And for the other stacks, the other buildings like the cube and the one that's half, which are narrower, as you expect, you get even lesser effects and they are all below the 40 percent.

I also wanted to mention this line. This line comes from the EPA's guideline on GEP -- GEP's guidance for the termination of GEP stack height. It was put out in 1985 by Bill Snyder. Alan Huber was probably involved in it as well, and this is based on work done by Rex Britter, Hunt and Puttock on a symposium paper they presented in 1976. It shows what they call the theoretical estimate of the maximum increase that you'd expect for any -- any building width to height ratio, and it's developed for a very long building. In fact, I believe it was a two-dimensional obstacle that they used to develop their mathematical estimates of what that would be, and you can see that all the numbers are below that line. Now when we jump to the wider structures that go from four to six to eight up to the twenty and look at the variation with building width and with stack height, you can see that except for the width to height ratio of four, almost all of them are -- again, I want to point out this dashed line is the Britter line which is supposed to be the theoretic less than the maximum increase. Almost all of the points are well above that and we're getting values for the very widest structures that are eleven and twelve times higher than what would have been gotten without a building. And again, all these points over here on the right side of this vertical dashed line are points that previous to the change in the model would not have been modeled. They would have been down here at one because you wouldn't have modeled downwash. So, with this change in the policy to avoid the discontinuity, you've introduced or exacerbated a problem that we knew about and made it a lot worse for a lot of sources.

So, just to go over this quickly, the EPA has done research on building width to height ratios. In the guideline for the determination of GEP stack height, it does say that the maximum ground level concentrations should not be increased by more than 40 to 8 percent for a stack that's at formula height. You see that that's not met with the previous work and the theoretical estimate of Britter, if you looked at the lines and tried to figure out what it was, it's about 85 percent increase at two and a half building heights which is formula height and about a 50 percent increase at three times the building height per stack, about three times the building height.

The research by Alan Huber's paper from 1989 was also discussed on Tuesday and it was used to evaluate this effective length parameter and it does offer a good dataset, but the summary of that paper at the end said that as the width to height ratio increased from above the two from four to eight, the maximum ground level concentration was downwind decreased at all distances, and that when you started to get width to height ratios greater than eight, it had little effect. And this -- this kind of makes sense because if you look at a building that's say a cube and you put it at a forty-five degree angle to the wind, you're going to get like a wedge shape that's going to create a Delta wind effect and you're going to get these strong vortices coming off the leading edge and around the ends that are going to be a very strong downdraft that streamlines, and you're going to bring strong downwash to that problem. When you start to get a very wide building and put it at an angle, you're going to lose that Delta wind effect and I think this is what this study is showing, and the air around the sides of the wide building never make it into the middle anyway if your stack is in the middle. If your stack is in the corner, it's a little bit of a different issue, but that's why we need more research.

This is a -- now I want to try and explain why we're getting these problems like -- Roger touched on it a little bit. It's the affected - it's the projected length issue, but this kind of illustrates the problem. This is the actual structure, and it's a long building. It's ten times wider than it is high. Stack is two and a half times the building height, but when you have a wind at a forty-five degree angle to that structure, this becomes the projected length from BPIPPRM, and now your stack, which is on the upwind edge of the real building, is in the middle of the new projected building that goes into PRIME and the cavity which would have started somewhere over here is now starting way down here. So, everything is skewed away from what it really is and it creates problems with predictions from PRIME. What I did was take the worst hour for the width to height ratio building of ten. I looked at 1-hour, just to see what was going on and what I did is I varied the length of the -- of the building. I started to bring it in from the downwind edge forward, and if you look at the -- this high number, this is the number that you would have gotten with the projected length as presented by BPIPPRIME, and you get a very high increase concentration with the building over no building. So, you're getting a very large increase with that building. The reason is, is that as the plume has more time to travel before it reaches the near wake, it's spreading more in the vertical and more of that plume is being captured in the cavity and giving a very high concentration. As you start to shrink that length, it drops very rapidly and it seems to be a switch point, in this case where the length to height ratio is about five and a half. There's a switch point and now the maximum jumps to the far wake, and it's pretty flat because most of the mass is in the elevated plume and the far wake is further away. It's much further out where the maximum is. It's a pretty flat curve. It does drop off and now this length that I used here is the length that would be comparable to what the long wind fetch would be for that angle for that building, and you're getting a much lower number with an increase of about two. This is consistent with the kinds of things that Roger was talking about with the effective length, and it's an important thing to get the length right in this model.

Also, I just wanted to go quickly through some work that was done earlier for a Alcoa, Tennessee smelter where they had measured data for two years. Most of the emissions were from point sources, and the simulations were made with BLP AERMOD and CALPUFF and measured at -- comparing it to monitor concentrations. This is an overview of the plant and you can see its line sources. It's quite long. Most aluminum reduction facilities have width to height ratios of about twenty, and the results that we found for the 1-hour, this is a quantile quantile plot, and the dashed line is the observations at that monitor. BLP and CALPUFF did quite well. They over-predict by, I don't know, about 50 percent maybe at the top end of the distribution. AERMOD was over-predicting by about a factor of two at the top end, but maybe by a factor of ten somewhere here at about the ninety to ninety-fifth percentile.

When you look at the annual numbers you see the same kind of behavior. AERMOD is about a factor of ten over what was observed for the annual numbers for the two years of the study. CALPUFF and BLP, which are using a different downwash model for this, it's the BLP downwash, do quite well because they don't have this effective length problem. So, the findings from that study for very wide buildings were that when you look at very wide buildings, you're getting numbers that really are comparable to what we've seen with our case analyses. The fact is, we got overpredictions of about ten to twelve, and in this study that we're getting overpredictions of about the same amount. So, it's kind of a comparable result with measured data that supports what we're seeing with our case study, and BLP and CALPUFF, the BLP algorithms did quite well. So, our recommendation from this is that EPA should allow the use of the alternative models and Section 3.2 petitions.

So, this brings up a couple of questions. For example, if you -- if you did a fluid modeling study and found the stack height that gave you that 40 percent increase, then you put it into AERMOD, you had a fairly wide building, you could wind up with an order of magnitude increase for that stack height. Now what do you do? Do you model -- do you keep that stack height that you got from the wind tunnel and get an order of magnitude increase or do you get a higher credit and say, well, AERMOD says that the value is ten times higher, so I'm going to be able to raise my stack even more. And in the end, aren't we really trading one discontinuity for another for these buildings?

So, the conclusions I have is that the -- the change to the downwash procedures in AERMOD are significant. We showed with the case study that there was about a sevenfold increase for width to height ratios of eight, and a twelvefold increase for even higher, with the height ratios for stacks of formula height. And these results are inconsistent and, in fact, they are contrary to what we've seen from the wind tunnel studies and theoretical estimates by Britter for basically two-dimensional structures which have a cap of about 85 percent at formula height. There hasn't been any peer review or comment on this change to the model and we think the clarification memo really isn't sufficient and it should be studied further before it's released. Our recommendation is that we should remove this from the regulatory version until it can be further studied because of the fact that it's causing even greater problems with some of these wider structures and also because we really haven't even evaluated it for narrow structures the stacks above formula height. And I think a suggestion I would make would be that we should bundle with the work that Roger's doing now with the effective building length, since they're really intertwined. I mean, the wide building problem is intertwined with the effective length parameter that Roger's talking about versus the projected length that's in the model now, and we feel that the best course would be to correct the wide building problem looking at something like the effective length parameter and then with that improvement, then go in and evaluate it for different stack heights and show that it works and release this all as one package. We're not -- we're not against modeling stacks above formula height with downwash effects because they exist, but we want to get it right.

MR. GEORGE BRIDGERS: Thanks, Lloyd. Now we have a couple of presentations, again under the auspice of the AB-3 Committee, from Joe Scire.

MR. JOE SCIRE: Okay, good morning. I have two talks. The first talk is about the -- it's an assessment of the EPA evaluation study that was recently -- recently released and, in particular, I'll be focusing on the ETEX portion of that study. ETEX is the European tracer study and it covers the area shown in this figure. It actually covers a very large domain and it involved -- it was designed actually for evaluating emergency response type models. So, accidental releases are emergency responses. In particular, the concern was nuclear material released from accidents from nuclear power plants. In this particular experiment, ETEX-1, the tracer was released for twelve hours, starting at about four p.m. local time, and there were samples taken at 168 samplers all over western and central Europe and -- most of these samplers were well beyond 300 kilometers. In fact, some of the samples were taken as far as 2,000 kilometers away. So, to put this in perspective, most of the applications with CALPUFF are done for Class 1 type studies, and this -- within about 300 kilometers, and this is a 300 kilometer box, just to give you a sense of what that -- that looks like. This application on the CALPUFF side is something that is well-beyond the kind of applications for which is normally used. Five different models were evaluated focusing on ETEX-1. There have been a number of papers and presentations over several years on the results of these evaluations presented by EPA or, more recently, by Environ as well. The information that I received and I'd analyzed was obtained through a Freedom of Information Act request, which was made in October of 2010 and the data were received in August of 2011. The study used the new EPA processor MMIF to drive CALPUFF that used 36 kilometer MM5 data. These statistics we heard about a little bit yesterday. There were two that I'm focusing on, the figure in merit -- the figure of merit in space and the rank statistic. I just wanted to mention about the figure of merit in space, it basically determines what's the overlap between a predicted and an observation.

The results that were presented in the EPA report or the Environ report, I wasn't sure what to call it so I called it EPA Environ, they -- they were -- they looked pretty bad for CALPUFF. They had the lowest figure of merit score and CAMx had the best. The other models were somewhere in between. In terms of the rank, it looked pretty bad for CALPUFF again. The rank was pretty low, less than -- one about .7 and, again, CAMx came out in the top and the other models were somewhere in between. So, I, once we had the data, we could then look at this and -- and really figure out what was going on.

As people have mentioned a number of times over the last few days, the details are important, so we finally had something we could work with. We had data and it is probably a year old now, so I don't know if it's exactly what was used in the final report or not, but all I can talk to is what we received under the Freedom of Information Act request. There were a veritable number of configurations for each model. CAMx was run 20 different ways, and the rank used was the highest of those 20 different runs. The HYSPLIT was run nine different ways. Its rank was assigned a value of 1.8. Although it had higher performance in some other cases, that's not the one that was presented in the summary slides. SCIPUFF, we could only find one run and FLEXPART, and then CALPUFF had about six runs. On CAMx, there were runs with the Plume-in-grid and without it -- without it produced the highest score and that's the one that was presented. With the pick the scores were, in some cases, substantially lower. HYSPLIT arranged from some pretty low scores, one up to the highest score of any of the models in those runs, which was 2.1.

Okay, so what did we do? Well, the first thing we did is we ran CALPUFF the way EPA ran it, tried to determine could we reproduce the results. We couldn't exactly, but we felt it was close enough. So, we think we understood what the model parameters used were. So then what we did is we did some sensitivity runs. Clearly, the problems I'll show you is that CALPUFF had the model plume going off on the left side of a diversion flow. And if you want a left side of the branch, it went off to where the observed plume didn't go. If you were in the center or the right side, then the material went a different direction. This was very important because by making certain changes to the way PUFF's splitting was done and also correcting some -- some areas in the input files, we were able to get numbers that were substantially better on this figure of merit score, and it's really almost a binary thing. Either you get it going the right way or you miss it entirely. So, it doesn't mean that you have to change a lot to change the results substantially.

On the -- the rank, which is a weighting of four different parameters, we did some tests. We think this is the one that would most closely correspond to the way we would run it in this particular application, and we're -- in this case, the results again changed dramatically. Small change of where the plumes go is at the beginning, other release matters a lot.

We also ran the model in a CAMx type of way with an initial dilution associated like in a layer in model, there would be a dilution over the 36 kilometer grid cell, and we find that if we add initial dilution, we'd get numbers that were really quite high.

I'm not saying that's the way we'd run the model, but it does help to explain to a certain extent why CAMx does fairly well because it's a highly diffusive model when you run it in a Eulerian model mode without the plumbing grid.

Okay, so how did we go about this? What happened? Well, in analyzing the winds, it's very clear that ETEX-1 is very sensitive to exactly where the puff goes at the very beginning. Small changes in the trajectory will result in either a complete miss, which is what happened in the EPA runs or not. So, what you do and how you initiate the splitting is very important. You really want to do a lot of horizontal splitting at the beginning and get lots of puffs. Some of the models do that effectively immediately like your layering grid will immediately dilute the plumes over thirty-six kilometers by thirty-six kilometers. So, it's less sensitive to the exact, maybe deficiencies in the wind because you have a wider plume to start with.

Also, the performance measures are very highly dependent on time and pure space statistics. So, the amount of overlap that you get matters a lot. We also looked at MMIF versus CALMET. We found that we had some better runs with CALMET, and I'll go into why a little bit later in the talk, and then we looked at the resolution. In fact, the meteorology at thirty-six kilometers didn't pass the performance tests that were done to evaluate its performance relative to the guidelines. So, and I think that it is quite coarse, especially when you're talking about the near-field, and then we -- as I said, we'll go through some things that I found that needed to be changed about the setup. So, I have four runs the way it was run by EPA and Environ. Well, the way it was run by EPA and then analyzed by Environ. Then different runs looking at thirty six kilometer MM5 data with enhanced splitting, and then we also rated with twelve kilometer MM5 data because we believed the -- the Met data is a significant factor in the performance, at least for CALPUFF, and then again with the enhanced splitting. This is the result from the EPA runs. These are the observations. You can kind of see the colored area with this plume going all over this part of Europe. CALPUFF went off into left field because it took the branch and got caught in that, and the puffs did not split property. The puffs basically went as a group unsplit the wrong way. SCIPUFF had -- was fairly diffusive and covered a large area, FLEXPART, HYSPLIT and CAMx. If you -- you're up here and all your observations are down here, your scores are basically close to zero.

So, the first run we did was to fix certain errors in the configuration, but random with MM5 data, EPA's MM5 data, although running CALMET rather than MIFF. And you can see what you -- what you started to see is rather than this blob going up here, this is the -- the original run at T plus thirty-six hours and T plus sixty hours. This is the modified run at the same two times. You start to see this plume extending in a proper direction over central Europe. Second run, we optimized the splitting in the near-field and we got even better results here. So, the original run and a revised run. In all of these slides, the left-hand column is the original EPA run in every slide and on the right-hand slide is the revised run. Run number three we use here better MM5 data twelve kilometers resolution. Then we start to get a really good pattern. It looks quite decent. Together with splitting, we're getting something that performs fairly well and then, finally, the last one is we, again, optimized the splitting in the near-field. The first twelve hours of release are actually critical in this, and you see that the -- the paths are quite different.

Okay, MM5, what are the problems with

it? Well, okay, thirty-six kilometers is a pretty coarse resolution. The dispersion models are not all equally sensitive to it. If you have a Eulerian model and you spread things out, it's less sensitive maybe to certain areas in the -- in the model winds. It doesn't mean you'll get a better result for regulatory purposes but you're less likely to get a miss. The twelve kilometer MM5 simulations had better resolution and they performed better. And when I say they performed better, there were some data at the release site. A sonic anemometer at eighteen meters, which is the blue trace, a sonar with four level -- three levels up to one hundred meters, and then the green line is the MM5 data. So, what this is showing is the wind direction and this red line here is the twelve-hour release period. So, this is a critical time period. This is where the tracer was released and it was allowed to get transported downwind. You can see that the MM5 winds were off by about thirty degrees, and it can make a big difference between missing and hitting that -- that branch point. And then -- then the wind speeds also were off quite a bit. You're seeing the MM5 winds being quite light relative to the sonic anemometer in the sonar data. When we used the twelve kilometer MM5 data, the MM5 did better. It had a better match to the winds measured at the release site.

Okay, Environ's report indicates that they conclude in, I believe incorrectly, that the meteorological errors that were noted in the evaluation were not the primary cause of poor performance. It certainly was a big factor in the performance of CALPUFF. In -- we also showed that with tests with the better winds especially, but even with the old winds, when you correct certain errors and had proper puff splitting, the results improve quite a bit.

I also will not have time to talk about this today, but I think the performance measures that are being used are not really as -- are not as relevant as they should be for regulatory modeling. I think they overemphasize the peering in time and space and they don't account for other factors which it can be sometimes even more important than that. But that will be another talk at another time.

Okay. So, I mentioned errors. What were the errors? Well, we looked at the files. There were some datums that were incorrect. In some places, the data was specified as NWS-84. In other places, WGS-84. The tracer released in the CALPUFF run was over the wrong time period. It was over thirteen hours rather than the correct twelve hours. So, and also the averaging periods when you had an absurd three-hour average concentration and the predicted three average concentration, they weren't the same three hours. They were misaligned by one hour. So, there was an overlap that they weren't strictly speaking correct.

The stack diameter in the CALPUFF one was set at one meter but it actually is not. It's a very small stack and that's a picture of the release. It's more like one inch. That isn't actually that big of a deal in the results in that particular issue, but one that is is the puff release rate. There's been some discussion yesterday about slugs and why does it help and why does it perform well. If you use the integrated puff model in CALPUFF and you only emit one puff per hour, which is the way this was run, you take a full time step worth of emissions, 3,600 seconds of emissions and packed into a single unit, a single puff, and send it on its way. And in a case where you're measuring overlap with observed plumes, that's not a very good way to do it because by definition, you're too much mass for a ride. Too early and you won't have the trailing mass coming out at the end. So, that's responsible for some of the problems is that the configuration it had, one puff released -- releasing the mass rather than many puffs. And then there were some other issues with documentation not being quite correct about whether trained adjustments were made or were not. They actually were made.

So, these things were all corrected plus the other things and then we modeled the -- the results with these new configurations. So, I wanted to mention one other thing. There are some statements in the Environ report about MMIF and CALMET and, basically, one of the presentations says that -- that, these are taken directly from that. Concerns have been raised regarding CALMET. There are many options so you can get multiple answers. That's true. You can draw many options. CALMET has been shown to degrade meteorological model performance. I'm not quite sure what that means. We don't see that happening, and we find that maybe there was some errors made in the attempt to achieve a pass-through MM5 data into CALPUFF through CALMET in the EPA runs. They -- EPA developed this tool which, by the way, I think is a very good development, especially for the fact that it is interfaced to the graphical display tools. So, I have no real problems with MMIF, but I guess what I'm trying to address is some of the comments related to CALMET. They go on to say MMIF will bypass CALMET and its problems. Well, I don't know. We got better results with CALMET, and these problems if you feel don't exist if you run CALMET properly. So, CALMET, if it's properly configured in its pass-through mode, does not change the MM5 winds at all. It passed through exactly at the same point in space to the same point in space in CALPUFF. I mentioned about the other day about MMIF, MMIF always does spacial interpolation of the MM5 winds. MMIF by its design does not place the CALPUFF and CALMET grid points at the same location as MM5. It's displaced by half a cell. So, if you have NX Plan Y MM5 grid points, MMIF will produce NX minus one by NX -- NY minus one grid points for CALPUFF. As a result of that, you always do interpolation, whereas if you do a true passing with CALMET, putting the CALMET grid points at the location of the MM5 cross - dark points where the winds are defined, that doesn't occur. CALMET will pass through MM5 data on MM5 grid points. MMIF does not exactly. It does this interpolation. Both programs will do vertical interpolation because they -- the two models have different grids and that's unavoidable. To make it clear, this is the configuration to do a CALMET pass-through. These are the settings that you have to place on CALMET to have CALMET go through and just provide the MM5 data unchanged. We did a couple tests. We took the ETEX experiment, took a bunch of grid points. We ran it through MMIF. We ran it through CALMET and we took those grid points and compared the original MM5 wind direction with the output coming from MMIF, and what you see is pretty highly correlated but there are some differences and some scatter, and it's because of the horizontal interpolation that's being done. That is not necessary to do that, but it is being done. In terms of CALMET, in true pass-through mode, they match exactly. The same with wind direction. This is -- I'm sorry, this is wind direction. This is wind speed. Same thing. The interpolation causes some change in terms of what CALPUFF sees coming out of MM5.

Okay. So, to conclude, EPA and Environ did this study. They focused on ETEX-1 and we focused on ETEX-1 for our analysis. What we found was that the relatively simple unqualified conclusions, CALPUFF is the worst, CAMx is the best, isn't supported by the data. I think it's more -- a broader range and, of course, you always have to run the models to try to model the physics properly. There were some errors, there were some inappropriate model configuration settings, and the data driving the model was really not adequate by the EPA's own definition of the model stats program. When we changed the configuration, it did much better and then with the MM5 at twelve kilometers, it also did even better.

What I would say is for a recommendation is that this process and these comments are coming very late in the process. A lot of this work was done over three or four years, and it was due to the lack of access to the data sets that we couldn't really comment on it. We saw the plots and the figures, but you have to go into the details to actually understand how the models were run and offer it suggestions or corrections. So, consistent with AB-3, what we're recommending, or what I'm recommending here, is full and timely access to all model evaluation data for the entire modeling community. Now I have this data set but others don't. I think everybody would benefit having the ability really poke around to see how these models were run and make suggestions. I also think that the evaluation process would benefit by the involvement of the model developers, at least to have a level of fairness. I mean, one model's run twenty times by a group that it does the evaluation, another model is only run once. I think to be fair in the evaluation process, our input can only help you do a better study.

For future work, we're planning to look

at the other data sets. There's a lot of information in that report. It's a work over three or four years and it will take some time to analyze, but we'll report on those findings in future papers and conferences and also, I just want to mention about the evaluation measures. I think this question was whether these are the correct measures to use for this kind of problem. That's the end. Thank you very much.

MR. JOE SCIRE: Okay, I'm back. This talk is concerning new developments and evaluation studies that have been done with the CALPUFF model, and I'm talking about Version 6.42b. This is the version that has been funded by a number of groups, actually, going back to API funding, AER to do some initial work with a version of this work that I did when I was at TRC, Phase I, and now work continuing at my current company which is Exponent Phase II. West Associates funded the implementation. The electric part, Research Instituting has funded some of the evaluation work. As an overview, 6.42b, for we have to be careful, people refer to Version 6 as 6.4. The current version is 6.42b that reflects all of this work. It includes the current version of the ISORROPIA aerosol scheme 2.1 similar to what's in the Eulerian models CAMx and CMAQ. It includes EQuIS phase chemical conversion of SO2 in rain droplets and then rain to sulfate, and this scheme was adapted by AER, extracted from RAD and then put into a version of CALPUFF. One of the new changes with this new Version 6.42b is that it couples the output of MM5 WRF with liquid water content data to feed directly into the EQuIS face chemistry. Previous versions just used a constant assumed liquid water content. And then there's some other enhancements as well. New RIVAD scheme, tracking ozone depletion of the plume, and then as a secondary organic aerosol module that is based on a CALTECH as OA routines that's in CMAQ. We evaluated the new version of the model relative to an existing dataset that's been used quite a bit and also a new dataset, the Cumberland 1999 dataset, and then we did some sensitivity into comparison tests between the chemistry and CALPUFF with that in CMAQ 5.0. For the SWWYTAF first dataset, this is a full simulation. 17 out of -- 700 -- 17 -- 17 out of 76 sources in southwest Wyoming and surrounding areas with two monitors in the area for air quality and a number of monitors for acid deposition, sulfur and nitrogen deposition. I can't talk about everything in 15 minutes, so I'm going to focus on the air quality data and I'm also going to focus on nitrate in particular.

This is the modeling domain, the

monitor's here in this area, complex train all over the place, and a lot of the sources in this region but also in Utah, Idaho and northern Colorado. We ran the model different ways. Well, we ran the current chemistry, the modified new chemistry with and without the ammonia limiting method. We tried the new aerosol chemistry and the old aerosol chemistry, background ammonia, constant as maybe the land managers would request and then seasonally varying, and then EQuIS faced chemistry looking at the effect on concentrations of both the -- the -- the wet removal brought by scavenger coefficient (Inaudible) phase removal through the EQuIS faced chemistry module. Liquid water content before, it was just a constant for precipitating and non-precipitating clouds. Now we're using full data from MM5 in this case. She modeled both of those. We had seasonal ammonia. Now these were based on new measurements for a different year than the simulation year but these were considered better data for ammonia because they're actual measurements in that area. Doug Bloom gave a paper on this. I think it was a year ago at an AWMA Conference. Okay, to get to the bottom line here. We have the results from CALPUFF - old chemistry, old scavenging and any observation at this site of nitrate, this is particulate nitrate, .1 observed and about .53.

So, you'll probably hear or have heard about lots of complaints about overprediction and, depending on how you run CALPUFF, you certainly can get that, you know, with the old chemistry. If you run the model, this line, as most class-run analyses would have to be run, you wouldn't use varying ammonia. You would use constant ammonia, and the results are slightly worse by doing that. Incrementally, we implemented each of the changes and evaluated its effect separate from others. When you put in, in this case ALM, which has been around for ten years or so, the -- the concentrations are better relative to the observations but still more than effect at three over-predicting. When you use the new ISORROPIA chemistry for nitrate formation, even without ALM, you do much better. When you use the ISORROPIA and use the EQuIS faced chemistry, you do a little bit better. When you combine these effects and use the MM5 liquid water content, then you're doing fairly well here and then if you use ammonia limiting with that, you're over here. Even with constant ammonia, if you just focus on the chemistry with none of the other improvements, you still -- you do much better. So just the new chemistry alone without better ammonia or any other improvement goes from -- from this to this, much closer to the observations. Still conservative but closer to the observations.

Similar result at a different site. The same story. I won't repeat it. So, to summarize, CALPUFF using constant ammonia with the old chemistry is over-predicting quite a bit. This is one of the concerns of the electric power industry in particular related to best available record for technology demonstrations that are due soon. This is really the reason why there's been a request to review and approve at least on a case-by-case basis the new chemistry for these types of applications. The ISORROPIA 2.1 improves things substantially. Use of seasonally varying ammonia also improves performance. The reason is ammonia tends to be highest in the summer and lowest in the winter, and the wintertime is when a lot of nitrate forms because it likes to form at low temperatures. So, the variability is quite important in terms of ammonia. EQuIS faced chemistry, the land managers have expressed a lot of interest in this because they've always wanted to have an EQuIS faced chemistry capability for these classified assessments and in this office now. ALM is less important for us it appears, than for the MESOPUFF scheme. The degree of overprediction with ISORROPIA is less than MESOPUFF scheme. That's fine. That might be the reason why.

The second study we did was with the Cumberland field study. The aircraft flying through a plume, this has been described earlier in the meeting, and I'm not going to get into talking about the data so much. What we did is -- we extracted what we could from the dataset. There are some significant limitations to the data, but I'll summarize it here. If you look at say the -- this gray area, this is the amount of NOx that's been converted to nitric acid plus nitrate, and you see with MCHEM=6, which is a new chemistry, the performance is better than with the old chemistry where it's not matching the observations so well, especially at greater distances.

This is another day in the study, similar sort of result here. The new chemistry does less well but still is reasonable. The old chemistry doesn't do so badly with this one on this day. And on the third day, here the new chemistry does substantially better than the old chemistry, which doesn't really correct -- predict the correct total nitrate. So, to summarize this, neither reports on this and there's a paper being prepared for this. The -- the observed conversion rates are something of the order about -- the upper limit is estimated at about 3.4 percent. The new chemistry is a little bit on the

-- a little bit less than that. The old chemistry in MESOPUFF is over the upper limit and the MESOPUFF chemistry is actually under the limit as well, but less close to the computed value. We found that we couldn't really use them to predict the observed nitrate in this case. There was some measuring problems and that's discussed in some of the documents.

The final thing I would like to mention in the last four minutes is to talk about some sensitivity tests. The question is we have a couple of evaluations. CMAQ is a well-established model used widely. How well does the new chemistry compute the CMAQ when the inputs provided are the same in terms of the concentrations? There were some differences in ISORROPIA in CMAQ versus that which is in CALPUFF. There were some bug fixes in the CMAQ version. There's -- there's expected to be a new version of ISORROPIA out soon by the model developers of that scheme -- that's something that it would be maybe useful to include in CALPUFF in the future as well. What we did is we used the Monte Carlo driver to look at a whole range of conditions. In fact, three million conditions and predict the values out of the CALPUFF sub-routines and the CMAQ's sub-routines, just to say these -- the implementations of these schemes the same in the two models and, in fact, it turns out they are. The ratio of the predictions are less than .01 percent and 99 percent of the three million tests done over a large range of temperature and other conditions. For CHEMEQ, because the schemes are different, that's using a different scheme, that doesn't show the same agreement as you would expect it not to. Although 63 percent of the simulations with the old chemistry in CALPUFF were within .01 percent of the ISORROPIA and CMAQ. So, the old chemistry wasn't that bad. There were -- maybe 35 percent of the time, it was -- it gave different results. But a lot of times it gave similar results to the new chemistry. This is a scatter plot of that showing CMAQ result versus CALPUFF result, and there's a little bit of scatter but pretty close to being a one -- a one-to-one line. This the old chemistry. There are huge numbers of points along this line. Hundreds of thousands and millions. There's not too many points elsewhere, but it just shows when the old chemistry is wrong, it can be quite wrong. It can be predicting a hundred percent nitrate and ISORROPIA or CMAQ's predicting zero and vice versa. So, although they -- it doesn't do poorly all the time, under some conditions it does.

Okay, so to summarize CALPUFF Version

6.42 includes significant improvements in the chemistry. The evaluation work shows that it does very well for a full year dataset. In Wyoming, it eliminates some of the issues related to large over-prediction of nitrate and in the Cumberland field study, the -- the comparison looked favorable. It looked like it performed better under most conditions than the alternatives and the old chemistry.

The conclusion from this is that the new chemistry just based on their origin and pedigree, in a sense, well established algorithms. They're -- they're the peer-reviewed literature. They're used widely by many different models, and they're almost universally accepted as better science by the modeling community. CALPUFF 6.42b is back-ably compatible with Version 5.8. Again, it can be subject to confirmation and additional tests. If it's not, it can be made so, but every test we've done it has been. And we recommend that it should be adopted as replacement for Version 5.8 in that the use of the chemistry in BART applications is something that should be at least considered and allowed if appropriate as a non-guideline tool but one that is ready for use today, and that's when these types of applications have to be made. Okay, thank you very much.

MR. GEORGE BRIDGERS: Thank you, Joe. That concludes the talks this morning that were scheduled by the AWMA AB-3 Committee or at least their sponsorship. In the 9:45 to 9:55 slot, unfortunately, CV had a family medical emergency and couldn't be with us today. He'll be submitting his comments to the docket, as I had said earlier. So, if you want to call it cap and trade or a little bartering, we're shifting things around just a little bit. Andy Berger's way down the list at 3:45 but we - we had some discussions since -- you don't have a presentation, you just -- okay, so he's just going to offer some -- some comments and then we'll get back on track. So, let me just put a stoic thing there.

MR. ANDY BERGER: Good morning everybody. Thank you, George. My name's Andy Berger. I'm employed by Tri-State Generation and Transmission Association, an electric utility in Denver. CV sends his regards. As George mentioned, he had a family emergency and was not able to attend. My comments being substantially similar to CV's, wanted to consolidate those into one time slot. So, I'll be actually offering comments on behalf of West Associates in CV's place. The comments are what CV would say if he was here. Some prepared comments about CALPUFF and using an updated version of that model, as you heard Joe just describe, the need to do that expeditiously. So, thanks again, George, for accommodating the change in speaker.

My comments are on behalf of West Associates as Western Energy Supply and Transmission Associates on the need to adopt specific enhancements to the regulatory version of CALPUFF, as you heard Joe mention a minute ago. Some of us in the electric utility industry have concerns about the application of the model for BART determinations and reasonable progress work under the Regional Haze Rule. You've heard some of the results of -- of previous evaluations described earlier in this conference. West Associates is an association of -- of investor-owned, publically-owned and cooperative electric utilities in the western U.S. West Associates was heavily involved in studying visibility issues for several decades. For example, in the '90s, technical data and information on visibility impairment assembled by West Associate members in EPRI were used by the Grand Canyon Visibility Transport Commission in preparing its report on visibility protection. EPA heavily relied on the Commissioner's -- Commission's report in preparing the Regional Haze Rule. That rule calls for using the CALPUFF model to assess potential visibility impacts on emission controls -- of emissions controls at major point sources. Coupled with met data, the model predicts conversion of SO2 and NOx emissions to sulfate and nitrate particles that degrade visibility. Over the past, as you heard Joe mention, several investigators compelled - compared field measurements of sulfates and nitrates of corresponding values predicted by CALPUFF Version 5.8. Those studies concluded that the -- that model over-predicts particular nitrate formation by as much as a factor of 3 to 4 under wintertime conditions.

In the preamble to the 2005 BART Rule, EPA acknowledged that CALPUFF 5.8 may overestimate the amount of nitrate that is produced from NOx emissions from point sources. For example, allow me to quote from the EPA's final rule. "The simplified chemistry in the CALPUFF model tends to magnify the actual visibility effects of that source" and "we understand the concerns of commenters that the chemistry modules of the CALPUFF model are less advanced than some of the more recent atmospheric chemistry simulations. In its next review of the guideline on air quality models, EPA will evaluate these and other newer approaches."

At the 9th Modeling Conference in 2008, a study sponsored by the American Petroleum Institute, API recommended using a new chemistry module to reduce CALPUFF 5.8's over-prediction of nitrates. After an April 2010 API and EPA modeling group meeting to discuss various air quality modeling issues, CALPUFF modeler, the developer of the CALPUFF model, Joe Scire, was asked to test and evaluate the new chemistry module. Those tests and evaluations were successfully completed in November 2010, and CALPUFF Version 6.4 with the new chemistry and certain code fixes was placed in the public domain on the TRC website.

In December 2010, Joe met with federal land managers in Denver and described the evaluations he completed with CALPUFF 6.4 in comparison to 5.8. At that meeting, the federal land managers in attendance reacted positively to the improvements made to the model and offered to perform testing of the model revisions.

About a year ago on February 16, 2011, representatives from the TRC modeling group, API, and West Associates met with EPA and federal land managers. The TRC team made detailed presentations on their tests and evaluations using a demonstration of the backward compatibility of CALPUFF 6.4 compared to 5.8 with the chemistry version turned off, as you heard Joe mention a minute ago. At that time, EPA informed West

Associates and API that the proposed enhancements to the model with new chemistry will have to go through the standard rule-making process, and that could take upwards of three to four to five years. EPA also recommended that certain additional tests of CALPUFF 6.4 be made with plume measurements, and you heard Joe talk about those evaluations. Under a pending consent decree between EPA and the National Parks Conservation Association, by November of this year EPA is required to approve regional haze state implementation plans or SIPs or promulgate federal implementation plans or FIPs if the SIPs are disapproved for various states around the west. West Associates recommends and requests that EPA conduct its own test, evaluation and evaluations of the newer version of the CALPUFF model as soon as practicable before some of these final best-available retrofit technology determinations are made later this year. I understand from one of the talks yesterday that some of that work is underway. If those tests duplicate the finding that Joe described -- findings that Joe described this morning, EPA should move expeditiously to designate CALPUFF 6.42b as a regu -- as the regulatory version of the model for BART determinations, at least on a case-by-case basis. Such an approach would be consistent with the process EPA has used with recent changes to the AERMOD model and to do otherwise would mean continue -- continued use of an inaccurate regulatory tool well beyond a decade after problems with that tool are recognized. In the meantime, due to the -- due to the demonstrated superior -- superiority of the chemistry in CALPUFF 6.42b, and its more accurate predictions of particulate nitrate formation, EPA should encourage and carefully consider modeling demonstrations using this version of the model.

That concludes my comments. Thank you.

MR. GEORGE BRIDGERS: All right. Let's see if we can get this laptop fired back up. Sorry, just another -- Now we'll have comments on behalf of API by Steve Hanna.

MR. STEVE HANNA: Okay, thank you. Thank you. As George mentioned, these comments are on behalf of the API and they are a very general comprehensive set of comments because API members are affected by and have to worry about all different types of models and source scenarios and so on. So, this is a little different from most of the talks which focus in on some specific thing, and we're going to try and talk about all subjects. It's sort of an overview of modeling, and we will be providing the written comments by the end of next month. And I'd also like to say that this is a team effort with myself leading a group including Bruce Egan and Bob Paine, and this is managed by Cathy Kalisz of the American Petroleum Institute and the Chair of the Committee is Chris Rabideau and many other members of the Committee are sitting out in the audience here. So, these have been worked on by several people and probably most of the things I'm going to be saying here in the next fifteen minutes or so have been covered in the past two days or will be covered by specific talks future today, but it all comes down to with the increasingly stringent standards that you no longer can have any room for over-predictions or slight problems with the monitoring or the modeling. So, there's a -- there's a lot of concerns with the new standards that we're sure we're working with the best models and they've been thoroughly evaluated and are not over-predicting and also incorporating the monitoring as much as possible.

So, how can we continue to improve with this? This is just a lot of general things but I think it's important to set priorities that -- in the past two days people have suggested, well, it'd really be nice to do this and that and -- and the EPA is responded by, well, we'll, yes, thank you. We'll consider all of this, and we're very busy and so on. But I think there needs to be some effort, maybe including the stakeholders as well as the EPA staff and just helping to set the priorities on the things that maybe should be looked at next.

There have been comments made at each of the previous Air Modeling Conferences, back to number one probably, on -- and these comments are still appropriate about complete documentation and guidance, workshops and so on. Previously, we had encouraged the collaborations, and there has been quite a bit of that that's happened in the past three years including the NO2 PVMRM type collaborations. It still could be improved, though, obviously. One example is the low wind issues that have been brought up, and there probably needs to be a little more work together on some of those, and I have five of these sort of general slides and then I'll get into the specific technical issues and, of course, the PM 2.5 guidance. At the time we wrote this, we expected there would be the guidance but then we still encouraged having the minimum of 60 days and the API Committee has a thread going through all of these comments encouraging the use of monitoring data together with the modeling data but really not relying on 100 percent on modeling and then various issues about recognizing the accuracy limitations and using actual emissions instead of potential to emit. And there have been several recent studies that have been mentioned already that the API has sponsored like the NO2 work or evaluations, the ambient ratio method that Mark Podrez presented yesterday, the low wind speed study that Bob Paine has mentioned. These are all sponsored by API together with other groups.

So, let me get into the technical list of topics and -- broke it down into these categories, so I'll -- I have a couple of slides on each one of these categories and then some comments on each one. And starting out with AERMOD and moving up through the scale of models. There's a few slides on AERMOD, actually, and there's been much talk already about the low wind question, which is primarily been generated by the fact that we're now using anemometers with lower thresholds, so we suddenly have to model with a lot of low winds, and we're discovering that they are over-predicting and there has been suggestions made by the API UR Committee, but in listening to the discussions in the past couple of days, I was thinking that there's really maybe a fives fit -- five phase approach and some of these are hit on here, and Bob Paine has mentioned some of these already, and one is better estimating the use start, which is increasing it.

And all of these, by the way, tend to reduce the concentrations because we're seeing big factor of ten over-predictions in the -- the Oak Ridge, the Idaho Falls and some of the AGA low wind cases. Increasing the minimum Sigma V and Sigma W would help quite a bit and are justified by observations. There's that - the interpolation procedure between the pancake and the discrete plume that I think was originally just suggested by Venkatram, was why don't you try this constant and see how it works, and now it appears to be flipping over the -- to the discrete too quickly, so, just a change in that interpolation formula would help a lot. And there seems to be a problem with downwash which can, even at very low wind speeds, it's assuming there's downwash occurring. And from a physical point of view, you would think that as the wind dies, the -- the eddies behind the building are going to die also. So, those corrections we would suggest and we will include them in the detailed write-up.

The next slide I think Bob used but, you know, we usually say something like, try to model that but actually, if you looked at an hourly average, there's quite a bit of lateral dispersion going on. That's what's spreading the whole thing out laterally and that's, I guess, part of the pancake assumption. But you -- this is something that you probably could model if you use these suggestions.

The urban dispersion is another question that's coming up more and more and, currently, AERMOD relies on the PRIME just by looking around at the nearest buildings and doing downwash, but the rest of the community in other countries and agencies are using these new urban dispersion models that account for urban canopy formulas. SCIPUFF's an example of something that does that, and right -- and also right up in the -- some different offices in this building, the research division has moved working on incorporating this exact type of thing, and some new urban models that they're going to be using for Detroit and Atlanta and so on for health studies, and some collaboration between them and OAQPS, and the stakeholders could probably help to just very quickly incorporate and improve urban model.

This is a picture of Oklahoma City where there is a big field experiment done with tracers at the black -- the release was at the black dot there, and what I -- this is a model prediction we're seeing here of a Lagrangian particle model and the -- and what we're finding is important to do -- to account for is that large lateral spread you can see a very broad initial spread and you also can see it -- the plume mixing into the wakes of the nearby buildings. So, this can all be in an -- accounted for in -- with current modeling systems.

The chemistry we talked a lot about yesterday and various ways of improving the NO2 chemistry. Further testing of PVMRM, and then possibly incorporating slightly improved models like SCICHEM. Oh, and then the bottom thing about the -- the field data are really inadequate and is seen to -- one of our major recommendations is somebody "ought to support a field experiment where there are detailed samplers and we know what the emission is" and so on.

This hasn't been discussed too much at this meeting so far, but there -- the whole question of how you extrapolate an airport meteorological observation over to an actual site is an open question now, and there are some ways to do this so as you can avoid problems with the current method where if you assume a low roughness at the airport and you go over to another area with higher roughness, the current method you'd end up with higher wind speeds at stack height and really in the atmosphere over a rougher area, you would have lower wind speeds at any given height. So, that sort of goes the opposite direction. And prognostic meteorological models are thought by some to be a silver bullet here but they are -- have some errors also and should be carefully tested.

The straight line assumption we've talked about a lot and the current 50 kilometer limit -- this I can see as sort of a catch-22 because 50 kilometers is very arbitrary but if we reset it and allow you to decide on the limit based on the current wind speed, it becomes very arbitrary, and who knows what's going to happen and what do you do beyond 20 kilometers. So, there's a lot of issues with that. You could just use the Lagrangian Puff model for all distances.

The emissions question has been covered in the past two days also. The issue of using allowable emissions, which leads to over-predictions versus the actual, and yesterday Bob Paine talked about the use of the Monte Carlo random estimates of emissions more realistically covered the spread. To switch to CALPUFF, that also comes under this 50 kilometer limit and, in principle, the PUFF models ought to work just fine at shorter distances. In fact, SCIPUFF has been evaluated at using the prairie grass data and works just fine. To get somewhat -- some perfect agreement with AERMOD, some work has probably needed with CALPUFF. And there are a lot of other models in the second bullet that's mentioned that are available and used in Europe for regulatory assessments and also in the U.S.

The chemistry Joe Scire just talked about and it was covered in other talks. So, it appears definitely this chemistry's being improved and we applaud that development.

Now, moving to CMAQ and CAMx, the regional models that -- they aren't really talked about in this conference, and I've heard it said, well, they have their own meeting, like the CMAS meeting. But then after we thought about that a little bit, those are really just technical forums and you don't allow for the -- the public is not allowed at those conferences to just show up like they are here and stand up and make comments that are put into some sort of a public record. So that -- I think that ought to be -- this sort of thing ought to be expanded to include CMAQ and CAMx.

The question of Plume-in-Grid model that there -- that's also been covered extensively and it appears that SCICHEM is being considered here.

The next topic is prognostic meteorological models, and this seems to be the way everybody's going these days. It seems like a good idea, especially if you can get the grid size down but, remember, these are the models that are forecasting our weather every day, so just think about that. So that the minimum, and there have been studies done, the wind speed uncertainty is the meter per second. There's -- mixing depths are uncertain and so on. This was an example of a study I did, I'll show later, where the mixing depths in the afternoon are a little uncertain. And there's a problem with grid resolution when you have terrain around, so you have to watch out for that. So, the bottom line is you shouldn't make major changes unless this is really thoroughly looked at and evaluated.

The background concentrations is a big concern with the API, and we're recommending use of properly sited local monitors and more than one so you can really determine the upwind area. And model evaluation and databases -- I think the -- we've heard a lot about the quantile quantile and that this should be expanded. The long-range transport evaluations that were described expanded the set but, as everybody said, it could be modified a little bit and include more significance tests. There's some model acceptance criteria which the military insists that you have in your models, and that would be a lot of fun if you ever had to impose that.

So, the final recommendations that -- I've covered most of these things -- is about the ARM2 be adopted and the tier two methods improvements to PVMRM are still needed. The whole low wind speed needs to be fixed, and there's been some suggestions made, model evaluations improved, improved in monitoring systems, use of actual emissions, and then in continuing the increase of communications and collaborations. And so that's the last slide saying contact Cathy Kalisz if you want information about the API. Thank you.

MR. GEORGE BRIDGERS: Thank you, Steve, and then the final presentation before the morning break, on behalf of the AF&PA is Ryan Gesser.

MR. RYAN GESSER: Thank you. I -- having only ten minutes and wanting to keep on schedule for the break, I don't have time to deliver my opening monologue this morning. So it's in the record from yesterday, you can go back and check that. So, just suffice to say that again I appreciate the opportunity to provide comments today. I'm Ryan Gesser speaking on behalf of American Forest and Paper Association or AF&PA and, obviously, today I'm focusing on NO2 and SO2. And a similar slide I showed yesterday, in the context of discussing PM 2.5, that the industry is generally well-controlled with respect to that pollutant. I think we would make the case that it's the same for SO2 and NO2 that we're generally well-controlled in industrial operations, although obviously we are major sources. All of the regulatory programs that I alluded to yesterday that require controls for PM 2.5 also require controls for SO2 and NO2 and, likewise, there's regulations coming down the road, be it Boiler MACTs or the various regional haze programs which offer opportunities and may lead to further reductions, not just to particulate but of SO2 and NOx emissions as well.

Like many industrial sectors, pulp paper mills can find it difficult to demonstrate compliance with the applicable NAAQS following the current EPA modeling guidance. Yesterday, in the context of PM 2.5, I said it was difficult and then proceeded to paint a very bleak picture, I guess. Today I'm saying it's difficult but I'm about to tell a better story or a more optimistic story for -- for how our industries has an outlook on demonstrating compliance with these standards using the models. That said, of course, it's not absolute. There are many mills that will have challenges demonstrating compliance with these standards with the guidance and modeling tools that we have. And many of the same -- the same consequences or implications would apply where there's new projects that can't move forward until the modeling issues are resolved, and that's obviously a concern. And if we're speaking about SO2, then obviously we have to recognize the SIP development process that's underway. We have draft guidance but not final guidance at this point, so that is obviously a program that mills that aren't undertaking projects for growth will still have to evaluate their impacts under their existing operations.

So, again, we find ourselves in a situation of having to explore whether additional controls may be required. I think our outlook in most cases suggests that that's not likely to be the case. Where we do have modeling issues that need to be resolved, we're optimistic that, in most cases, they would be resolved by the on-paper reductions of on-paper permitted limits on our maximum allowable emissions that, again, we have to model under regulatory context where we might have to accept some reduced flexibilities in terms of operations, and especially fuels moving from higher sulfur fuels to lower sulfur fuels, but we're optimistic cautiously that that would -- that would be mainly on paper exercises to do that.

Yesterday, we heard the presentation from EPA's AERMOD implementation workgroup or AWIG. In that presentation, they did not call specific attention to our sector. Thank you. But we were included in the report, and so I wanted to take a moment just to acknowledge that, and we're going to talk about -- I'm about to talk about our outlook for answer to an NO2 based on some work that we've done. And it looks different than what is presented in the AWIG report, and since that did go into the record and it is part of the report, you might see I wanted to offer some additional information from that. I want to be clear that we don't have a critical review, and we're not trying to be critical of this finding necessarily because, obviously, the report just came out. We haven't fully evaluated it, and we don't fully have all of the inputs to evaluate it, but we reach a different conclusion than you would reach looking at this information. So, we did want to take the opportunity to discuss that.

Just to summarize what was reported in those findings, like you saw, they've -- the group looked at various modeling scenarios and cases for the different sectors, and ours wasn't a particularly optimistic picture. In what was called a base case, the impacts for SO2 were found to be up at a level above 900 micrograms. Of course, the standard is closer to 200 micrograms, so that's -- that's not a very optimistic picture. And then in various combinations of control strategies of stack height increases, the results improve but there weren't any scenarios that were shown to get below the NAAQS standards. So, that obviously is -- is a concerning conclusion, and it was a little bit surprising in that that's not what we were expecting, and that's not what we see when we look at our own analyses for typical mills. So, there's some of those details there. You know, if we have the opportunity to review those with the group, I think we'd be happy to do it but, you know, we're going to show some different results here. What I would just note from that report is that the emissions that were modeled totaled about 777 pounds per hour in a base case and 372 pounds per hour in a base case. And that jumped out at us right away that that seemed to be somewhat higher than what we would have expected for -- for a typical mill, but yet it still could have been legitimate. I want to make that clear that when you follow the guidance and model emissions at their full permitted allowable emission rates, you can certainly see emission rates that high if you have a permit condition that allows a very high sulfur fuel such as coal or oil in certain circumstances, and certainly that -- that does exist for some mills, but it's not typical of our -- of our typical process that burns primarily biomass, which has very low SO2 emissions, and also is sort of inherently a closed loop and tends to remove sulfur dioxide from the process just in the inherent way that the process is designed. So, again, we were a little surprised or -- that the results were so high but not surprised when we saw that the emission rates that were being modeled.

So, again, I have a number of cases here. I don't have time to go through them all, but what I would point out in summary is that the various ones we looked at had emission rates based on permits or what we think would be acceptable for permits and, in these other cases, they're closer to about a 100 pounds per hour or maybe up to 280 pounds per hour in that case. And in those cases when we modeled them, the outputs we got were -- the results at the highest fourth high level, not including a background, were closer to about a 100. Not much higher than a 100 -- a 108 in this case, a 118 in the other. And so that, obviously, is below the standard if the yellow highlighting is showing up there, I kind of displayed that as caution because in some cases the background concentration is also on that level. So, just adding a conservative background, we still might have an issue to work through.

But one other case in the middle here that did have a higher result is one where we saw emissions on the levels that were shown in the AWIG study. We still got a lower model result overall, although this obviously would have been below the standard, but this is a case where we looked at the permit limits and said, yeah, you know, these permit limits are really high. They might be based on old SIP limits or fuels that aren't burned anymore. So, when we looked at a controlled case that didn't necessarily require controls but would be more representative of emission levels at what might be considered backed or at least some kind of control, those model impacts came well below the standards. So, we saw that as a better sign.

It's worth noting, you know, these are

sources that when they were originally permitted with the three hour and twenty-four hour NAAQS, they were well less, 50 percent or less than those standards. You would now -- this one would have been two times the current 1-hour NAAQS, so this is a message we already knew that even sources that might have been considered well under the NAAQS in the past under the old standards can have a challenge with the new standards.

So, one thing that this leads us to recognize is -- is that the -- the idea of variable emissions processing, we recognize there's a number of implementation challenges but we can see how that would be useful. I'm interested in exploring it to account for that sort of fuel variability where coal or higher sulfur fuels may be fired at some limited times, but most of the time the emissions would be representative of cleaner fuels and, therefore, that distribution would be interesting to represent.

I wanted to touch briefly on NO2. This was not a subject of the AWIG study, but I thought I would mention it because we've obviously looked at it from the industry perspective and, you know, the order of emissions that we looked at these various examples are a few 100 pounds an hour, and these are major sources, combustion sources. And when we applied the current tier two methodology, if this had been done by AWIG, I wouldn't have been surprised to see a high result there. We do often see values that exceed the 1-hour NO2 standard by a factor of two or more under the tier two assumptions, but if we then go and apply the tier through approach, whether it's OLM or PVMRM, we do see results that are much lower and far below the standard. And this is largely due to accounting for the NO2 to NOx in-stack ratio that's characteristic of our sources. What stack testing we've done usually seems to come out pretty consistently at a ratio of around 2 percent and so that -- that's a very low number even if you modeled at say 5 percent conservatively, which is I think what we had in these studies. You see the results that are a lot more -- a lot lower and that we think more representative.

So, the recommendation really is just that we're encouraged to hear about the improvements and attention that these tier three NO2 models are getting. We obviously support their use and -- and to any -- any measures that can be taken to sort of streamline that process, whether that be getting approvals not at a state level and not requiring a case-by-case review in every instance. We think that would be encouraging and help facilitate this process, and as these kind of data become more readily available, I certainly expect that that process to become smoother.

So, again, just in summary of the -- the comments here, we appreciate the work that the AWIG group did, but we were a little concerned that it may have overstated our impact. So, we're happy to work to -- to refine that or address that in their report, if it's -- if they're willing to do that. And we also appreciate the efforts that we're heard about to improve the NO2 models and the other issues in model performance that have been -- gotten a lot of attention this week. So, as always, we promote reasonable implementation of the guidance and we're happy to participate in that process. So, I'll conclude there, and thank you very much.

MR. GEORGE BRIDGERS: Well, thank you Ryan and to all the presenters this morning. We've ended this first session right on time. So, at this time, let's go ahead and take a fifteen minute break and we'll be back at -- at 10:40.

(WHEREUPON, a brief recess was taken).

MR. GEORGE BRIDGERS: We're going to start back up the second session of the morning with Bob Paine from AECOM.

MR. BOB PAINE: Thank you. The first two ten minute presentations will be presentations sponsored by EPRI. Eladio Knipping and Naresh Kumar were the project administrators. And you're going to see a couple of new terms here, AERMINUTEPLUS and sharp, which we'll go into here. And this AERMINUTEPLUS sort of -- sort of brings back the memory of CTDM which was made CTDMPLUS slide some twenty-five years ago by Steve Perry. And we've augmented their AERMINUTES to make it do some hourly meteorological periods and developed a sub-hourly AERMOD run procedure which we fitted into the acronym SHARP. We selected databases for this evaluation and some test procedures and some preliminary evaluation results. So, why do we want to develop such a capability when we know that we have -- this is obviously a problem for low wind speeds because in under high wind speeds, you have, you know, fairly persistent flows and the wind doesn't fluctuate too much. But you could have a large fluctuation during the course of an hour and if you have the data, why not try to use that data? And then certainly in low winds, we -- low wind speeds -- during the course of a full hour, winds can go in several directions and resulting in multiple concentration called the lobes. This is actually courtesy of Joe Scire. Three years ago, and he presented this AERMOD depiction of a -- the meander or pancake plume in the middle of that source, and the coherent plume above that. Now, the coherent plume in many cases has a much higher concentration than the -- the meander plume and therefore it only goes in one direction. And here's a plume footprint from the Bull Run experiment with a fairly good gassy and plume shape, but if you consider this hour where we had I think at least four lobes. It didn't happen very often but it can happen. We found at Bull Run that most of the time the plumes were reasonably gassy but sometimes, you can get this, and you can't do that with a -- a model like this two slides ago. So, we developed this procedure called AERMINUTEPLUS to take the two minute averages and develop averages that are sub-hourly averages of the winds, and just if you divide two into sixty, well, you can have as few as two periods of thirty minutes long or many as thirty two minutes long, and our procedure will allow the user to take any combination in between as long as the number divides into sixty. The output files of this procedure looked like copies of hourly files but, for example, if you add two thirty minutes files, you'd have two files and the first file would be minutes zero to thirty, and the other one would be minutes thirty one to sixty for each hour of the year. And then you would run AERMOD with each one as many times as you had to get each part of the hour. Then there's a procedure. I'm going to go through these steps here. You take output from AERMINUTEPLUS. You run AERMOD multiple times for each part of the hour. Each run is the same minutes of each hour. We have a bin merge procedure that takes the output concentrations and averages them, and then we also are configuring out the process calm periods consistent with the AERMOD's approach so that if you had more than fifty percent of the periods that were calm, you'd have to make -- that hour would not be able to be used as a definitive hour for AERMOD results. The third step is to take this merge concentration file and put it into a process that we called post-one hour to get the required design concentrations. We had decided to evaluate this procedure then using evaluation databases that had tracer releases from a single stack. We wanted to avoid building downwash wash issues, and we needed obviously sub-hourly meteorological data. We wanted to have databases with significant plume meander otherwise it's not very exciting, and we had two databases we had budget for, a low level release and an elevated release. One emphasized stable conditions, the other unstable, and they were predominantly affected by light winds, and these databases were the Three Mile Island. They actually had a tracer release prior to constructing this facility back in 1971. We -- we had only five hours but they were very interesting hours of very low wind stable conditions and sometimes they had to use smoke handles to figure out which way the wind was blowing, the winds were that light. And then Bull Run and EPRI database from 1982 dominated by unstable conditions, tall stack releases. We used one hundred sixty two hours in the half of the database reserved for development. Now, the Three Mile Island only had one arc of receptors whereas Bull Run had twelve arcs from point five to fifty kilometers.

Here's an example of the Three Mile Island set up, and each of these spokes is actually a concentration in -- in every twenty degrees, and you can see that in one hour you have and -- and these rays are the wind directions through the hour and these are the magnitudes of the concentrations at various directions. Obviously, that coherent plume model wouldn't do too well in this case. We ran AERMOD in each case for five-minute averages and so far we've only chosen the five-minute periods, so there are twelve - twelve of those periods within an hour. We directed the plume -- for the 1-hour average runs, we directed the plume toward the stack with the highest concentration to give the model the best chance of succeeding.

With the sub-hourly runs, since the wind directions vary, we took them at face value. And we used a variety of tests which I won't go into detail here because of the time limitations, but I'm going to focus on the bias and -- and -- and what I call a goodness of fit like is the plume width too tight, too loose and just right?

Now, Three Mile Island, there were five databases. The red bar is the highest on the arc for observed. Blue is the hourly AERMOD and sort of like the greener mustard is the sub-hourly for five minute averages and we can see that the hourly model over-predicted in all cases. The sub-hourly model over-predicted for most cases, slightly under on the first hour. These are all stable hours, very light winds. The average over-prediction ratio for the hourly model was twelve point six for the five minute, sub-hourly model - three point seven, so the -- but the -- one -- one way to do the goodness of fit is to say, well, what percentage of the values were more than fifty percent of the peak? Observations fourteen percent. Hourly model only seven percent. The footprint is too tight, too concentrated. The five hourly sub-hourly models a little too loose, and we speculate where maybe if we went to ten minutes, we might get a better fit.

Okay, Bull Run overall results. There's a variety of arcs and we noted from previous work on convective conditions that the hourly AERMOD models does well, and we found that in fact the predicted observed ratio is a little bit higher than one overall, but it's -- it's not too bad whereas the five minutes of hourly model was a little low, but we see that also it's -- a footprint is a little loose whereas an hourly model is too tight a little bit. So, we speculated where maybe if we went to a ten minute average we might get a better fit to the sub-hourly model to the observations.

So, concluding remarks on this sub-hourly evaluation. We've developed a capability that EPRI will -- it's still evaluating, but we'll certainly consider providing to EPA. We've done a limited evaluation on two databases that featured both stable and unstable light winds. We can see for -- certainly for stable conditions, so we have another stable light wind database adding to the others ones I talked about the first day that indicates AERMOD needs some improvement. Now, one way to do it is to divide the hour up into sub-hour periods, and so this is one approach. We find that in all cases the hourly predicted plume for it is generally too tight especially in stable conditions. The five minute period may be too loose of a fit and you might -- we might try a ten minute period if we have opportunity to do that. Obviously I would recommend further testing. And that's the end of that presentation.

MR. BOB PAINE: Okay, people have always wanted to know how far is the short range model applicable, and someone came up once with a fifty kilometer distance and we're still trying to figure out where that came from. This is another EPRI funded investigation. I'm talking -- going to talk about the limitations of short range models. Can we make a puff model a plume model equivalent to do some testing on where do the models actually start to diverge and at what distance. And so we looked at both puff and -- puff trajectories and concentration comparisons for this study. Obviously, as you know, plume models assume city-state conditions, so we have a lighthouse beam effect. Every hour, the lighthouse beam shifts to a different direction. The previous hour's light beam is forgotten, and worse case conditions, and especially associated with low winds are -- are persistent or what I would say impossible distances, but impossible equals fifty kilometers at this -- at this point. This study tried to more carefully quantify what's a reasonable distance for how far these models are applicable, and I'm looking at both AERMOD and ISC is -- is good representations of a city-state plume model.

So, one way to do this is to take a non-city state model like CALPUFF and make it a equivalent to a city-state model for constant meteorological conditions, and then run a year's worth of data hour by hour and see where, at what distance do the models start to diverge in terms of their predictions. Now, we tried to make AERMOD and CALPUFF equivalent and we didn't have enough money to do it -- to make them equivalent. We -- we -- we tried to make the horizontal and vertical dispersion equivalent but AERMOD's formulations, especially for convective conditions is -- it's complicated and in its -- we just gave up in that effort.

But we had much better results between ISC and CALPUFF and since ISC and AERMOD are reasonably comparable in flat terrain, we decided to go with ISC versus CALPUFF to do this -- this test of city-state versus non-city-state models. So, we -- we found the best results between ISC and CALPUFF were for non-buoyant sources and flat terrain since there are some plume rise differences between the two models. And we ended up looking at thirty-five out of one hundred meter non-buoyant release heights as being good candidates for testing. The big difference is not exceeding a four percent and usually not two percent for distances beyond two kilometers, and we -- we will give a two kilometer applicability or more to a city-state model.

We used two met databases, one year each, and one was a - a sort of a valley orientation, the Willamette Valley for Salem, Oregon with pronounced preference for north or south winds, and then Evansville, Indiana, more of an isotropic wind regime here. This database used pre-ASOS, pre-AERMINUTE but this was fully developed with AERMINUTE 2007 where it says 1986 for Salem. First of all, we conducted a segmented plume analysis by connecting hourly time travel using ten minutes -- ten meter data from each database. After the air parcels left a one hundred kilometer by one hundred kilometer domain, we didn't further track them. We wanted to see, well, how far do these air parcels go after one, two, three, four, etcetera hours. We found that more than ninety percent of the time they don't go much more than twenty or thirty kilometers at most, often less. Even after four or five hours, fifty percent or more could still be within fifty kilometers of the release point if you would consider recirculations, and certainly they -- they can travel in non-straight trajectories. For example, Salem, Oregon, we have the twenty kilometer, the thirty kilometer radius distances. After -- after once -- after one hour, most of the air parcels didn't travel more than twenty kilometers, and these are -- since these are whole knots, you're going to see basically concentric circles whereas with the AER - AERMINUTE process, Evansville, we're going to see more of a cloud of points, but again, each -- each radius here is ten kilometers further out. Most again are within twenty kilometers, especially even within ten kilometers. Even after five hours, we see that half the -- half the -- half the puffs are still around whereas, of course, the lighthouse model, they are -- they are gone every hour. That's Salem, Oregon and this is Evansville. Again, certainly a - a - a non-city-state puff model will give you a -- more of a - an accurate depiction, and here's another way to look at it. After end, you know, one, two, three, four hours going from front to back and the probability of distance travel, after one hour for Salem, Oregon, we're seeing ten percent or less going beyond twenty or thirty kilometers, obviously growing with increased the number of hours of transport. And similarly, we see for Evansville, Indiana.

Again, so going out to fifty kilometers, hardly any air parcels get out that far, at least looking at the ten meter winds. But we're assuming in city-state models that they do.

Now we're going to the model prediction comparison part. We ran CALPUFF in ISC equivalent mode, assuming flat terrain to get the models equivalent for city-state meteorology. We used polar receptors out to fifty kilometers and looked at certain popular statistics for the daily one-hour max such as the ninety-ninth or ninety-eighth. I'm going to look at the ninety-ninth percentile. The fourth highest for some of the next few slides. Here's the pattern for Salem, Oregon for a source of the middle and the colors are such that they -- the hotter colors are higher concentrations, so you can see that the concentrations would - of course, there's -- there's -- there's a whole in the middle because that source is released at some height above the ground and maximum concentration here may be out, oh, with -- well, within ten kilometers is ten, twenty, thirty, forty and fifty kilometer rings. But you can see when we go to CALPUFF and -- and remember, this is a -- even though we're not doing terrain, this is flat terrain, the winds are influenced by the -- by the valley. And so, when we go to CALPUFF, we're going to see that the lower concentrations creep in because the winds, once they start out, the puffs once they start out, might go to the left or right after one hour of transport. And if we take the ratio of these two concentration patterns, these are the one hour, four -- highest four find that's when our max is at each receptor. The color scheme here on the ratio is such green. This green is from one to one point five. Anything that's gray or blue is below one and there's hardly any of those. So, the ISC versus CALPUFF ratio, well, that is the city-state model to the non-city-state model is showing that we're -- we're conservation, but once we get to the browns, we're -- we're in factions two over, and those start to creep in in several directions, especially along the directions toward which the winds might diverge. Certainly beyond twenty kilometers, I would say, we're getting many directions where we're starting to get factions of two relative over-prediction, model to model over-predictions.

Looking at Evansville, more of an isotropic wind pattern. This is -- this is ISC highest forth highest pattern here. Just looking at the color scheme, let me go to CALPUFF. Again, less coverage by the higher concentrations in many directions. The pattern just shrinks because the -- the controlling conditions are not persisted as far as were the city-state models. So, the ratio again, we see hardly any colors that are gray or blue. I've got a little sliver here. Again, we're seeing factor of two model to model over-predictions, ISC versus CALPUFF. It's starting to creep in. Even ten kilometers. It would certainly be on twenty kilometers, so we're seeing that the pattern is that the ratio's start to diverge, especially when you get to, I would say twenty kilometers would be opt in several directions.

So, conclusions are we saw some of the results for both of these different meteorological databases, the concentration ratios were close to one within five kilometers, within a factor of two within twenty kilometers, let's say, but beyond twenty kilometers, we saw several directions where we got ratios exceeding two. Sometimes much higher than two. And we didn't even consider terrain. This is just flat terrain but the winds are influenced by terrain for

Salem. So, I'm throwing out the idea that from this limited experiment, twenty or thirty kilometers, you know, giving -- giving some generous benefit, might be the extent to which you want to trust a single hour, you know, a city-state single hour's travel. You know, the travel doesn't go much beyond twenty or thirty kilometers in a single hour, and even after four or five hours, with a true city-state model, you get many parcels that are still within fifty kilometers.

Our results suggest that you might want to consider really twenty kilometers may be a more reasonable limit for the appropriate applicability of the city-state model. We are working on a new debugging output for AERMOD. It will tell you for each hour, each source, some useful information. The -- not only the wind direction and the winds -- the effective wind speed and the effective SIGMA W and SIGMA V, but also the partial penetration fraction, the meander fraction, the downwash fraction, but also how far does the -- this puff go, this plume go at an hour and how far is it to the receptor? And so that, that debugging information will be useful for users to determine, well, is the model really doing the right thing for this hour for this receptor, the peak receptor for each source for each hour.

So, we're looking forward to developing that and -- and -- and providing that to EPA and hopefully getting that into a future version of AERMOD. Okay, I think that's it.

MR. BOB PAINE: Okay, the last talk in my series will be a model of AOA -- should add an actually monitoring network in North Dakota. Going to talk about why we did this study, describe the database. Model results using, you know, the proposed modeling for NAAQS compliance. Results from refinements and a problem with low wind speed stable conditions. Obviously, we've talked about many issues with modeling versus monitoring, so we decided, well, let's test the model where there are monitors, and where there are multiple sources. So, a real live test of the EPA proposed procedures.

We had this opportunity because we had hourly data and many electrical generating stations. This study was, by the way, funded by Base & Electric Power Cooperative. There were two nearby sources and then there were other sources within fifty kilometers. There were five monitors and there was an on-site tower close by the sources. Here's the map showing the -- here's the distance scale here. The five monitors are these yellow diamonds. The two source groups here that were close by, and Low Valley and Great Plains Synfuels Plant. A few other EGU's, one on Coyote and the met station I think was one of these two here. Zooming out we have a few other sources. Some of these actually, I'm going to focus on later. Actually, I just -- influenced the concentrations way over here.

First run-through we used a lot of emissions of all sources, soon to be constantly emitting. We placed receptors only at the Monterey sites except for a -- some runs that characterized the spacial patterns. Now four of the five monitors were elevations sort of near the local stack base but the fifth was about a hundred meters higher, and we see that, looking at the terrain, this particular receptor here, the monitor here was about a hundred kilometers above, but most of these other ones were close to stack base, but we see the pattern of terrain, the pattern of concentration prediction there was a similar concert of high concentrations in the high terrain. That's interesting, you might say, but look at the concentrations. This particular monitor was about a hundred meters higher in elevation but the concentrations -- these are the design concentrations over a five-year period, they actually had the lowest, although within five percent. There was hardly any gradient of concentration, but the model, look what the model is doing, using liable emissions. Our -- our ratio with terrain included of the model to monitored accounts for both the -- the emission, liable emissions versus actual emissions but this terrain issue was -- was highly interesting, even though this monitor actually showed the lowest concentrations. So, we looked into that a little bit more. In fact, we just modeled it as all flat. The actual terrain slope is about two percent anyway. What we modeled was flat and low and behold, everything was more consistent. The concentrations were consistent. The model was consistent even though over-predicting. At least it was relatively consistent. So, that's one -- one, you know, one recommendation is, well, if the terrain isn't that tortuous, just model it as flat. Then we used actual out of the emissions and we got even better results for this. There's one monitor here where we think some of the -- there are -- there's one source that has a bypass stack, and sometimes that stack may have sub-hourly emission parameters that -- that may have to be tweaked to get that a little bit better, but we're seeing the combination of flat terrain and actual emissions makes a big difference in the model performance for these five monitors.

So, for all -- overall conclusions are that, well, at the -- one of the monitors, I just said the remaining over-prediction might be associated with uncertainty of exhaust parameters or in bypass stack conditions, but the model performance improves greatly with the use of actual emissions and flat terrain for the -- the slope is reasonably gentle. We have one more significant problem that we looked into, and by the way, this was, this whole study was submitted to the docket that was due December 2nd. It's also going to be in a AW May paper in this June's conference. We found that with very light winds, mechanical mixing heights can be much less than the plume height. We -we saw problematic emissions where the mechanical mixing eye was three meters. And we found some sources that were forty kilometers away or -- or contributing a large amount to the predicted impact. Because those plumes, the stack bases were low enough so that the plumes by that time being assumed to be perfectly level of model, we're hitting the terrain. The plume dispersion was very restricted for plumes above a low mechanical mixing height, and we found that the result was when the plume finally intersected the terrain forty kilometers away, it came up with a high concentration there was somewhat anomalous. Now, what we did is we did the test with a generic tall stack, modeled it with both flat terrain and a one percent slope in all directions, like a very shallow cone. We modeled an entire year in meteorology and got the peak concentration on each ring of receptors up to fifty kilometers, and I'm going to show you the plots for this -- these concentration pattern. The peak concentration is the functional distance for flat terrain and this one percent slope. This is the flat terrain case now. For an elevated stack, you would expect, yeah, the concentration peaks at a reasonably close distance of, well, a kilometer or so, and then if it, you know, tails off with distance.

Now, let's add onto that the case where we have a one percent slope and low and behold, what is this? We have a - a bump. When the plume finally hits the terrain some twenty, you know, thirty kilometers away, this is what AERMOD is doing. I don't believe that that's really happening in real life, but this is what the elo -- and we traced this to a stable nighttime hour with extremely low mixing height. So, I -- I -- I'm puzzled. Well, I'm not, maybe I'm not puzzled. I guess, I -- I think it's an issue that has to be looked into, as to do we believe that plumes go perfectly level in areas of very low sloped terrain.

And so in each of the identifying an issue that EPA needs to look into.

So, conclusions from this test are that AERMOD has an unusual prediction result, and I think the problem is caused by the very low mixing height that leads to compact plumes. Now, you can imagine that if you have low level sources that are closer to low terrain, maybe we're seeing this issue all over the place with light winds, low stacks and we're getting like a slight terrain rise for stacks that are five meters high. Same thing might be happening with all sorts of sources all over the place. When the mixing height is below building obstacles, why doesn't, you know, shouldn't -- shouldn't the mixing heights make other mixing heights respond to the fact that we have a building canopy? It seems like there's a way maybe to get the building canopy into the model.

So, then since the plume stays perfectly level, maybe we should have a policy not to consider terrain in such cases because we are getting a better result for both the monitored part of this study I looked at earlier, and we would avoid this bump at thirty kilometers that we saw in this test. That's the end of that talk.

MR. GEORGE BRIDGERS: Thank you, Bob, for all three of those presentations. Moving right along. Allen, the floor is yours.

MR. ALLEN DITTENHOEFER: Thank you, George. I'd like to acknowledge my co-author, Michael Hirtler, also of Environplan Consulting. On behalf of the coke industry, the American Coke and Coal Chemicals Institute, we prepared a brief presentation to discuss some of the technical issues and challenges in modeling a buoyant line sources. Earlier in the week, I was pleased to see that EPA has listed this modeling as one of its priorities in improvements to AERMOD. This is, you know, is of critical importance to certain industries such as the metals industry where we have a lot of buoyancy associated with heated fugitive emissions, such sources like electric arc furnaces, basic oxygen furnaces and aluminum reduction plants. You've got roof monitors, roof vents and positive pressure baghouses configured in long lines where you've got buoyancy associated with the -- with these emissions. Coke batteries also. You have not only the release or volumetric flow of hot gasses coming out of the leaks from these ovens but you also have the hot surface, which provides convective heat transfer and the applications, of course, with -- now with the 1-hour SO2 and NO2 standards, we need to more accurately model these types of sources. When you model these sources using a non-buoyant volume source configuration, you get large exceedances of the standards. Also, there are obvious PSD new review applications. Next year, EPA is going to be doing its risk and technology review for the coke industry on the pushing batter -- quenching and battery stack mack. So, we need more realistic models to conduct that modeling and so on.

Now, as you know, AERMOD does not treat buoyant line sources and previous approaches have used some type of a hybrid approach where you first estimate plume rise using a buoyant line source algorithm and most commonly the EPA BLP model has been used. But then you take those effective stack heights from the -- from the plume rise algorithm at BLP and then incorporate them into a dispersion model such as, well, back -- back in the 2005 time frame, ISC was used in the sub-part L Coke oven residual risk studies. Now, of course, we have AERMOD.

Now, what are some of the relevant features of BLP? This -- this model was actually developed thirty years ago for the aluminum industry. It treats the enhanced plume rise of buoyant line sources, and -- a line source as compared to a -- an unobstructed stack release, you have one less degree of freedom in the entrainment of ambient air, so you've got enhanced rise compared to a stack plume. You also have -- you get a plume enhancement when you have multiple line sources aligned in parallel. The plume rise is dependent on the wind direction relative to that configuration of line sources, the line source length, the number of parallel lines and their spacing. And BLP is also capable of treating vertical wind shear on plume rise and incorporation of building downwash, but one major drawback of BLP, it does not treat complex terrain. So, many of these types of facilities are located in complex terrain settings. You need to have a model, dispersion model, that can adequate treat complex terrain.

Just briefly, what -- to show that the BLP line source of algorithm, plume rise algorithm differs from stack sources. We note that in a line source, we have dependence on the distance, linear dependence whereas in a stack plume, you have a two-thirds dependence, and this -- this illustrates the enhancement that you get on a line source. A key parameter in this modeling is the buoyancy parameter. For a stack buoyancy parameters are fairly easy to define. You can do a stack test and measure the volumetric flow and the temperature, and you know pretty much have it in a -- for a line source, particularly for complex sources like coke batteries, you don't have a well-defined method of calculating the buoyancy parameter. It's a function of the dimensions of the line source and exit velocity and, of course, temperature difference. Now, for coke batteries, you have two components of this buoyancy parameter. You have a convective heat transfer component, and this is -- this is due to the heating of the ambient air surrounding the hot coke oven surfaces, and you have a myriad of surfaces. You have, of course, the top of the oven, you have the sides, and the doors and buck stays and off-tape piping that's found on top of the -- of the batteries. So you need to calculate what heat transfer is occurring off of these hot surfaces and then, secondly, there's a volumetric flow associated with the fugitive emissions, and there are a myriad of sources of fugitive emissions along a battery which can extend a hundred meters or so. Whenever you charge an oven, there are door leaks along the battery, topside leaks, every fifteen minutes or so a battery is pushed -- I mean an oven is pushed where massive hot coke is pushed out of the oven onto a quench car. You typically have a movable hood that captures most of the emissions, but when you -- and then that goes to a baghouse, but oftentimes when you undercoke the coal, you don't get a good capture. You get a lot of volatile organic compounds of particulate matter that escape the hood contributing to the volumetric flow which adds to the buoyancy. So, it's critical that we account for this and it's not always a straightforward calculation -- calculation. Like for the fugitive emission component, we often have to relate observed opacities we use in relationships between opacity and PM concentration and then when you calcu -- when -- if you have an emission factor to estimate the PM emission rate, you can back-calculate what the volumetric flow is. So, it's a -- it's not a simple, straightforward process.

Now, our proposed procedure is to apply -- it's a two-step process and then, as I said, this has been -- this two-step modeling scheme has been used for a good number of years, but now that we have AERMOD, we need to take advantage of the advanced science that AERMOD offers as opposed to ISC. So, what we've proposed is to use BLP to estimate the hourly line source final plume rise based on line source buoyancy parameters, physical dimensions and source orientation similar to what I -- to what I just described, and then take advantage of the volume source option in AERMOD where you can, on an hour-by-hour basis, use height adjustment factors where you're going to be changing your volume source release height on a hour-by-hour basis based on the predicted plume rise estimates from BLP. And we're currently in the process of testing this procedure and plan to apply it for the 1-hour SO2 modeling that's going to be done and we've also had discussions with EPA regarding its possible use in the upcoming risk and technology reviews for the sub-part C, five C residual risk modeling next year.

Just to note, so in summary, until EPA develops a buoyant line source algorithm, we've proposed this two-step modeling scheme. It can treat enhanced plume rise from multiple buoyant line sources. It's -- it is more time than resource intensive because BLP requires the use of the Ramet meteorological processor and, of course, AERMIC is used for AERMOD. It's more cumbersome and time-intensive but until a new algorithm is incorporated into AERMOD, this is probably what we'll be using. Okay. Thank you.

MR. GEORGE BRIDGERS: Thank you. And we'll just keep following right along. We have a presentation from Trinity.

MR. QIGUO JING: I am Qiguo Jing from

Breeze Trinity Consultants. I did this work with my advisor,Akula Venkatram, Marko Princevac, and David Pankratz. n order to understand the disposition of buoyancy mission from open source, we need to know the emissions for sure and some source parameters. You need geometry and urban meteorology. So, the first question is what is the significance of urban boundary layer. This picture shows the difference between rural boundary layer and urban boundary layer. As we can see here, because of the presence of buildings, the wind speed is decreased and the turbulence is increased. And because of the presence of buildings, there's an extra layer, an oven canopy layer. The dysfunction within oven canopy layer is an unsolved issue. It's quite difficult. To be straightforward, if we put stack height -- stack here, and put the same stack here, we will expect different disposition, be heavy.

So, tracer study has been done in Palm Springs in California in 2008. This is a stack, let's see, the source parameters and look and see, it's low level and buoyant. Forty-eight assemblers were arranged in-house at a distance from sixty meters to two kilometers, and we did three daytime and four nighttime releases. Meteorology's was collected from this tower. The wind speed indicates that promised wind studies a low wind case. The wind speed is below two meters per second during most of the day, and the same spot indicates the daytime is pretty corrupted. The nighttime is stable.

Despite the low wind speed, the turbulent level is pretty high and because the wind speed is low, turbulent level is high, the meandering is important. And this is proved by the spacial variation of observed concentration. Here at this negative means upwind. Five means downwind. We can see the concentration is really all over the place, especially during the nighttime.

This is another plot of spacial variation of the low concentration. We can see the daytime concentration falls rapidly -- more rapidly with radio distance being nighttime, and the nighttime concentration adds to one kilometer is significant. Next, we are using AERMOD to model the disposition of one-day emission. This is unique from horizontal diffusion in AERMOD accounts for the meandering and accounting processes are plume rise, building effects and turbulent disposition.

These are model performance results. First, we look at the daytime. AERMOD performs really well during the daytime, and it does predict the upwind concentration, which indicates including meandering, the AERMOD model is very important. However, at night, AERMOD tends to only submit to the concentration. The possible reason could be high plume rise, high mixing height of high vertical spray. So, we did some sensitivity studies on nighttime predictions. First, we switched the building downwash off and on, but we didn't see too much difference here because the average plume height is thirty-five meters above the ground. The building is only seven meters, so probably the plume rise way over the building downwash regime. We also used NWS data from airport adding impulse to AERMET. The high wind speed at airport not improve the model wind loss.

Next is the sensitivity study on mixing height. We used, as we can see here, use fixed mixing height does not improve the performance. So then we thought about the vertical spray. Probably the vertical spray is too high and this could be true because low winds may be trapped in the shallow stable bungalow layer during the night. So we modified the AERMOD according to wind catch and the past 1985 paper. In their paper, they describe the plume by two parts, the updraft and downdraft. And they say some limitation to these two parts. And we had some limitation to the overall vertical spray. After that the, you know, the (Inaudible) is reduced dramatically, and the model performance is improved.

This is my conclusion. AERMOD provides an inadequate description of concentrations associated with the point of release from the level of source during the daytime convective condition. AERMOD's own estimates concentrating during the night when conditions generated by wind shear and a simple modification can -- can improve this performance, and check out our Breeze -- you know, website. If you are interested, we are release parallel version of our newest EPA model in couple of weeks. If you are interested, you can, you know, contact us, and I leave a stack of my salesperson's business card here. Thank you.

MR. GEORGE BRIDGERS: Oh, everybody's not crazy if they're looking at the agenda. There are two five-minute talks back-to-back but Bob's going to do them combined.

MR. BOB PAINE: I have an alternative way to do the buoyant line of source modeling in AERMOD. I'm also going to talk about -- a little bit about downwash and low winds, which was brought up earlier today. As we've already heard from Allen, we need a buoyant line of source approach in AERMOD. I'm going to talk about one way to do this, and an actual -- a limited evaluation of that approach, and then a mention of downwash concentrations of low and stable conditions, how these may be affecting routine assessments for 1-hour SO2 NAAQS compliance.

The only prudent approach for buoyant line of sources is BLP, although that -- that module is in CALPUFF, and I -- I think I saw a bullet wish list that Roger may have mentioned that EPA intends to augment AERMOD to add this feature, although it would be nice to have a working solution now, and I don't know when the EPA will actually be able to implement this feature. BLP has some limitations besides that it only models 180 receptors and 10 sources, which can be easily fixed, but it has some constraints. The buildings have to be equally long, equally separated and have identical buoyancies. Often at aluminum production facilities, you have the most of the SO2 releases are actually not on these buildings, but they're in these clusters of little stacks that are -- don't -- don't match the geometry of the long buildings. So, it's -- it's not a good fit. Also, BLP uses a different dispersion procedure, a different -and so it's not a good match to -- to AERMOD. So, the alternative would be, that Allen Dittenhoefer mentioned, was well use BLP to predict the hourly plume height and use -- use AERMOD's hourly volume-source approach, which was a very good feature to put into AERMOD, Roger, for setting the hourly release heights. Now this will work with BLP if the buildings and rooftop dimensions are fairly ideal. They are all equally long, equally separated. They have identical buoyancies. What about point sources interspersed within these rooftop vents? The buoyancy isn't really well-represented. So, I'm going to describe a non-BLP approach that doesn't have this limitation. Use as recommendations communicated by John Irwin some -- eight plus years ago to an application in Region Five of -- for a, I think a slag pit and other iron steel types of applications. There was - it was used in a Lake County, Indiana SIP. We've identified this as a potential approach for a limited production facility and what it does is it uses plume rises as recommended by John Irwin for buoyant fire plumes. You can do the -- you can group your emissions into various individual volume sources that are applicable or consistent, have consistent characteristics and emission constant, you know, concentration of sources. And then the details are, I'm going to submit it to EPA, but basically, you would use volume source initial signal y's and signal z's similar to what you'd use for AERMOD and ISC. But the way we do the plume rise is we extract the debugging output from AERMOD for the -- the wind shear, and we would use that to calculate the plume rise from the Irwin-recommended procedure, get the plume height for each hour from each of these volume sources and -- and then use AERMOD's capability to do hourly varying volume-source heights to do the rest of the modeling. So, we're using all the meteorology from AERMOD, not from BLP, to do the plume rise and feeding it back into AERMOD. We don't change AERMOD. We just extract the stuff from AERMOD, use an Excel spreadsheet to do all the plume height calculations and put it back in the AERMOD as individual volume sources each hour with individual heights.

Now, again, the -- the distribution of buoyancy in many of these cases is not uniform. The emissions are not uniform. The whole area acts as an integrated large heat island and so in one case, we divided an area into four parts to define different emission clusters. Due to the evaluation on a monitor for over two years that was within a kilometer of the plant to indicate whether this approach worked, and the last two years had these observe design concentrations well below the NAAQS of one ninety six, AERMOD predictions without enhanced plume buoyancy rise was a little bit high, I would say. But with this proposed approach of almost shocking good agreement on this year and then again, equal to or over-predicting the monitor. So, we propose this approach for EPA's consideration.

The other thing I want to discuss here is, oh, okay, so this is the conclusion that we need an approach. This is -- this is a non-BLP approach because we tried to get away from the -- the source limitations that are built into BLP. We avoid these limitations and use the actual AERMOD meteorological profiles to characterize in an Excel spreadsheet the plume rise.

Okay, I'd like to also look at the fact that we've been seeing in -- for buoyant stacks that have heights close to the billing heights, not very high above the building heights, that we had large predictions of downwash under almost stable conditions. I mean, almost calm stable conditions. For example, a peak concentration with a wind speed of point three meters per second is severe downwash, I'd have to say. So, I was going to -- I looked back at the AERMOD evaluations and found for AGA, the American Gas

Association, three stable experiments with a wind speed of one point eight meters per second, and the observed concentration is in this calm, and the AERMOD concentration is this calm, so I would say in the words of my former Governor, Mitt Romney, this model is seriously conservative.

So, what can -- I -- I -- I think there's no way -- there's no way to tell AERMOD -- well, there is a way to tell AERMOD you can have a wind speed dependent source that turns on and off with wind speed but it's an issue I think that deserves seriously -- serious -- serious consideration for a seriously conservative issue.

So, one type of study that may have had this -- both of these issues affected, has been submitted or mentioned by the American Iron and Steel Institute in meeting last summer, where several sources in northwestern Indiana were modeled and there were two monitors and using just out of the box AERMOD, we found several times higher predictions and observations of two area monitors. These may have been affected by both the -- the downwash under very light winds and the lack of a buoyant volume source capability. So, I think both of these issues are important to consider in refinements to AERMOD. That's it.

MR. GEORGE BRIDGERS: Thank you, Bob, once again. The next fellow, George.

MR. GEORGE SCHEWE: Good morning. When I started with the agency back in 1975, we were using 1964 meteorological data, and we continued to use that for a number of years, gradually moving into the seventies and the eighties and the nineties, and my concern, I guess, from the transitions from the eighties to the nineties metdata, was we all complained that we had too many calms, and so over the last several years, now we're using AERMINUTE and now we don't have any calms. But we have a lot of light wind speeds.

So, my interest in taking a look at the difference between the AERMET and AERMINUTE AERMET, I'll call it, comparisons was to figure out what happened to the wind speeds. Are they lower? Are they higher? Are there a lot more of them or fewer? Oh, and I'd also like to thank my co-author, Dr. Abhishek Bhat.

So, I used the AERMINUTE 1159 version, and, oh, I'm sorry 11325 along with AERMET's 11059, and when I refer to AERMET datasets, this is what I'm referring to, datasets generated using straight NWS ISHD, 144, 3280, whatever those regular hourly regular data sets with the upper air soundings from that NOAA website. The AERMINUTE then supplemented that by including the one minute running, two minute averages that are also available from NOAA.

The datasets I looked at were for Harrisburg and Dulles, Cape Girardeau, Fargo, North Dakota, Orangeburg and Gainsville. I'd like to thank some of my co-Trinity modelers, Angie in Minneapolis. Aubrey in Florida and Ashley in Kansas City for assisting in putting together some of these datasets. Of course, they didn't have them run in both modes, so I got what they had and then I ran it in the other mode so that I could use those for my comparisons. Just as one example of what we found, this was just comparing, and I know you can't read the numbers on here, the percentages are set up the same. You have to be careful when you generate your wind roses sometimes. So, the first extent there is ten percent but even the wind directions were affected in terms of what we saw with the AERMINUTE-generated data.

This is for Cape Girardeau, and you can see that the number of events in some of the wind directions, for example, to the southwest and southeast increased as you included the AERMINUTE data and some of the calms went away. So, just as a comparison and I tried to blow this up as big as I could last night, so I think you can see this, with straight AERMET, of course, you had values in the twenty to twenty five percent range of calms which, of course, takes away a lot of the hours that you want to model.

Fargo was not as big of a deal because Fargo has a lot higher wind speeds there and you can see from the average wind speeds across each of those locations, the wind speeds other than Fargo were about a meter per second less, and then when you run the AERMINUTE data, which is all we hoped would happen when we ran the AERMINUTE data, the number of calms went away or went down significantly, I should say, by a factor of two and a factor of more than that for Cape Girardeau. It went down to two percent.

Oddly enough, the average wind speeds actually increased across the whole set of model -- averaged wind speeds, so the average wind speed across the whole time period actually increased even though the number of low wind speeds increased significantly, so it's kind of an interesting phenomenon.

So, the question was, my question was, how does this affect the modeling? And I've been asked this question quite often amongst my fellow modelers. Well, how's this going to affect our modeling? So, I took the standard set of all source dot INP sources that were part of the AERMOD test cases from the AERMOD website listed above there. I added a few more. There were no short stacks, no twenty meter, ten meter, anything in the shorter range. So, I added a few shorter stacks with some comparable type temperatures and velocities and the emission rates that would be comparable to sources that I'm looking at in the state of Ohio right now, where we're looking at small sources with large twenty-four hour and hourly emission rates and trying to compare what those results are compared to perhaps a power plant. So, we're looking at all of it. So, this was the range of sources I looked at.

I did leave out the area of polisource. You'll see in that dataset from EPA, there's an area of polisource, too, but it's -- it's kind of an odd one and didn't fit nicely within my receptor grid. My receptor fence line was set at about a hundred and seventy five meters from each of these sources. So, they all sit right on top of each other basically, but they're all modeled independently of each other.

Just some examples then of what I saw and you'll see these as the top button... Okay, this is for the sixty-five meter stack, and unfortunately, we did all of our ratios upside down. I should have put the AERMINUTE numbers on top expecting higher numbers and the AERMET numbers on the bottom but I've got the AERMINUTE in the denominator, so a value less than one means that the AERMINUTE values are higher, okay? So, just -- just for your knowledge, so then you can see for -- by averaging period across here, that typically we're seeing higher concentrations until you get out to the longer averaging periods, and that's pretty consistent on the two higher stacks that were within EPA's test cases. For the two test cases that they did not have in there, though, for the shorter stack, you can see there's quite a bit of spread first of all between the five different meteorological stations, but you can still see that there's generally at the lower averaging times you still had higher concentrations. This becomes more pronounced for area sources. If you look at my range here, I don't have anything even close to one. All of the area sources were showing concentrations that are much higher with AERMINUTE, which is probably no surprise. Oddly enough, the area circled did better than the square area that was in EPA's dataset, so that was kind of interesting phenomena there. I actually had some that were, and this just disappeared, George. Okay, I think that holds even better. So, here you can see that the factor of one, the model actually did a little better, at least I think most of the time, thought where it was doing better was in Fargo, North Dakota again, where the low wind speeds are not quite as much of an issue.

So, that was basically all I wanted to put together. Just a demonstration showing what kinds of concentrations you get. Four different kinds of source categories at different averaging times. The full paper on this will be, as Bob was mentioning, also presented at the Air and Waste Management Association and it's under review right now. Thank you.

MR. GEORGE BRIDGERS: Thanks, George. And we have one more talk before our lunch break, which looks like we're right on schedule. So, Sergio.

MR. SERGIO GUERRA: Yeah, my name is Sergio Guerra with Wenck Associates from the Minneapolis, St. Paul area, and I think it was last November, that I took a class with George Schewe and it was one of those things that I was asking a lot of questions about metdata processing, and I was asking, well, what's the affect of surface roughness and how would we account for this and that, and I remember, maybe George remembers, he said, well, there'll be a paper, you know, so over these few months, like I kind of worked on a case study and the main thing was to try to identify what parameters on the metdata will affect the AERMOD concentrations. So, as we talked in the last couple of days, the metdata is very, very essential into like what type of concentrations we find and to what kind of affects we see on -- on -- on the predicted concentrations in any kind of model, Russian model, Eularian model, Lagrangian model.

So, I divided my talk on three parts, the first part, this is going to be specifically one slice of what I've kind of looked at. The full presentation will be presented at the Air and Waste Management Association on San Antonio this June, but I want to focus also, like George did, on AERMINUTE and kind of what, what we found. So, the first part is going to be kind of a review of what we've already seen on some of the webinars of the SO2, what we talked about on Tuesday, on what is the purpose of AERMINUTE. Then the second part is going to be, what are the unintended consequences of AERMINUTE. I would say like low speed winds have really affected what we expected to -- to see, and its -- it was all done in good faith in the part of EPA's kind of data that was out there and that is used to complement some of the other affects that -- that were done because of the change in the -- in the way that the calm winds were identified.

And then the third part will be just kind of like a recommendation.

So, I did the presentation before I heard what happened the last couple of days, so I'm going to jump through some of the slides and I'll come back and forth. So, again, like this will be presented at the Air and Waste Management Association on June, but AERMINUTE. Okay, so basically what happened, I think it was in '96, the meter started to classify winds in a different way. So, there used to be it took a non-threshold for winds and all of a sudden, they decided, well, we don't really care for low winds below three knots, so then they said okay, now anything below three knots is going to be labeled as calm, and as a result, when we take that data and put it into AERMET, that data is basically lost. So, now, so we have a gap between like three knots and two knots.

So, then what happened. So, I think Roger or someone mentioned that they stumbled upon these one minute data. That is actually pretty good dataset. Well, I guess James mentioned that it had a kind of some issues that had to be resolved because of the way it's kind of a archived and everything, but I think the AERMINUTE was a very genuine effort into kind of getting back that, that chunk of data that was lost.

But, so AERMINUTE was -- it's a pre-processor of this one minute ASOS data that can complement the wind data that we have. It's non-regulatory AERMOD, so it's actually not required, and as we know like light wind conditions may be a controlling factor and this case we're adding the number of calm winds, and concentrations are not calculated for hours of calm or missing meteorological data, and the last bullet, I think, is kind of important to realize, you know, like EPA from -- I mean, on that piece -- quotation from some other presentations from EPA, well, if there was not their intent to add a level of conservatism because it basically was a -- an intent to kind of reclaim some of the data that was lost because of these meteorological kind of shift in the way they classified winds.

So, so, what happened? Well, so we did that and now we have this sonic anemometer on the left and basically, there are no moving parts. They're just pulses of sound and based on the difference of reception, you can calculate the wind direction, wind speed. But there's basically, it can identify very, very low winds. So, basically the first one is zero, and on the right we have a (inaudible) anemometer, you know, the ones that have moving parts, and of course, there's -- they're not as -- as accurate. So, anyway, so this is something that I want to focus on because basically, if your station is part of the -- one of the ice-free wind stations, it would not have any threshold. So, you, even the lowest wind that is recorded would be processed into AERMET, so this is kind of one of things that we talked about, and of course, we learned on Tuesday that there's a memo that will address this issue and will set a threshold. But then the threshold's going to be set at one knot, so again we went from three knots every -- like there was a gap from three to two. Well, now we're going down to one knot.

So, let me switch here. Like what I did in that paper that I'll be presenting this summer is like I processed metdata in different ways to identify which are the driving parameters that are driving the concentrations. So, the first situation is kind of like the control one where actually we kind of contacted the fuel staff of the National Weather Service and said, okay, where is your met station, you know, because I think Roger mentioned, somebody can look at the aerial imagery and you can think that you know where it is and then you find out that it was another building that was not the right one. So, we went to that process but anyway, what I want to focus on is on the iteration six where we ran AERMET without AERMINUTE and at iteration nine, which is the last one where we ran AERMINUTE but we ran it without the ice-free wind group. Now, by doing that, though, what we're effectively doing is setting a threshold of two knots. So, any winds below two knots are basically ignored. They're not entering to AERMET to be processed into the -- the model. So, I mean, this is the location. It was in Manhattan, Kansas. It could have been anywhere, really, like for the purposes of AERMINUTE.

Let me go back through. So, I just want to show here and this has kind of been shown before, I think George had very good slides, but AERMET does pretty good. I mean, if you don't have AERMINUTE, which is iterations fixed in the middle, you have a twenty six percent of calm wind data. So, that's about one fourth of your data that is basically ignored. But when you use AERMINUTE like in the first road. You see that you go down to only like eighty hours of missing data which is about point nine percent of -- of calm wind and point five of missing winds. So, but then what happens if you actually set a threshold at two knots, which is kind of like what you do by not specifying that your station is part of the ice-free wind group. So, if you do that, you have -- you do gain some, you do gain but you lost by the meter cannot reclassification of calm winds, and then you end up with fifteen percent calm winds. So, it's kind of an improvement, and I ran this with -- from different stack scenarios. One was like a short stack representative of like avarice engine, in just shut boiler and the third one was like a tall stack which would be indicative of like maybe a powerpoint, something like that.

So, this is what we got for the tall stack, which was the one that was most sensitive according to this one case study, and on the left you see what I called the marigold. So, that's with AERMINUTE and you, I mean, it kind of looks pretty but the bright it is, the higher the concentration. So, we see that it's almost like looking inside the stack, you know, you kind of vary like dense concentrations, not much dispersion going on. And the one on the right has no AERMINUTE, you know, so there we kind of see what we've been used to, you know, kind of like a nice pattern of kind of flowing dispersion going around. When we used the -- again on the left, we have AERMINUTE data and on the right we have the tall stack scenario run with the no-ice free wind group as a scenario, you know, where you don't specified, you have effectively a two knot threshold for the winds that you're processing.

So, in that case, we -- we still see a little bit of dispersion. It's not as nicely as the one without AERMINUTE but this is kind of like a virtual interpretation of what's going on. Now, here's really where we can kind of -- the rubber meets the road kind of deal and we talked, I mean, today and yesterday, like to various people like in the API and also the Forest Association of Paper or whatever it was. We talked about how sometimes you can affect, they can have the best type of controls and you have very difficult timing meeting these standards. So, in this case, it's a good interpretation where we identify that the method of processing is actually driving the -- the concentrations, that no matter how tall your stack is, no matter what controls you put, if your method is not processed properly, if you include those very low winds, you will have no -- no way to pass.

So, let me see if I can -- So, anyway, in this case, I can -- I just used one grant per second for the emission rate. I mean, this was just kind of like trying to identify the met parameters that were driving the concentration. So, if you use AERMINUTE to get thirteen point one on this scenario. If you don't use AERMINUTE, you get two point one and if you have a threshold of two knots, you get five point four. So, you basically on the 1-hour scenario, you have a fifty eight percent difference between using AERMINUTE and not using AERMINUTE. So, again, if you were to kind of put this into like real emissions and kind of run like even around if you're doing a tour or even the one hours or two, really, what's going to drive your results is while you process your data with AERMINUTE, is your station part of the ice-free wind group or not.

So, that's kind of what I just wanted to share today. This is kind of like nothing new. I mean, EPA really already knows about it, and that's why it's been addressed in this upcoming memo. And, well, it is a problem. We've seen it in Minnesota with a lot of facilities. We've had a lot of trouble in helping getting compliance. I think Tyler mentioned yesterday, well, there's been like, what was it, twenty-seven projects that have met compliance. I think that was with the -- with the NO2 standard. But what we don't care about is like all the others that are still kind of tied up in this process trying to figure out how to make their emissions be controlled, how to kind of bring about a run that will actually be a passing run. So, that's kind of one of the things that -- that we cannot -- don't have much data about but, I mean, they might be some pretty excited that maybe didn't happen because of this. So, it's obviously something that we have to address, so how can be address it? Well, I think that what -- like we've talked about for the last couple of days. If we had a first one two knots and we were comfortable with that and I think that it should be kept at two knots, and then once the adjustments on AERMOD are made like we talked about -- I think Bob Paine mentioned about the use star and also the lateral and the horizontal dispersion. Once those effects have been properly validated and -- and -- and incorporated into AERMOD, then it would make more sense to kind of go down and -- and -- and take advantage of these entities out there. But I think that we need to kind of rethink our -- our threshold of one knot and keep it at two knots, and again it does kind of still give you like a more conservative answer but it gives us a little bit of time to figure out what's going on and -- and we can try to kind of fix those issues that we're seeing on the over-prediction of AERMOD with low winds.

So, that's my talk. Thank you.

MR. GEORGE BRIDGERS: Well, outstanding.

We have the morning in the books now. We will take our lunch break from noon until one.

(WHEREAS, the conference concluded for lunch).

MR. BRIDGERS: Okay. I think we've reached the one o'clock hour, so - oh and I've got to get my tie back out. We've reached the Ides of our public session on the Ides of March so we're going to start the afternoon session with Dana Wood and on behalf of BP. Okay.

MS. WOOD: Thank you for this time to comment. I'm Dana Wood with BP and I'd like to start out with thanking Doug Blewitt with AQRM for all of his assistance and help with this presentation. Today I'm going to be asking the question: Are EPA regulatory models capable of providing accurate estimates of future air-quality emissions, and I'm not going to answer this question, but I want each of you to draw your own conclusions.

Essentially, with a background of 30 parts per billion, you're left with 70 parts per billion for all new and existing sources. You know, if the west and if the model accuracy is about plus or minus a factor of two, compliance with the standard can range from 35 to 140 parts per billion, meaning that several well-controlled sources could pass while very uncontrolled sources I mean could fail, while very poorly controlled sources could pass.

AERMOD is very much skewed to over-prediction. You know this analysis kind of ignores the form of the standard, but when it's considered I don't think there's going to be any difference anyway.

One thing I do want to note is that NO2 background is not really a measure of NO2 but N0Y, because the measurements pick up ammonia as NO2 and even with this inflation of the NO2 numbers, we're not really seeing violations of the NO2 standard anywhere in the country right now. I think this further shows the exaggeration or over-prediction of these models.

I'd like EPA to kind of look at model evaluation with the American Gas Association database for natural gas fired engines. This is the source that's of biggest concern to us because you've got short stack heights and you're right next to a fence line or, quote, fence line. We don't really have fences. And you know so you're - it's very difficult to meet the standard and these are where we're having more - a lot of the problems. Of course EPA doesn't see this because most of our sources are minor sources and going - you know dealing with the states directly on this problem.

This database has used experiments using Sf6 tracer gas to help to determine what the emission rates are, so it helps make the emission rates more known as opposed to say Empire ALBO or the Apollo Hawaii databases that have no real emissions data. It also had multiple downwind sampling points as opposed to one or two monitors like what was used in Empire and Apollo. This is a pretty high-quality database for evaluation of total NAAQS and if we do any other future field studies you know, I would recommend that it be something similar to this method that was used.

This is just a Q-Q plot of a linear Q-Q plot of all the data in the AGA database parsed by stability classes, observed as on the X-axis, YY is the AERMOD and if you notice there is a bunch of stars that are black on the Y-axis. These are the stability Class F and it shows the AERMOD predicted 225 micrograms per cubic meter while there were absolutely no observed NO2 measured. You know there's a multitude of reasons that this could be showing this, but I think the model is showing downwash, but, where there is none.

The red squares that you see are for stability Class C and you can see AERMODs primarily under-predicting these sources.

This is a graph of the stability classes that are broken out by over-prediction and under-prediction by AERMOD with over-prediction greater than 2: over-prediction less than 2 and then under-prediction less than and more than a factor of 2.

So what are the implications of looking at the AGA database for model evaluation? Well, first it shows 60-percent of the time the model over-predicts by more than a factor of 2 and 35-percent of the time the model under-predicts by more than a factor of 2. With this kind of uncertainty, are the models accurate enough? The likelihood of getting anywhere close to right is very minimal and well-controlled sources will probably fail under this scenario.

The other issue is with - the wind speeds tested were 1.3 meters per second and EPA is now claiming that the accuracy of the model is valid up to 2 meters per second with you know, no backup data to support this. Industry provided model improvements three years ago for light wind speeds. It's been talked about repeatedly here and still this has not been incorporated into the model.

Also, AERMOD is a study state model that assumes a plume goes to infinity instantly. These A-physical results should be allowed to be excluded in your analysis. These A-physical results can easily be removed if it exceeds the meteorological persistence or actual trajectory of the plume not to arrive at a receptor. You know you could always use CALPUFF as an alternative for this to address these A-physical results.

Now, I'll switch kind of over to AQRV analysis. CALPUFF and industry has spent a considerable amount of money over the past several years to comment and provide a new chemistry mechanism and as of yet, EPA hasn't incorporated this.

It can be very difficult from our perspective to use best science or better science in a regulatory setting. We've spent a year and a half waiting to complete an EIS while Wyoming DEQ, BLM and EPA debated whether we could use CALPUFF or CAMx for doing our AQRB analysis. Meanwhile, you know - meanwhile, we can't develop. Even with CAMx there is considerable over-prediction and the models really need to be used in a relative mode.

So, in conclusion, I think everyone in this room feels that models must continue to be improved to better reflect reality. This is going to require peer review of EPAs work and better collaboration between the whole modeling community and EPA. It would be great if from this conference, EPA would publish a work plan for public comment and input so that we can all you know just look at how to address these problems.

Thank you for your time.

MR. BRIDGERS: Thank you, Dana. And we can get rid of that one. The next presentation is not by Doug, it is by Nicole.

MS. DOWNEY: Hello, my name is Nicole Downey. I'll be presenting on behalf of Doug who is sorry he can't be here: he got the flu. And, I'll be talking about issues associated with NO2 model evaluations, specifically the Empire ALBO dataset.

As a little background to this, Doug was involved in the original data collection as were several other people who have been in this conference. We're working from a conceptual NO2 plume model where you have this core of NO. This is basically like the PVMRM or the ozone limiting method where you have an No core and the ozone is mixed in on the sides and then you react to NO2, but the complete version to NO2 obviously takes some time and that's going to depend on the meteorological variability, the rate of reaction and your ozone concentration.

With AERMOD, the NO2 is calculated, its parameter based on the NAAQS concentration and you have both the issue of dispersion as well as the conversion between NO2 or NO to NO2 that you have to keep in mind when you're making this conversion to No2. So there are a couple of processes going on.

And, in this last point, the NO2 model performance, it can be better but it probably shouldn't be if you think about how it's actually a parameterization of NAAQS, and so I'll show you an example of how this parameterization can lead to perhaps overestimates in the conversion rate between No and No2 because you're under-predicting NAAQS and then you end up over-predicting NO2.

So, first I'll give you know, we saw from Roger's presentation that they - you know the empire ALBO data that was used to evaluate AERMOD, it's been one of the primary datasets used along with the whole Hawaii dataset and it was an Amoco gas plant data that was collected and I have these dates wrong: it was 1992 and 1993, over two years, and it was designed to develop a database for performing OLM calculations to demonstrate compliance.

There was an ozone monitor upwind of the plume and there was an ozone monitor downwind of the plume and then there were two NAAQS boxes there as well and then there are also - it's not an isolated source - there are more regional impacts and I think it's important to realize this study was not designed with this in mind, because there's no actual emissions data. No one ever measured the emissions coming out of the plant during this time. This was an exercise to get a permit. This was not actual emissions data used and as we all know, you don't permit with actual emissions, you permit with maximum emissions.

So there are at least three different emission inventories for this plant. There's the 2600 tons per year based on historical plant operating capacity, a compliance inventory of 1800 tons per year and then in 1995, inventory based on a compliance strategy of about 1500 tons per year. And so there's quite a bit of variability in what could be used in an evaluation and it's not clear what was used and I think some of this gets at the fact that all of these analysis should be publicly available and the data use should be accurately described so that you can recreate what's done.

And, basically the idea from this slide to take away is although we have these emissions inventories that doesn't mean that over this time period that the emissions were close to this. There are a lot of things that can happen operationally that make you drift from your permitted levels. And, then, there are a couple of other NO2 databases: we've heard about the Palaau, Hawaii: it's an oil fired turbine. There's not a lot known. We heard that the instrument was very close to the source. The Wainwright, Alaska has also the oil fired electric generators and here you're almost in a different ozone regime. You're up in Alaska. You've got nothing going on, right? I mean is it reasonable to use something that's not even close to a normal chemical regime that you would see in the United States?

And as we've all talked about, we need to get out there and collect some data and there needs to be some real data where you measure real emissions and you measure No and NO2 and that's the way to solve this problem.

Doug, here, is making the argument that if you do the typical Q-Q plots unpaired in time, unpaired in space, you can mask some compensating errors because it looks like you're making good predictions but in the end, actually, you may be not, and so he suggests that you use you know, the NAAQS NAAQS- the typical Q-Q plot to evaluate total dispersion. The NO2 model predictions compared to the observations where you've kept the right NAAQS pairing so you're - you're evaluating the total dispersion and you're matching the chemistry at the same time. And then, you're No2 models compared to NAAQS observations unpaired in time is you know sort of three ways to look at this problem.

And, so, I'll show you sort of what the effects of these can be. So the first plot is just the NAAQS, NAAQS plot and this is using the 1995 emissions inventory for Empire ALBO and also other regional sources and you can see that at the high end of the spectrum, AERMOD is under-predicting the concentration of NAAQS relative to the monitor. Okay.

Then in the next slide we show NO2 monitored and No2 modeled where these are still paired with the NAAQS measurements, so this isn't just a flat out Q-Q plot. We're actually keeping some of the temporal characteristics here. And you can see that in many cases there's more than a factor of 2 over-prediction in the No2 that's modeled.

Now, if you remember that initially you have an under-prediction in NAAQS and now you'll have an over-prediction in NO2, this is an issue because the conversion rate that you calculate from a typical Q-Q plot will lead to more No2 production from NAAQS - from No.

And so here's the typical - this is just NO2 versus No2 unpaired and it falls within 2 - the factor of 2 bios line, but again this is just a flat out Q-Q plot and we're not taking into account any of these other errors and I think this is a good example of how you need to be very careful in how you look at the data to be sure that you're not masking any compensating errors.

So in spite of the uncertainties in the emission data, the NAAQS model performance is under-estimated by almost a factor of 2 and if you keep the NAAQS pairing and evaluate NO2 that way, the No2 model performance is overstated by more than a factor of 2. And that implies that matching No2 conversion with NAAQS dispersion can overstate the No2 formation, so basically, we just have the reactions going a little too fast.

If it's compared to the monitoring data independent of time, you basically get this over-prediction of a factor of 2 at the high end and I think I would like to stress on behalf of Doug that it would be really nice if all of this were publicly available and laid out so that everyone could evaluate the database and see what's going on.

And why is this important? I think this is something that we've seen before: This is a plot of the observed versus AERMOD from the AGA database and there are different stability classes plotted here so for instance the "F" stability class where you have various stable conditions, you're getting something like oh I don't know, an infinite amount higher. I mean it's very high compared to the observations. It's hard to - 200 divided by one, I guess it's not that high, but - so you know, it's important to identify these things because you can mask a lot in a Q-Q plot and it's important to look at this stuff critically and to be sure you're doing the right thing and you are actually - when you're parameterizing these things, a lot of stuff - slough can be taken up by a parameterization, so you need to be sure that you minimize that slough before you make the parameterization.

So some conclusions and recommendations that the code review of PVMRM has some formulation problems I guess from the Steve Hanna presentation. I think the conversion of No to NO2 overstates the No2 conversion in this analysis. I think one other thing that I forgot to mention is that there were these two ozone monitors: one upwind and one downwind, and I think

Roger showed he tested both of those and it did make a difference depending on which one you use, because the downwind monitor was there to observe scavenging and if you use that, then you'll get again, a different answer because you have lower ozone in order to mix into your plume and so it's very important that you understand you know what the data - how they were originally collected and to keep the people - I think this community seems lucky in the fact that a lot of the people that collected all the data are still in the community and you can ask them. That you know this is important you know, you need to be sure that the study is well laid out and well described and you're using the most appropriate data.

I think everyone agrees there's an urgent need to move forward on this NO2 issue and until refined techniques occur, we should find some way to get our arm into the guidance.

I think that's it.

MR. BRIDGERS: Thank you, Nicole. And we wish Doug all the best in recovering from the flu.

Okay. Well, the next presentation - we're actually - I'm going to just - I'm going to put a backdrop up for you, Cindy. There you go. We have a presentation by UARG... I'll just let you introduce yourself.

MS. LANGWORTHY: I am Cindy Langworthy and I'm speaking on behalf of UARG, which is the Utility Air Regulatory Group and I'm going to be talking more policy so no slides.

Results from EPA's suite of models and related guidance play a key role in how UARG members operate their existing sources and numerous plans to build new sources. The more EPA tightens its air-quality standards, the greater is the need for accurate, unbiased EPA preferred models and reasonable modeling approaches.

When models are called upon to play such an important role in clean air act implementation, that puts a premium on their producing accurate predictions.

Forcing regulators and regulated entities to use models that over-predict source impacts by potentially significant amounts create serious problems for and imposes unnecessary costs on industrial sources, the states in which the sources operate and the individuals who are their customers.

Today, I'm going to summarize a few of UARG's concerns on modeling related issues and UARG will provide additional information on these issues and others in written comments.

First, it is clear that EPA states and industries must rely on models for implementing some provisions of the Clean Air Act. For that reason, it is important that the underlying regulatory models and related modeling tools and any changes to these regulatory models and modeling tools be fully tested and evaluated before EPA accepts their use for regulatory purpose. Not only must such evaluation be done, but it must also be done consistently with how the model is used. For example: if a model's performance is evaluated with actual emissions, then for regulatory purposes, the model should run with actual emissions rather than with combinations of inputs that have not actually occurred and are unlikely to occur in the real world. Similarly, if background concentrations will be accounted for when a model is used in a regulatory context, then background concentrations should also be taken into account when the model is evaluated and monitored air-quality values should be the benchmark against which models are evaluated. Concentrations measured by an EPA-approved monitor at an EPA-approved site should be relied upon and preference to concentrations predicted by a model, even one that had been appropriately evaluated and designated for regulatory room - regulatory use.

Next, we want to touch on the process that EPA follows before making changes to the models, that the agency has approved regulatory use. To be clear, UARG members want to see improvements made expeditiously to EPA's guideline models, but such improvements should be carried out under the framework provided by the Clean Air Act.

Recall that EPA's modeling guideline is a regulation, therefore, EPA does not have authority to make significant changes to the guideline and the models it references without first giving the public a meaningful opportunity to comment. What this means is that the finalization of modifications to EPA's models must be preceded by the agency first proving the public with documentation on how well the modified models will perform and also giving the public a meaningful opportunity to comment on the proposed changes and related documentation.

There have been several recent instances in which such procedures have not been followed, for example: EPA added AERMET to AERMOD through a modeled change bulletin. EPA provided no opportunity for public comment before adapting the - adopting the change. EPA did not provide any information on the model's performance when AERMET is used. And AERMET, apparently, as we've seen, exacerbates an over-prediction problem at low wind speeds. EPA also did not provide notice and a chance for public input before it modified how the GEP stack height limits are to be modeled within AERMOD.

Comments over the past couple of days suggest that EPA intends to continue making modifications without following the procedures, which the Clean Air Act requires to be followed. In particular, EPA has suggested that it distinguishes between changes to the codes of the preferred models, apparently, and changes to tools used in conjunction with those models to characterize source emissions through meteorology. EPA apparently plans to allow prior public comment on the former, consistent with the Clean Air Act but not the latter.

UARG strongly disagrees with the distinction EPA appears to be making. First, it is unclear where EPA is going to draw the line between which changes will trigger the need for public notice and comment and which will not. Changes to modeling tools can affect the modeling results as much as - or in some cases more than - changes to the model code itself. Any change that alters the modeling results should be made only after the agency has applied evidence concerning model performance in light of the plan changes and has provided the public with a meaningful opportunity to comment.

And let me say that we're hearing you know three to five years to do rule making. EPA has demonstrated in other instances the capability of moving much more quickly than that, particularly when it's supplied information to go through proposal and to a final rule.

So UARG also requests that EPA simplify, expedite and make more transparent the procedures followed when those outside EPA suggest changes they believe should be made to EPA developed models. For example: the tendency in AERMOD to over-predict concentrations in low wind speed conditions - we've heard a lot about it - it's well-recognized.

Although stakeholders have suggested improvements to AERMOD to address this problem, EPA has delayed consideration of - or some would say ignored - those suggestions, thereby inhibiting substantial progress toward addressing this issue with AERMOD.

And then we know about the AECOM solution that's been proposed, but EPA has taken no action on the suggested fix. EPA needs to take action now on this issue and it needs to put in place a better program for considering on a timely basis any additional outside suggestions for improving AERMOD and EPA developed models.

Over the past couple of days, it has been heartening to hear that EPA wants to work more closely with a broader modeling community, including industry, to address these kinds of modeling issues. It has been far less heartening though to hear EPA's list of obstacles that are in the path of greater cooperation. One such obstacle - about which we have heard a lot - is the lack of sufficient agency resources, which EPA says means that the agency will address only those modeling issues of higher priority. If EPA is truly interested in working with industry, we urge the agency to take industry's concerns into account when determining what issues are of high priority. If a modeling issue is of high priority for industry and if EPA is willing to work with industry on it, then industry will devote resources to addressing that issue. This has been the case with the industry support for development of a fix to improve AERMOD's performance during low wind speeds.

If EPA is unwilling to take time to review and consider such work on issues of importance to industry though, then industry may be less interested in using its resources toward working cooperatively with EPA on this type of issue in the future.

AERMOD of course was developed with EPA's support. EPA seems even more reluctant to consider implement changes to models that have not been developed by the agency.

CALPUFF, for example, is a preferred guideline model that was not developed by EPA. There is general agreement on inadequacies in how CALPUFF treats the formation of nitrate and sulfate. This leads to inaccurate assessments of the contribution of sources of So2 and NAAQS to visibility impairment.

For industry, this CALPUFF inadequacy was a high enough priority issue that West Associates and EPRI have helped fund efforts to develop and evaluate improvements to CALPUFFS industry modules.

When information on these improvements was shared with EPA, however, EPA indicated that it would be several years before the improvements could be considered for conclusion in the regulatory version of the model.

It is neither reasonable nor appropriate for EPA to allow such lengthy delays to occur before it considers where appropriate implements improvements to models used in regulatory decision-making.

Finally, because of the importance of the issues raised at this and other modeling conferences and the speed at which technology is advancing, UARG urges that such conferences be held more often and UARG urges that other EPA sponsored events on modeling issues: for example, the yearly meeting with modelers with state and local agencies, be expanded to allow consistent meaningful participation by all stakeholders. And I know that you've said not this year and maybe in the future. We urge you to do it.

In addition: In order to assure the public will have real opportunities for input on modeling issues at such conferences and meetings, EPA should make sure that key document on modeling policy are released well ahead of time. For example: We wish that EPA had been able to make the PM 2.5 modeling guidance available prior to this conference.

In closing, UARG urges EPA to become more agile and open in its approaches for improving models and modeling tools. Industry and those who work with it possess demonstrated expertise on air quality modeling issues and are eager to work with EPA and other stakeholders to ensure the best modeling tools and procedures are used in all regulatory proceedings. EPA will benefit from such a partnership by taking advantage of the resources that industry is willing to put forward to address key issues. The agency can both gain cutting-edge models and simultaneously gain the practical insights of stakeholders.

UARG appreciates the opportunity to speak to you today.

Thank you.

MR. BRIDGERS: Thank you, Cindy, for those comments.

All right. Let me make sure we have the right one: we good? Okay. So up next is Mark and I'll let you introduce yourself.

MR. BENNETT: Thanks, George. Yes, I'm - for the record, I'm Mark Bennett with CH2M Hill, but I'm giving this presentation on behalf of Rio Tinto. And before I proceed with my very short presentation - it's only seven slides - much earlier this morning, but not that early, I got my final presentation into George and so I had whatever; two, three minutes to reflect on what's happened over the last couple of days, so I wanted to get wind of those thoughts on the record to all of you and I was pleased to see that the three previous speakers I think are on exactly the same page: That most of us here and those over the past couple of days are very detail oriented, very technically oriented and we've heard again and again in the last two days that we have to get the details right and believe me, as somebody with a PhD, I understand that. With that said, I'm hoping that we don't lose sight of the forest for the trees, that we keep the big picture in mind and, frankly, I believe that we're all on the same page here - that we all have a common goal of making sure that the regulated community can do their jobs in a manner that's protective of public health.

Now, you know, in the past it may have appeared that those two groups were in somewhat of an adversarial relationship, but even with our spirited discussions today, I certainly believe that we are all on the same page so we all want the same thing here and you know, I have many different clients in my role as a consultant, whether it's the mining industry, steel, auto manufacturing or even some of my clients are with the Department of Defense and their job is being prepared to defend our country. They all want to do their job, they all want to do it well and they all want to be protective of the public health.

So, that, I think is our common goal here and so with that, I think that we need to - it's always been true, but certainly never more true given the current economic situation, that we need to look for practical and pragmatic solutions and again, I'm somebody who took seven years to finish my PhD looking for all the right details, all the perfect solutions - we can't wait that long I don't think. I think we need to find practical and pragmatic solutions maybe step-wise solutions to come up with as the previous speaker said much more eloquently than I, you know, three years has severe -- severely negative economic impacts to our clients that are in the manufacturing industry here in the country.

So with that, let me go ahead, get off my soapbox and talk for Rio Tinto. Again, as we've all said, that the drivers now - it's really causing all of us here in the regulated community to be of concern are the new standards. This certainly requires the performance of AERMOD to be refinements to be expedited and address known deficiencies. The specific issues that are of particular concern to Rio Tinto are of no surprise: the low wind speed that we've heard again and again about: the air minute and how that exacerbates that, the buoyant line sources, pit retention is one in particular we haven't heard about, but is of particular concern to Rio Tinto and then I'm not going to have another slide on it, but again the impacts of frequent updates to AERMOD, delaying projects which again has real world impacts and certainly I nor anybody else is advocating, not fixing, any knows deficiencies, but I think if we work in a collaborative spirit maybe those fixes can come in bigger blocks with longer time periods between the fixes.

Okay. So low wind speed: we've heard about it again and again, so I'm not going to belabor all the points. The main one being though for the mining industry that for mining operation emissions from the haul roads, which are near ground level sources, are frequently the highest model impact sources and can be the primary driver for approval for a site. And we're referring again back to Bob Paine's proposed fix, from AECOM. Maybe that's not the perfect fix - it doesn't fix everything, but it certainly seems like a practical step-wide improvement that could be implemented fairly quickly. And again, we've heard a lot about the ASOS and AERMINUTE, that this is just exacerbating the over-prediction of those types of sources, so again, I really think - and again I was heartened to hear again and again from industries, so I'm hoping that industry's voice is being heard loud and clear by EPA: That this needs to be fixed and maybe you know "fixed" is the wrong word. I would say needs to be refined so that answers are better - still productive of public health but not over-predicting and not having negative economic impacts.

We've heard about AERLINE coming along and this is one way I wanted to mention again you've heard several times about buoyant line sources, so again, haul roads - right now the trucks - the emissions from the trucks, in addition to the fugitive mission from the dust that they're kicking up, we have the NAAQS in particular with the new standard - is going to be difficult to address and if we don't have a buoyant line source approach - and we've heard some over the past few days - some possible approaches; that's going to again exacerbate the problem that we have a neutrally buoyant line source near the ground, low wind speed conditions, the new standard, all the things we're hearing again and again and again. So there are other models out there that can deal with buoyant line sources. We need to have a practical solution to this, again, maybe a step-wide approach expeditiously.

So I'll spend - I'm ahead of time so I'll spend a little more time on this one since we haven't heard about this before and I'm not going to raise a huge issue about it, but I just want to make sure that EPA is aware of this. Now, this is the pit retention algorithms, which were added into EPA's ISC model back in 1995. It has to do with estimating impacts for particular emissions originating below grade for open pits. Now, of course, I may have missed it, but I didn't see anything in the AERMOD model about it and the ISC Users Manuel, I mean the Manual - it he ISC Users Manual it says - and hopefully I've quoted this correctly: "Pit retention and wet deposition algorithms have not undergone extensive evaluation at this time and their use is optional." These - if you look back to where this came from, the open pit algorithms are derived from a limited set of wind tunnel studies. They came up with some proportionality constants, which again may not be applicable in all cases as is - and I'm going back to the details I suppose in this case - it's being characterized by a rectangular shape with an aspect ratio of 10-to-1.

Now, when you scale up from the wind tunnel studies to the actual pits or the theoretical pits that are being representative, they have a disorder of size of you know, 200-some meters by 400-some meters by about 45 meters deep.

The challenge that we have at Rio Tinto at their Bingham Canyon Mine in Utah is that it's several you know - they keep digging so it keeps getting bigger, but it was about two and a half miles wide by two and a half miles wide by more than a half a mile deep and we felt that the use of this algorithm was inappropriate and since there was no guidance at all in the document, we had possibly some disagreements over that issue - let's just put it that way.

Now, something very interesting and we can certainly provide it to you is that all the pits in the wind tunnel studies were symmetric in shape and this led to there being large recirculation in those pits and the algorithms that were implemented into ISC uses reduced pit area and have the emissions coming out of the upwind side of the pit as you would imagine from large recirculation.

At the University of Utah, several years ago - and we've done some more recent studies too - they did some computational fluid dynamic studies with a discretization of a real pit, they didn't see any of this large recirculation at all and they were like, "Hmm, I wonder why that was?" Well, then they went and made it into a regularly shaped pit, one was trapezoid and one was rectangular of the same - roughly the same size and dimensions and depth, and they saw these large re-circulations and this is one of those moments where those of us who spent too many years getting out PhD goes, "Oh my goodness, here's something that's actual application of what I learned back then which I studied bio-variation analysis." And I won't bore you with all the details, but basically, as you break symmetries, various periodic behaviors go away, so this was no surprise to me, but I think that it calls into question these algorithms, because unless you have a real world pit that's very symmetric, it's not going to see these large re-circulations.

And I guess my time is up. This is just a figure from that study to show you the size of the pit.

And then in conclusion, I was just going to say again, as EPA prepares the new NAAQS, I hope we have continued coordination. Rio Tinto was very happy that - we've heard over the past few days and over the past years, we've heard about increased collaboration between EPA and the regulative community. We believe, as several speakers have said, that EPA has a responsibility to provide tools that accurately evaluate the impacts in order to avoid unnecessary barriers to beneficial economic growth. Rio Tinto has discussed these concerns with their fellow National Mining Associations member companies, which may or may not be providing their own written comments during the public comment period, but Rio Tinto definitely will.

So, thank you very much.

MR. BRIDGERS: Thank you, Mark, for the comments and for the presentation. All right. We'll shift gears here to a TVA talk by Stephen Mueller.

MR. MUELLER: I'm Steve Mueller and I work with the Tennessee Valley Authority and my presentation today - my comments are regarding modeling for the one-hour So2 standard and the question that I wanted to address initially was whether the tempo resolution of the meteorological information that goes into modeling, such as AERMOD, using AERMOD, affects the outcomes for one-hour So2 levels that are simulated.

The objective of what I did was to compare the performance of AERMOD running both and using a standard hourly modeling approach as well as sub-hourly versions based on the work that Bob Paine described earlier today with the air minute plus and the sharp software. I've compared those results against some hourly observations that were available around a point source.

This summarizes the modeling. I won't go into the details but again, as I said, we used the AECOM sub-hourly data processing software and essentially modeled the meteorology using 10-minute time steps so six 10-minute periods per hour. We modeled the 800 megawatt coal fired power station and used actual emissions. The modeling was performed for a period of four years. I had to drop a year because there were no ground observations to compare the results with. We used standard National Weather service: a 1-minute surface data and didn't have to do any downwash calculations because it was an uncontrolled source that was modeled. The - this plot here shows the location of the model source, which is the green circle located toward the upper center of the plot. Near the number one: the one, two, three and four represent the So2 monitors: they were located within 50 kilometers of the source. There was another monitor that was located toward the southwest: that was on Mountaintop. We used the data from that site to represent background conditions. There was also winds measure on the summit of the mountain or above the summit of the mountain I used to help me decide when these ground level SO2 monitors were essentially downwind or with a plus or minus 30 degrees of a projected plume from the source - the model source.

There's another source in this plot, which is a purple source up near three and four and that source was not models and that did affect some of the comparisons with observations, which I'll show you here in a minute.

The concentrations that were compared with my results were background corrected.

Okay. These plots - the spatial plot shows the distribution of the concentrations that were averaged over the four years that we simulated. It's a 99-percent total daily maximum of one-hour So2 values, and you can see that the left plot represents the standard hourly meteorology in AERMOD. The right plot represents the sub-hourly meteorology and there's very little difference and in this particular case it's very hard to point out or to see where the differences are. There are a number of ridges that run roughly west, southwest through northeast and throughout this model domain. The highest concentrations tend to occur - that is the oranges, the reds and the magenta colors: tend to occur on the tops of those elevated topography.

Okay. In this plot, the left plot - the left graph shows a comparison between the annual 99-percentile value or average value at the monitoring sites versus the observed 99-percentile values for all four monitors.

Now, site four, which is the purple and it's the point -represented by the points that are farthest below the line, that site is essentially often downwind of the non-model source. And I believe the disagreement here that has affected the models significantly underestimated those concentrations due to the fact that that source was not modeled. So we compare the model results with sites one, two and three: we see that there's generally - the results are either on the line or substantially above the line, in essence representing an average overestimate of about 80-percent of the 99-percentile - the annual 99-percentile values in this - for this particular source.

The highest point was almost affect free over-predicted by the model.

On the right, I compared the modeling by using the sub-hourly meteorology and AERMOD versus the hourly and as you can see, there's not much difference.

So although there was some indication before we did this work that the sub-hourly modeling could provide us some benefits for some of these situations, in this particular source or this original source and this particular set of meteorology, that was not the case. Clearly, the sub-hourly modeling, although it may be beneficial in some cases, is not a panacea.

This next plot compares the hourly 99-percentile concentrations simulated for all the different receptors in the modeling of the bank, as a function of downwind distance.

And the left plot is the one-hour modeling approach and you can see that the concentrations tend to be clustered together and toward the bottom of the plot they tend to peak about two or three kilometers downwind and it gradually tail off, but there's a large number of high concentrations that are scattered well above that cluster in the lower part of the graph that tend to also tail off with downwind distance.

If you look at the right plot, the sub-hourly modeling, you see that we - the sub-hourly modeling tends to get rid of some of the highest concentrations, but it also tends to increase the highest concentrations for receptors, particularly those beyond about 10 kilometers.

So, clearly, there's a shift here and where the highest concentrations are occurring, but it has not totally reduced that large number of values that are in this case well above 500 parts per billion.

The yellow squares in the plots are for comparison purposes. Those are the observations for doing the 4-year period of modeling. As you see at two kilometers, the left plot - the observations are actually below all the model values that were simulated for the entire 4-year period for all the receptors at two kilometers.

So, clearly, the model was overestimating concentrations that distance for all receptors regardless of the elevation receptors.

Most receptors are also - at 17 kilometers - are also overestimated compared to observations.

In the right plot, we see that the 2-kilometer results are actually more centered on the observation, so there's some that are below and some that are above 2 kilometers - the observed 2-kilometer values, but still, at 17 kilometers, most the results are still simulated to be well above the value that was observed with that one receptor or one monitor point. In conclusion, I wanted to say that it's clear that the time scale processing of meteorology is not a panacea, it does not always reduce the simulated concentrations by the model. The tendency in this particular case was for the model to overestimate. For those receptors that are clearly, directly, primarily impacted by the source that I modeled, the overestimates average about 80-percent but in some cases were as high as almost a factor of three. And it causes me to ask the question: Is it time for a different modeling paradigm. As we switched from hourly to sub-hourly, we got rid of some of the - we increased meander in the wind field, but we also introduced a lot of ours where the wind is very light, so we're actually swapping one problem for another and we actually have more hours under the sub-hourly processing when the steady state model assumption is less valid.

So, I encourage EPA to consider offering alternatives to a steady state modeling approach for near field plume impact evaluations.

MR. BRIDGERS: Thank you, Steve. We go from one Steve to another Steve. It's all yours.

MR. GOSSETT: All right. Yes, my name is Steve Gossett. I'm from Tennessee, but I'm originally from North Carolina. I'm glad to be back home. I'm an alumni of the most prestigious school in the Triad. I will not give their name but they do not wear blue, either shade of blue, and they will be playing a first round NCAA tournament game tomorrow at 12:40. I hadn't been there for a while, so looking forward to that tomorrow afternoon. I also want to say that I believe 99-percent of you do not know me because I do not know 99-percent of you. The reason for that is, I am not a modeler, so everybody keep that in mind as we talk - I am not a modeler, but I do work for Eastman and before I get off that, I want to say that I am a modeling skeptic, and I have been most impressed with this group: some very intelligent people, some smart folks and I'm going to go home still a modeling skeptic. Okay. All right.

So Eastman Chemical Company: just a word about Eastman. We were a part of Eastman Kodak from 1920 to 1994 and now spun off - we're a totally separate company. We have two major manufacturing locations in the United States: Longview, Texas and in Kingsport, Tennessee, which is where I work. And I'm going to tell you today about - let me back up here - we have a little problem with the So2 NAAQS, but we're going to solve the problem. I'm going to tell you about that problem and our proposed approach and you'll note, I have a co-presenter, Bob Paine, but he's already talked seven times so unless George yields me about three or four minutes, he won't get to talk. Okay. But you really want to hear what he has to say if we can give him time. All right.

We have a monitor in Sullivan County - been operating for 40 years. Guess who runs the monitor - Eastman. We have to by regulation, but we are altered by the state I guarantee you every quarter. We wish that monitor's data was wrong but it's not, it's right, and we have a design value of 196 - the standard is 75. We've had no problem with the So2 standard up until now.

Many of you do not have the luxury of a monitor and a lot of you are going to find problems when you start running these models that I'm highly skeptical of, especially when you use this airport data and I don't see how any of you are going to get anything done by 2013 and get it submitted to the EPA, but we have a little more time because we have a monitor. We have to submit a plan by I believe it's February 2014. And so we have this monitor. We were recommended for non-attainment last June and we do have the design value of 196 for the last three years.

Now, the little plant there in Kingsport, Tennessee - been there since 1920 - we employ about 7,000 Eastman employees. It's the headquarters, the corporate headquarters at Eastman. There are about, oh, probably 2500 contractors that work there. There's probably 5 or 6,000 Eastman retirees in the area. Needless to say, this is a very important facility - very important to the economy, very important to that neck-of-the-woods, and also we are in Kingsport, Tennessee, not Kingston or Kingsport upper northeast tip, and Tennessee does not stop in Knoxville. There's a nice part of Tennessee that goes right on up. It's the most beautiful part of Tennessee. In fact, the last presentation was from east Tennessee because I recognize those lakes because I've fished on all of them.

All right. This is the plant. We have three - where is the pointer? There it is. We have three - is there a better one? All right. We have the powerhouse here. This is - this one's a new one, relatively new, nicely scrubbed. Here's the middle-aged one, here's the older one. It's a system of 14 coal fired boilers. We're there because there's a lot of coal in southwest Virginia and eastern Kentucky. That coal's been there a long time and we been there a long time, but this plant runs on coal. Okay. So there's - you know where the So2 is emitted from at that plant? From those three powerhouses.

All right. So here's a bigger view of this part of Tennessee. It is in the Tennessee Valley of Ridge and Valley Province I guess is what you'd call it. You all call it complex - I call it hilly. This is the plant - Kingsport, Tennessee. The plant is bigger than the city, almost. We have a nice feature here called Bays Mountain, a thousand foot high. There's a river that cuts across, the south fork of the Holston. Here's the airport. It's a very nice airport for our size of a town - the tri-cities airport. It's seven or eight miles away. There's a lot of hills between that airport and our plant and you know, I'm learning about modeling and I'm learning about surface roughness and even I know that within on kilometer of any MET tower at any airport, the surface is not very rough. I could tell you that around our plant it's rough. We have 10-story buildings. We have mountains and hills. I mean it's - it's rough. All right.

Our plan is an elevation of about 1210. Here's the airport up about 1500, so that's the setting and you'll all agree, it's hilly. This is the windrose and the predominant prevailing wind is from the southwest to the northeast. Here's zooming in a little better and you can see the plant here. We have the monitor - the bad monitor right there. We had an upwind monitor called Meadow View there that was - that ran for many years. We had another monitor right there called Skyland Drive. You see these are hills at 1700 - this is 500 feet above the valley floor - placed there in the early 80s because they had some smart modelers back then too and the modeling showed that would be an impact area, so they placed a monitor there. Didn't run it very long because it didn't show that it would be any worse than down in the valley so they shut it off. There's a quick plot. We went back and retrieved the data from the state. It ran for a couple of years. There's the annual averages. This is the monitor that is downwind from us. This is the monitor that ran upwind for a long time and you can see that monitor didn't read any higher. Here are the fourth highs for the year for when we had complete data - you can see there for those two years a complete data. No worse than the valley. That's not what the model shows. We ran the AERMOD, we ran out sales. Then we had a real modeler, Bob Paine. He got the same answer we got using the airport data. Remember, the monitor data was just about the same for the data that we have. There the modeled number actually was pretty good, but not in a complex terrain - it was about six times higher.

Now, we are going to have to do something about that monitor that's exceeding the standard. We have a project and what we do is going to cost somewhere between $100-million and $300-million. We have two basic options and that's going to put So2 controls or convert to gas for that biggest powerhouse that runs half the plant.

Now, I told y'all I'm not a modeler but I came up with a model. We're going - this is going to reduce our emissions by 65-percent, overall emissions.

Here's my model. There's the design value. Reduce it 65-percent, there's a nice linear model and it's just about as good as yawl's I guarantee you. It shows we're going - we have a shot to obtain with that project.

Now, I do not want to go to the Eastman executive team and Board of Directors and tell them: "There's this mathematical model out there and it says if we're not going to make it, y'all are going to have to spend twice as much because this model shows some high, high concentrations up on these hilltops and there's downwashes on it also it predicts.

Now, so the question is - and I'm probably going to run over here for y'all: Will these plane controls be enough? Common sense and the available monitoring data says yes. AERMOD using the airport data says no.

Now, our plan - we've had some advice from Bob Paine and his colleagues and we're going to follow it. That is, we're going to - we're not going to use the airport data. We're going to use on-site data.

Now, do y'all know what it takes to get one year of on-site data? Has anybody gone out and gotten one year of on-site MET data? How much did it cost?

MALE: Half-a-million.

MR. GOSSETT: We're going to put in what I call an MD - an MDMT: Million Dollar MET Tower. We're going to put - I've got some photographs of it that went out last week. One hundred meter tall, co-located sonar and we're going to get one year on-site MET data. In parallel, we're going to continue running that monitor. The upwind monitor: we're going to reactivate that one that we ran for a couple of years and we're going to locate a fourth monitor in what we think is a downwash settlement. We're going to try the hourly So2 emissions, we're going to evaluate the performance of AERMOD and this other model - I think Bob wrote it and we're going to propose a modeling approach using this evaluation, this guidance.

So, now, everybody can't afford a one-million dollar MET tower, but you can see the importance to a facility like this that it's worth it to us to do and luckily, we have the time to do it.

Now, here's a close-up of the plant. There's one powerhouse - and by the way that's a wide building. Somebody said something about it's real long and we have another Palace here. You gonna yank me? Okay.

All right. When the wind blows for however many hours in here it blows right across these two stacks, those - it shows a downwash area right over here.

Now, Bob saw that and said, "I'm not sure I believe that." So we're going to put a monitor right there because I really don't believe y'all got a mathematical algorithm that really knows how wind blows and makes an eddy over a building. I just - I'm just not - I just don't believe it.

Here it shows an overall picture, again, the plant. The MET tower, we have a perfect location for it. It's right here in the valley. We own the property. By the way, we used to own a mountain and we gave it to the city and they made a park up there, so we own - we own this property, it's in a perfect location, it's just a stone throw you know from the plant. We are spending quite a bit of money on this thing. I joke a little bit but by the time we finish paying Bob to do all this modeling and everything, it's going to probably get up close to a million by the time we're done. We probably already - I mean the tower itself was $100,000, just for the tower. That doesn't include anything else. There's the monitor - that's a compliance monitor. Here's the upwind monitor. Here's this other monitor on the - on the ridge and here's that downwash and we're going to - we're going to do all this concurrently for a year. Here's the - here's the tower. The installed it last week. If you've never seen one of these things, go up. It's pretty cool. Here's a picture of it still going up, there's a picture of it all the way up, there's eight guide wires. On March the 9th, I climbed to the top with my camera. I shot back toward the plant - here's the plant. Here's looking Northeast, you can see some hills. There's that big hill. By the way, the model shows this to be all along the flank of this mountain. I like that - the light - the lighthouse beam, boom, it hits right there. There's nobody living on the side of that mountain. You could not stand on the side of that mountain. There's another shot looking to the south. There's the golf course right beside us. More hills. There's a new product center going in right there. Meadow View Conference Center is right there - more hills. There's my legs. There I am looking down on my partner. There's the light on the top of the tower. And I've already told you what the plan is and I'm way over.

MR. BRIDGERS: Would you want to move on to save some comments?

MR. GOSSETT: I would.

MR. BRIDGERS: If everybody would use the schedule, I think we're all right. It's a great presentation.

MR. GOSSETT: Thank you. I just wanted to change pace for y'all a little bit. It's fun to drive for a little while.

MR. PAINE: This is Bob Paine with AECOM. As you can imagine, it's very interesting working with Steve as a skeptic. The four monitors actually will be very valuable since they're going to collect hourly emissions data to evaluate the models under various things and also for concurrent regional background, they have to do a modeling study and some of those hills may be actually amendable towards CTDMPLUS being a discreet entity, so we're going to keep that option alive. I think the value - and it gets to Steve Mueller's issue too, and by the way Steve, this is the other facility that you didn't model. We ought to get together.

The vertical temperature difference from this tower is going to be much more accurate than what you would get from AERMODS parameterization without that information. Also the direct turbulence measurements from the tower to the sonar I think will make AERMODS predictions much more accurate, so I - I think this will be optimal data for AERMOD and I'm hopeful that EPA will consider that any facility that wants to do this prior to rushing to judgment on modeling, with whatever data you can throw to AERMOD would be allowed to do this type of program. As expensive as it is, it's much less expensive than the consequences of not having this.

So let's see - so we're going to obviously obtain wind measurements up to at least 200 meters with a sonar, get the temperature difference, have the tower measurements heights and have continuous Q&A with the Sonar because overlapping 50 and 100-meter concurrent measurements. And we're filing all the regulatory guidance and so we're going to put all this into AERMOD's vertical profiling input, so I'm going to go through this pretty fast.

And of course, we're going to have a limited evaluation just to see - and you know, maybe we can even use some of ours, you know, we have - we have a variety of options here, because when we click to see - because we're going to save data every second in case we need to actually figure out how to average the calms during an hour, so we want to have flexibility in doing the averaging of the on-site MET data.

We also would like to offer, with Steve's concurrence, this database for comparison to -

to MMIF - the MMIF profiles. This would be an excellent comparison type of site to see can you really trust the MIFF profiles for input to AEMOD.

And I think - I think that's it, so that was pretty fast.

MR. BRIDGERS: Thank you, Steve and Bob and just so that everybody understand is that Appendix W would say that we give preference to one year of on-site data over that from the airport. And Steve, if you ever need anybody to change a light bulb, call me, I'd love to come, then I play the golf right down the - down the.

MR. BRODE: I want to know did Bob ever climb the tower.

MR. BRIDGERS: Oh, yeah. Has the contractor ever climbed the tower?

BOB: I'm not the measurement contractor.

MR. BRIDGERS: So what we'll do - and I know that we ran over, but I thought that that was worth the extra 10 minutes. We'll go ahead and have Ashley talk now and then we'll take our break and then we'll get back on schedule. So -

MS. JONES: Well, good afternoon. My name is Ashley Jones. I'm with Trinity Consultants and I'd like to recognize my co-author Casey Dubbs. I'm giving a short case study for an aluminum plant with our challenges with the one-hour So2 NAAQS and a lot of these topics have been discussed throughout the last couple of days.

I am going to give a brief history on the facility. I think the timeline really shows where the plant has been with the modeling and some of the regulatory actions that we've gone through. The most noteworthy or the initial one is in August 2010: the facility received their PSD permit for a plant expansion. It was under a short timeline and a lot of capital expenditures were going to have to be done to comply with the model. Our challenge of the time was pm10.

So we had a couple of stacks - the pipeline stacks that had to be raised to GEP in order to pass initially. Shortly thereafter was the effective date of the one-hour So2 NAAQS and then a couple months later, the client decided to submit an amendment to the PSD permit to optimize these stack changes. Our request was to reduce the stack - the pipeline stacks from 65 meters down to 42 meters. That 42 meters is just above the formula height and of the time with version 09292, this was a working solution for us. We did address the one-hour So2 NAAQS obviously because it was after the effective date. However, our amendment request was not reviewed and right away, due to you know resource constraints and workload at the facility, or at the agency.

So the release of 11059 and 11103 came and then the agency got to review our amendment request in August of 2011 and when they did their modeling confirmation found we no longer had a passing solution. Not only do we not have a passing solution at the 42 meters, we didn't have a passing solution at the 65 meters.

The plant had to weight the option: "Well, we do have a permit to move forward with our expansion at the 65 meters," but we know these So2 one-hour SIP requirements are right around the corner and 65 meters no longer was going to be enough as things stood?

So our challenges we originally thought, the reason the stack would be a solution, no longer was likely. We evaluated an So2 emission rate reduction, however, that was very cost prohibitive. Not very common for this industry to have wet scrubbers and would be very expensive. An additional challenge that you've heard talked about was the buoyant line plume source model and we were instructed that we had to use this model and we had to use AERMOD And combine those impacts.

As Bob mentioned earlier, there is a receptor limitation around 100 receptors and that did not obviously allow for our full AERMOD receptor grid to be incorporated, so we - our solution was to go ahead and recompile that code to allow for the full receptor grid that we had in AERMOD and then to be able to combine those impacts spatially and contemporarily, we also had to recompile the BOP post-program to output a binary post file so that we could take the binary post file from BLP and one from AERMOD and combine those together.

This is just a screen shot of - I'll breeze through the analyst software that allowed us to take those two binary post files and merge them together and then extract out the concentration that we needed for comparison to the NAAQS. This help efficiency greatly and then helped reduce some of the conservatism from initially having just to look at both of the outcomes from each model separately.

Once we were able to combine those BLP and AERMOD impacts, you can see here this first row is our original So2 one-hour modeling analysis with version o9292, where we reduced the stack height down to the 42 meters. We were passing the model - you can see the AERMOD concentration of 144 and then combined with BLP and our background, we were below the NAAQS standard. When we had to update the version change to 11103 at the 42 meters, we had a significant increase.

We were then also instructed that well, we might as well go ahead and update the MET data because that was going to be part of the So2 SIP requirement, so we updated the MET data not only for AERMINUTE, but also for the actual years that were being used because this project has spanned quite a timeframe. That did add an additional 40 micrograms to our impacts as well.

And then we have the case where we reran back at the 65 meter height and you can see that we're still well - well over the standard.

This is an image of our facility layout. You can see here, this is our - this blue building is the - the influencing building for our GEP height and the lighter outline is our projected length area. I'd say this fits some of the scenarios that we've talked about earlier this morning and likely where our issue is stemming from, however, without any additional guidance or clarification, the facility really felt like we were stuck. We had - we couldn't really do much on the stack height, we couldn't go any further than 65 meters. We didn't have any guidance on being able to take credit for stacks taller than GEP and there were other recent clarification memos that came out, it was kind of discouraging to really pursue and for the client to spend money on evaluating these equivalent building dimensions.

So in summary, this is still an ongoing process for this facility. If we are - as things stand and we aren't able to do anything with the stack heights, taken the emission reductions necessary would be pretty detrimental cost to us - to them and our question and comment really is: Is any relief going to come from this pending downwash guidance? Is there going to be grandfathering clauses that would really help or is the So2 one-hour SIP just going to push everybody you know, is that really going to help or not? Will there be credit for taking - be able to take credit for the stacks above BLP height and are there going to be any streamline approaches for equivalent building dimensions?

In addition to waiting for this guidance that is very much needed and eagerly awaited for, the facility plans to initiate a field study to better understand their plant's monitored impacts and model impacts. We're not quite as far along as Steve there, but they - something that their next steps are to look into.

Obviously, we don't have time for question, but - thank you.

MR. BRIDGERS: Thank you, Ashley. As I said, let's go ahead and take our break. If we could try to be back right at the 30 mark, that's a good round number for us to get back started. Thank you.

[BREAK]

MR. BRIDGERS: Okay. We have a couple of talks this afternoon on behalf of ERM, so as we take our seats, I'm going to turn the podium over.

MR. YEGNAN: Thank you, George. Good afternoon, everyone. Glad to see everyone hanging in there on the final day - the afternoon of the final day. My name is Anand Yegnan and I'm at the ERM. Mark Garrison could not be here due to some personal reasons, so I'm going to be doing the talk in his behalf. What I'm going to be trying and do in the next 10 minutes or so is talk about some of the experiences that we have had with assisting clients in the electrical ability. Some in the industrial sector, but modeling, preparing for So2 designation and also for employment applications for SO compliance, especially with the one-hour NAAQS. I'm going to try and cover - we already talked extensively about the challenges with the one-hour NAAQS so I won't delve much into that. What has changed, what has remained the same, briefly touch on that more as a recap from the last two days. Talk about the specific antidotes relating to - relating to the challenges with the using model and more particularly for So2 modeling and finally, some recommendations.

Here's a map that shows So2 actual emissions greater than 110 per year, based on the last year sub-guidance - draft guidance. And source greater than 110 per year was recommended to be used for designation. As you can see it has a whole number of sources and once you start modeling these sources every day, it ends up being that way. It's not going to be a pretty picture, so it's something to think about.

A quick sensitivity analysis. Here it's an auxiliary boiler/process heater, small to medium size boiler. With no background, again, I will not delve too much into the numbers. The color codes are intended to show what the results are. The red is in exceedance of the NAAQS; yellow is more than 50-percent of the NAAQS; green is in compliance with the NAAQS. Along the row you see the - along the road it's emission rates, stack heights. Along the columns, with our background it's already a challenge. When you add 50-percent of the background - 50-percent of the NAAQS with the background, it makes it all the more challenging. So that is something to think about as - especially for industrial boilers/process heaters, which might be that - which might have a challenge with this.

Just a daily - maximum daily average trend of measured - monitoring and measured data for So2. The fourth highest is showing that and also the daily maximum. As you can see, with the - I have no data about 100 micrograms, we're already consuming 50-percent of the NAAQS so you're not left with much room for showing for compliance for sources, so that would play an important role.

In terms of using potential experimental PTE's, as you can see this is from a medium size coal fired boiler showing the daily trends of measure - of SIMS measured So2 data. There's quite a bit of variability and even if you have 1000 receptors at which you predict the impacts, it still does not capture the impact if you use actual emissions, but if you have potential emissions, it's going to be over the impacts, so use of potential might be a different answer.

So what is the likely outcome that we can see? Again, I'd offer it as new, considering what we have heard in the last two days and many of us who have been working on this for the last two years.

Significant non-attainment areas are likely to be seen due to the conservative nature of the standard, conservative approach, which is the use of the monitor and conservative background. All three together make it extremely challenging. One-hour NAAQS results could identify individual sources, unlike regional impacts, which we have seen for ozone and pm2.5. Many hot spots are likely to be shown as non-attainment areas with the model we use model for designation and potentially we might see many pseudo-non-attainment areas based on modeling which might not show up if you just use the monitoring data.

So what's remained the same with what we have done in the past and what we are doing right now? At the end of the day, AERMOD is still just a model and extraordinarily complex meteorological process we are trying to simulate using a steady-state model. It's almost like Lagrangian process being trapped in a semi-state model, which is something to consider. The presentation of MET data - that has always been a difficult issue even before the one-hour NAAQS and it continues to be an issue and will potentially in the near future.

Modern sensitivities. Many times it might predict things which are not physically real and that is - that I'll touch on briefly. So that is something which has been the case but the use of model, to potentially to remain an issue even in the future. Just to go a little bit further, just the use of model - I show it here - what we are showing is that you have a profile measurement in the top half of the picture and MMD, which is Model Meteorological Data: in the bottom half, although the prints are similar, you can see broader patterns of similar, but if you look at specific days, specific hours, it might not be similar and they are quite different actually and that's where the concern is. Overall trends might show comparable results, but with the nature of the standard that we're looking at, even isolated individual events could point to an exceedance and a non-obtainment.

So what's different? The stringency of the standard, I think we are beginning to get how stringent the new NAAQS standards are - with the use of the new - with the use of models for the designation, that is something which is new and probably had not been applied on a nationwide scale, but for designation purposes, complex - treatment of complex, of transport and exposure in boundary layer, point to more instances of unusual behavior. That is something which will be, which is a new phenomenon.

And one thing which I think Roger talked about on the first day: The models have been used and with the stringency of the standard, things which have worked in the past, pressing the EASY button so to speak, that might not work in the future and that might need to be brought under consideration of how we apply these models and also careful concentrations of sensitivity of input parameters. Which parameters is impact model for instance, that will need to be looked at very closely as we look into the future.

I'm going to briefly talk about two examples. The Logan's Peak looks like the favorite topic for this conference, but that's again, quite a bit.

Again, to illustrate even further, here is a very simple example. It shows three cases. The first one, A, it shows using AERMINUTE data, as you can see, the vent speed is in the range of .39 meters per second and the character impacts, it's a narrow plume, a concentrated plume. If you limit the wind speed to 1 meters per second in this case, the impact size varies quite a bit. And if you take it - that's in Example B. In Example 3, which I think Bob Paine talked about this morning: in terms of using the sub-hourly meteorological parameters, in this case we have used that similar data and use of that reduces impact quite a bit too.

So something to think about is how can AERMINUTE, which has been used to process a minute at ridges, how can that be used to better represent the variations within the given mark and how can that go into the model. That might be something in which alleviate some of the problems with the low wind speeds which we are seeing, with the use of AERMINUTE and might predict results, which are more - for lack of better word, realistic.

Again, same thing tabulated. As you can see, when we heard about it this morning, again, about it, that in some cases with these low wind speeds you see mechanical mixing heights, which are really unrealistic in many cases. They're like five meters and that's really low and that - that point to two things. First is we need to find a way to plead these low wind speed events and find a way to - find a fix for that or refine it as we heard before. And secondly, we can just use the model that needs to be some kind of an effort to understand the meteorological parameters, which are behind these results and help us understand why we're seeing these high impacts and which would help to find a solution to them in the near future.

So summary of the writing of the output from AERMOD, especially for events when we have exceedances or very high values that would be good to have.

Again, one more anecdote of value. We are seeing that with the taller stack, the impacts are higher than with the shorter stack and this is an elevated area. What we have done in here is that we have - we have an enhances version of AERMOD with the latest version. We have made some modifications to write out the meteorological parameters, which are associated with these high predicted events.

And it provides a better understanding and as you look at it, you can see that the buoyancy with the taller stack was much lower, almost nonexistent. And because of the ambient temperature which the AERMOD ended up using in there for that particular case. And that - that goes back again, point which I made earlier, to a better understanding of the meteorological parameters which make up the model prediction which would be useful and if AERMOD can write those things out, it would provide for a better understanding of the results.

So finally, trying to wrap it up, so what - what is going to be done? Low wind speeds, some - we are treating that by either limiting the wind speed to one meters per second, which probably might be a more realistic representation for a steady state model or find a way to use the sub-hourly values to provide for the variations within the air.

And secondly: the treatment of background data. There is some flexibility provided with the guidance from last year with the use of season-by-season by our approach. Once of the things which Appendix W talks about is the use of an average background data and especially if you have identified the meteorological conditions which make up the model predictions and trying to better treat what is in there and identify those specific background values and exclude the ones which the sources contributing to that. Then an average of those values could potentially be used as a background value and that might reduce some conservatism in the background value quite a bit.

Specifically relating to one-hour So2 modeling: Sensitivity analysis is an important piece. It should be an important piece of every analysis that ends up going into the model. And you have to pay very close attention to the meteorological conditions because of - because of issues of low wind speed and other similar parameters which might potentially contribute to the high impacts.

Finally, one thing to remember here is as we go through the designation and the implementation process, before we designate areas as nonattainment or come to any conclusions, better understanding or better data of the meteorological parameters of the background values which make up for that designation is an important recommendation that we would make before - because of ramifications of designating area as nonattainment could have - could be big, especially if - for future growth of industry or for even continued operation of the industry and that is something to consider. Use of actual emissions: some we are characterizing, capturing the variability in the actual emissions and instead of using the potential, that might be something to consider very seriously. Use of multicolor simulation or the impact as we told the board before, that might be an option.

And finally, sensitivities of fill-in parameters, that needs to be an actual value and documented and some fixes to identify unusual events could help towards a broader implementation of the use of these models for designation and implementation purposes.

So, that's what I have.

MR. BRIDGES: All right, second part.

MR. YEGNAN: All right. Round two. What I'm going to try and do: this is some work that we have done for the state of Maryland Department of Research program. It's a part of the Department of Natural Resources. We have spent quite some time looking at a broad range of issues as it related to models, including CALPUFF and AERMOD and also the use of prognostic MET data. Even before the MMIF too was available, and I used it in un-regulatory applications so I want to share some of the insights from what we have - experiences that we have learned from that and see if some of those could be used for the future. I'm broadly going to touch on three topics. First is evaluation of CALPUFF 6.4, that is something which we have been using in the more recent years, more recently for some applications. Use of - discuss some trends relating to the - relating to the measure data and also to some modern predictions using previous models and OLM. And how did they combat with the measure data for NO2 and NAAQS ratios in Baltimore, Maryland and finally wrap it up with some experiences with the use of MM5 extraction and MM5 profiles and use for a local scale modeling.

Starting off with CALPUFF 6.4, the Maryland Department of Natural Resources, Wildlife Research Program, a client that we work with, has been using CALPUFF for regional scale analysis, especially due to value impacts on Chesapeake Bay and in terms of impacts on the nutrient load of the bay. CALPUFF has been used as the modeling tool for these applications and many of these results of these analysis help the Chesapeake Bay Commission and other stakeholders understand the contribution of different sources and the impact of mitigation measures - part of the impact in terms of reducing the nutrient load of the bay. That has been - and we have done some comparisons with the results and the results from SEAMAC in terms of the nutrient loads and in so many cases. The new chemistry with the version 6.4 - based on some preliminary analysis, we find that it - it seems to improve the predictions of the nitrogen load quite a bit and the earlier indications is that it is very promising change. It's a work in progress and evaluations - we have converted to aerosol and gas concentrations - wet deposition estimates from NADP and other sites, we have looked at model evaluations by comparing it to CASNET and NADP monitored, measured data. So earlier indications show that 6.4 is a step in the right direction and it seems to improve model predictions quite a bit.

Just to show the modeling domain, as you can see, that's the - that's the extent of the evaluation domain and the modeling domain spans much bigger. It pretty much covers a good portion for the east of Midwest actually and this is the bay and - which is a primary focus area for us as a part of these analysis.

Just to give an idea about what we are looking at in terms of emissions, NAAQS emissions range close to seven - I believe it's seven million times and starts in the range of 8.8. We have developed some tools to capture this emissions from these regional scale sources and a model with CALPUFF and that is something which we can - we haven't discussed here but we'll be happy to share those.

Earlier, a I mentioned or indicated, preliminary results indicate significant improvements in prediction of nitric acid concentration, particularly nitrate and a event deposition of nitrates, so it's definitely promising to see the change.

Now, I'm going to switch gears a bit and talk about some of the measured data in NAAQS monitors, ambient monitors, more in the context of AERMOD. We have two monitors near Baltimore -- one at Old Town, the other one at Essex -- which have recorded date for nearly 20 years now. What we did in this case is modeled the actual emissions from 2009 with AERMOD, the focus being on stationary sources and nearly 438,000 pounds per day of NAAQS was modeled in these cases and did some comparisons with how did the modern predictions compare with the measured data at these two monitors. And this shows the - this slide shows the location of the Old Town and Essex. Old Town is right the urban area and Essex is outside of that.

Broader trends: this is a mightier trend ranging from '93 to 2009. What we see in here, the different lines in here have different NAAQS concentrations and we are showing ratios of NAAQS to NO2 and I think these trends are similar to what we heard before in the last couple of days where with the higher concentration the ratios are lower. That is NAAQS to - No2 to NAAQS, which kind of makes sense that with the lower concentration that is more ozone available, which converts NAAQS to enter with much greater than the higher concentrations. Looking at meteorological data for this analysis. We looked at Baltimore, BWI and I pulled up data from Sterling, Virginia. Use of AERMINUTE was made so there is a number of missing as it is a number of hours. One thing which we also found as a part of this a significant number of hours where you have really low wind speeds. AERMOD was used even more in this case. The next three slides essentially show trends in the windrose. The first one: this one that we see is the '91 to '95. The next one is with the BWI without collection for AERMINUTE 2005 to 2009 and the trends are much different and the average percentage of missing values and calms is significantly higher as you can see in here, as compared to what we saw in the previous data, so if you use AERMINUTE the trends are much broader trends and fairly comparable to what we saw before with the '91 to '95 data. However, you do see a significant increase in the percentage of the frequency of low wind speeds, which we know is an artifact of using AERMINUTE. So we might need something to do in terms to refine these AERMINUTE results.

The next three or four slides, we have - I'm going to show some comparisons between measured and prediction - measured data and between predictions for - in order to do NAAQS. In this case we have used a ratio of .1 and the first one which we see with oil and group as the option. Along the rows we see the hour of the day, along the column different concentration ranges and within each of those blocks we see months in the year, so we have the monthly trend along with the daily - the monthly and the daily trends and the ratios for different NAAQS concentrations.

The color-coding is intended to indicate - the red color is - the hard colors are more of higher concentrations, the cooler colors are lower ratios. Let me count that. With PVMRM, it's much different than what we see for OLM group, which again, based on what we heard in the last two days, it's no surprise. The ratios are much lower than that for OLM group and here if you - those are the predicted values. Now, if you compare it with the measured value, these trend seem to indicate that the trends are much closer within measured and predicted values if you use OLM group, enter a in-stack ratio of .1.

So that might be something to consider in terms of future guidance of - if - and this might be more case-specific- it might be applicable for Baltimore. It might be different for different areas, but OLM group seems to be a good fit in this case with an in-stack ratio of .1.

Similarly, if you use an in-stack ratio of .5, the results are over-predicted - they're much higher in terms of the ratios.

And we also looked at comparison between Old Town and Essex. For Essex, the impacts were under-predicted and one of the reasons why we think is because Essex do it - what you see in here are these are - we call it pollution roses, essentially, that is the measured value of- a type of thing prepared using measured value and based on the prediction of the wind. It still shows that the contribution comes pretty much from the Baltimore area, which might be - if we - we haven't accounted for mobile sources in here - that might change the impacts if we account for that.

So broadly speaking, one of the observations, OLM group appears to predict better than PVM (ed. PVMRM) model for Baltimore with an in-stack ratio .1.

We have seen some problems with AERMET processing, especially with low wind speed and DNR is currently recommending the use of pre-ASOS, but we don't have chemistry being used in AERMOD and with chemistry based on guidance from Region 3, we have - we have been using concurrent ozone and MET data for more recent years. Baltimore analysis points to one of the critical limitations of AERMOD in terms of issues relating to the transport time and - and just the simulating study of a Langrangian process with a study state model and it might be something to consider for future application - would be use of a Langrangian model like CALPUFF, even for new scale applications.

And finally, validating improvements and treating urban coal. That might be - that's an important issue.

Finally, to just wrap it up, as I mentioned, we have been looking at use of prognostic MET data as input to AERMOD similar to what the MMIF would do for quite some time now and many of these have been and do compare the regulatory option in AERMOD using National Weather Service data and we have done some comparisons with the tower data, with the - from nuclear stations and the earlier indications - the results have been fairly encouraging. We have - we are pretty encouraged to see the MMIF too now and hope to evaluate that and provide input and any kind of feedback we might have with the EPA in terms of what we see in comparison to the tools that we have available.

And we think finally that use of MM5 and (inaudible) potentially, holds a lot of promise and in particular for cases where the airport is at a far away location in complex situation, but it's - it could potentially be a good surrogate or an alternative to installing a MET tower or- we had to wait for a year. This might provide some good addition insights to - for use in a local scale model.

With that I will wrap it up.

MR. BRIDGERS: Thank you. Okay. For the next step we have Eladio.

MR. KNIPPING: This should go fast. Hello. I'm going to give an update on the development of the SCICHEM model. If there are a lot of pauses it's because I had throat surgery a few weeks ago, so just bear with me. I am Eladio Knipping. I'm a senior technical manager of the environment sector at the Electric Power Research Institute and we have been sponsoring the development of SCICHEM now for about 14 years. Some introductory notes: A new SCICHEM version is forthcoming in a few months. A prior version of SCICHEM has been described in previous presentations at this conference. This version has some but not all of the enhancements of the future version. As such, it has seen limited distribution for example to EPA for testing as a prerelease version.

This presentation will provide an update on the status of SCICHEM development overall. First, let me describe a little bit of the origin of SCICHEM. A general overview of the SCICHEM system has been provided earlier in this conference and detailed presentations on SCICHEM have been given at past EPA modeling conferences and at past CMAS conferences as well as other venues. The development of SCICHEM itself started in the late 1990s and all existing versions of SCICHEM at this moment are based on a 1998 version of the SCIPUFF model. As you recall from yesterday's presentation, SCICHEM is SCIPUFF with chemistry.

Testing and evaluation of past SCICHEM versions has been documented in EPRI reports. These EPRI reports are publicly and freely available at EPRI.com. If you should want to download these reports, you can contact an EPRI customer assistance center through our website for information on how to download them and also to obtain a legacy version, which is even older than the version described yesterday version of SCICHEM.

Now, SCICHEM has branched into two models. First of all the stand-alone air dispersion model SCICHEM - SCIPUFF with chemistry but also the APT branch. In the 2000s, EPRI invented the SCICHEM model into the EPA CMAQ model as a proved grid module for sub-group treatment of industrial plumes. This big version of SCICHEM is known by the name Advanced Plume Treatment, APT, which leads to the acronym CMAQ APT. A version of CMAQ APT was publicly released via the CMAQ center and it was based on version 4.6 of the CMAQ model. This version was based also on the sectional MADRID aerosol treatment for the treatment of particulate matter and as mentioned yesterday, it used carbon for the gas based chemistry.

Further development of APT continued using the CMAQ 4.7.X branch of the host model. This added the option of model aerosol treatment consistent with EPAs CMAQs AERMOD modules, which are the aerosol modules within the CMAQ model. It added the ability to run in parallel processors and the University of North Carolina tested this version and provided useful feedback to help refine the model development. CMAQ APT based on CMAQ 4.7.1 was not released to the public as development efforts focused on releasing an APT version with - that is compatible with CMAQ 5.0 as the host model and this is what was referred to in yesterday's presentation as the upcoming release of

CMAQ APT. If you should want a CMAQ 4.7.1 version of APT, we could make that available to you, but we would encourage to just wait until the CMAQ 5.0 APT version is available, which will become the most current version of the model and it will be released in an interim release of 5.8 in upcoming months, which will also make available other modules contributed to the community to the CMAQ system.

So what we've had is SCICHEM has developed in over the 2000s by focusing efforts on SCIPUFF on transporting the dispersion modules of SCICHEM and APT by focusing on the chemistry treatments.

So the chemistry elements of SCICHEM underwent refinement through continued development of APT. Similarly, the transporting of dispersion elements of SCICHEM underwent refinement through continued development of SCIPUFF in the 2000s. The evaluation of APT and SCIPUFF during this period has been documented in the period review of scientific literature. However, this branching of the code has led to an effort to reconcile the two codes into one uniform code for the stand-alone dispersion model, because now there's confusion as to who has the right chemistry and who has the right transport and "What

version am I working with," has led to a little bit of confusion. We want to avoid that confusion.
At the end of this reconciliation there will be only one stand-alone model, however, the two units will remain in order to emphasize whether the chemistry is on or off, so the SCIPUFF SCICHEM equivalency may be thought of as: SCIPUFF is SCICHEM with chemistry off or SCICHEM is SCIPUFF with chemistry on. The pre-released version of SCICHEM which was provide to EPA and others for pre-released testing does not have a graphic user interface. This is one of the - one of the important user considerations that we have also taken into account with the release of the next version, which will include a basic graphics user interface. It won't have you know a very fancy graphics user interface, but it will have the basic graphics user interface. This release version will be available in Windows and Unix Linux versions. The Unix Linux versions can also be run from a command line interface. The release version will be able to take advantage of multiple cores on a single machine, so if you have a dual processor, four-core on each processor, it can run it on all eight cores and it will include user manuals and documentation on evaluation and testing as well as a test case for users to run. The new release of SCICHEM

will become available to the community in upcoming months. We are working hard to get this out as soon as possible. The model will be free, open source and public domain.

So as some of you that are familiar with SCIPUFF may know, it did have some development that was based on Department of Defense funding, but the model has been approved for release as a free open source public domain model and this includes all of the development of model that has been sponsored by a variety of different agencies include DOD and EPRI.

As derivative products of SCICHEM may be developed since it will be available to the whole entire community, we can - what we want to clarify is that EPRI will remain the custodian of the core SCICHEM model and I would like to acknowledge Sage Management and Environ as the current model developers. We have Biswanath Chowdhury and Ralph Morris here of Environ. Just being from Sage if you both will stand up it would be nice. I guess Ralph already left, Biswantath. All the hard technical transporting disbursement questions; please send them over to him. Distribution of the core SCICHEM model will be through EPRI.com and through other linked portals. We haven't discussed this, but we are considering linking it through the websites of the model developers: the CMAS Center and as appropriate through the EPA model clearing house. We encourage the community to test, evaluate and apply the model as appropriate and to provide feedback that will allow for future refinements so that you know as with all the models, we are continuously looking to refine our ability to simulate the atmosphere.

And as an appendix, I'm not going to go over this right now because I am about at the limit of what I can handle. I have included some selected enhancements to SCICHEM that are related to the transport and dispersion and again you can contact me or Biswanath and we'll be happy to go over these enhancements that are not in the current pre-released version that was provided to EPA, but that will be taken into account in the final - in the - in the final release that will be available in - in the upcoming months. And all that is just shown in order for you to know that it's there for you to view at your leisure.

Any questions? Fantastic.

MR. BRIDGERS: Thank you, Eladio and best wishes as you continue to recover from your surgery. Just for everyone's reference - I know he had a handful of slides there that he had to step through - probably by five o'clock this afternoon, depending on how the open mic session goes, all these presentations will be posted on the SCRAM website, so you can look at them at your leisure.

So now, we're going to have a couple talks by Steve Hanna. Let me make sure I got the right one there. The floor is yours.

MR. HANNA: Thank you. Before I get started on this one, I was just thinking over the break about a possible solution to this downwash low wind problem and it occurred to me basic science principals of you're going to have the recirculation form only if the wind speed is larger than the turbulence speed and since - even AERMOD has a minimum turbulence speed of about you know whatever it is, half a meter per second. That means you can set a criteria when the wind drops below a half a meter per second, then downwash turns off and then you can have - use their usual extrapolation thing and assume you have full downwash at say two meters per second and then do a interpolation between two solutions between half and two, so I'll leave that solution with you.

But this particular talk, I thought I'd - since there's a lot of discussion of evaluations and PUFF models, I thought I'd give this example of a CALPUFF and SCIPUFF comparison that was done by myself and Joe Chang. This was a few years ago, but the - I'm going to emphasize the statistical evaluation and methods and sort of the approach to this. And this work was sponsored by the Defense Reduction Agency, who's been sponsoring most of the SCIPUFF development in the past 10 years or so.

So now, instead of looking at these huge long-range transport ETEX 1000 kilometer, we drop back to two mesoscale field experiments involving trace of releases sponsored by Department of Defense - they're called DIPOLE PRIDE 26 - they have these cute names that they dream up for all experiments and at Nevada test site in OLAD and these DIPOLE PRIDE was an instantaneous PUFF release, only 14 trials and OLAD was a line source with 11 trials. Because the military is more interested in dosage for health effects of chemical agents or something, then we're not looking at concentrations over 10 seconds - it's the total dosage over the period and the ARC MACS for each trial: we have the Boot software that we've been developing over the years in sort of the standard performance measures, but the key thing here I wanted to emphasize was the significance test that we use and also mention that because the military forces you to have model acceptance criteria, we came up with some - even though we didn't feel like it was appropriate to assign specific things - so we came up with acceptance criteria and they apply the same validation standards to dispersion models as they do to tanks or combat boots, so what are the acceptance criteria? Check it out that yes, it does or doesn't pass the acceptance criteria.

So here's the two sites. The first one, a Nevada test site, it's a 30 kilometer domain and there's mountains to the west. It's a big, flat valley as there is out there and these were releases of either from the north end or the south end of the domain of FS6 PUFFS and then there were three sampling lines and those are shown by the three lines with 30 samplers on each line and there were right meteorological surface sites on an upper air site and the Doug Way was the OLAD and those were instantaneous line sources also on - in a flat valley but with some pretty big mountains 10 kilometers away.

This shows you the observed winds and whenever you measure observed winds on a mesoscale network, you discover that they're all in disagreement with each other. This just happens every place, but this shows a typical variability of one to two meter per second and wind direction variability of about 60 degrees. We applied a CALMET - and these are the interpreted wind fills. SCIPUFF has its own diagnostic wind model and we ran that for input to SCIPUFF, then we ran both models with the CALMET inputs and just to show you what we got, these are scatter plots which you can just barely see, but up in the top right CALPUFF is on the top and SCIPUFF is in the middle and a Navy model is on the bottom, but they're sort of similar scatter with the CALPUFF And SCIPUFF - the line of best agreement goes through approximately the middle and they have about the same amount of scatter and with the DOUGWAY, the OLAD test models all seemed to under-predict the higher values, but there's still similar agreement among the models and this tabulates some of the performance measures and if you look down at the bottom at the high, the observed - the DIPOLE PRIDE was modeled fairly well by each model and at OLAD was under-predicted by a factor of two and the geometric means and factor of two and so on are there.

So what we found is that there was good performance at the DIPOLE PRIDE 26. At OLAD we're not sure why there's the under-predictions - that might have had something to do with some elevated sources with unstable conditions, but the key thing that we always emphasize in these evaluations is when you go from sight to sight you notice difference in performance. So it might be 30-percent over at one site, 50-percent under at another site and then the important thing is to look at the total group of sites. My two models' performance was about the same and the middle bullet is the important thing that we applied the Boot model evaluation a significant difference criteria and there's no significant difference between the performance measures for CALPUFF and SCIPUFF. Now, part of this is due to the fact that we're only 14 trials and 3 arcs, so there aren't a whole lot of data. And the references couple - the paper where this is described and then some recent papers on the model acceptance criteria so I guess now I need to go to the other one.

Thank you and George is getting pretty fast up here as the meeting goes -

This - continuing on, we switch over also now talking about evaluations of prognostic meteorological models of field observations and this is some studies that also were funded by the Defense Threat Reduction Agency over about the past 10 years and it's focusing on the boundary layer of variables because usually the meteorological values are evaluated of more things of interest to forecasters like rainfall and whether the front goes through on time, things like that, so the -

there was a seminal paper - a review paper by Nelson Seaman in 2000, at atmospheric environment in which he reviewed all the methods and recommended approaches for evaluating the forecast models for boundary layer inputs that go into the dispersion model. For example, the SCIPUFF model wants surface fluxes, PBL height. It likes TKE turbulence to be fed directly into it. So do the MET model outputs agree well with each other with the field data and so on, and I'm going to summarize just briefly two studies. One in which we compared MM5 and RAMS from the SARMAP domain, OTAG domain and ASOS domain and all these data we got from Tom Tesche and Dennis McNally and the SIRAN evaluations and other one was the OMEGA COAMPS and MM5 models for the Iraq 1991 scenario where they were - the army was blowing up munitions dumps and there was some chemical agents and the second study is using the IHOP data: That's not the pancake house. There was a big study and Department of Energy and so on of boundary layers and the HO stands for H2O, it's the international H2O project where they measured a lot of different body layer parameters in Kansas and Oklahoma on a large domain.

So looking at just a summary of the first study, for those four models and four domains, roughly what we can see is the - for the mean wind speed and the boundary layer with the winds - mean winds about 3 meters per second, the mean bias is only a meter per second and 10-degrees, but the RMS, the Root Mean Square error is typically 2 meters per second and 60-degrees for wind directions and we seem to find this everyplace, whether you go to urban areas over the ocean - there's similar amounts and this is sort of a fundamental uncertainty in the atmosphere that the models can't beat. The vertical temperature gradients are underestimated and these are 10-year old models, primarily because of poor vertical resolution and they've improved since then.

Now, to get to the IHOP study, it took place over a couple months in 2002 and we picked three test days. This is a picture of the domain. Some of these sites are the DOE's routine sites and others are the Oklahoma Mezzo Network and others were set out specifically and they measured surface fluxes and did a lot of slow rise radio signs and so on. And the meteorological models that were run - MM5 Penn State, Dave Stauffer was leading this aspect of it and ran MM5 with agnostic grid down to 4 kilometers and ANSEP, Jeff McQueen and his group ran WRF NMN for the scenarios. So here's the MM5 modeling domain with the 4 kilometer nest over Oklahoma and Kansas and the WRF NMN, which doesn't really show too much, but this is the figure they gave me.

First of all the vertical profile comparisons: There were slow rise radius signs. Every three hours five different sites and this is an example of the type of thing we got. This is one vertical profile and you often found here that WRF was 30-percent too high: In fact, that was a consistent thing and led the NCEP to go back in and change their PBL approximation method, which was based on a level that the TKE dropped a certain fraction of what it was at the surface. At night, you're in serious trouble with all these models because you have a really - often a really strong surface and version and one of the models got it that the others - the other didn't.

In this case WRF has - our MM5 is an elevated version WRF doesn't, so there is a lot of uncertainty in the mixing depths: As just a typical example of the wind speed over a day at one of the sites. This is as height of 60 meters and the RMSC typically was found to be about 2 meters per second here for both MM5 and WRF. The TKE, which was of great interest to SCIPUFF because it can accept TKE as a - which is a turbulent like SIGMA U-square plus SIGMA V-square plus SIGMA W-square and that was not too bad during the day. At night, again, there were big problems with using the MET model output at TKE. So as I said, the minimum or inherent uncertainty is about 1 meter per second. The largest bias is at night. Wind direction is proportional of one divided by the wind speed - the wind direction error. The mixing depth 20-percent error during the day plus or minus 100-percent at night and TKE has some problems. And these are incidentally in general agreement with the Seaman 2000 review paper and these - at the end here I put the references as well as the reference for the Seaman paper, so you know this is sort of a template possibly for doing Mezzo scale comparisons.

Thank you.

MR. BRIDGERS: Thanks, Steve. Just a few more talks and then we'll have another break. Let's see if it will work. Okay. David. And a special thank you for David making it. I know there was some challenges getting here, but, welcome.

MR. LONG: Good afternoon. I'm David Long. I'm a senior engineer in the Air Quality Services section of the Environmental Services with the American Electric Power Service Corporation. I'd also like to acknowledge my co-authors on this work: Pete Catizone, Mike Anderson, Steve Zale and Mike Newman from TRC who have done an excellent job of taking this from a proof of concept position into something that is - we're getting real close to being ready for primetime with it. The background of this work came about as a result of some questions from our engineering people about one of our plants in Texas and wanting to know what kind of an SO2 limit they'd have to meet for the one-hour standard and looking at the area, it was sort of, "Well, there's a lot of stuff down in that region and we better figure out what background is and when we went looking just sort of at the rough cut number for what's the closest monitor, there's one monitor in that region at Longview Airport and it's design value for 2008 to 2010 was 66 parts per billion. Not really something that's going to make a good background number in a fairly heavily industrialized area.

Well, we went and went ahead and Texas was very good about supplying us with the hourly data from that monitor and when we started looking at the hourly data, there were just hour after hour of very low values followed by a handful of hours with very high values, then went back to very low values again and sort of - something's hitting that monitor was sort of the conclusion you drew from it and it made it real clear to us that sort of the initial proxy is suggested in the guidance that EPA proposed last year - used the design value for a nearby monitor just really wasn't appropriate in this case.

Now, the area that we're looking at - as I mentioned we do - there's a number of industrial sources and these are just the power plants in the region that we actually used in our work and this was mentioned by one of our speakers a little bit earlier, there is a chemical plant in this area too. He's going, "Yeah." And really we didn't find a whole lot other than what we can attribute to the utility sources and as you'll note I've got two different colors up here: The black sources are coal fired plants and the orange sources are gas and oil fired facilities. Now, I will not the gas and oil facilities don't use that much oil. In fact, we own all three - all three of the gas oil facilities shown on here and in looking through the operating records of those facilities, only one unit at the Lieberman plant burned any oil in the three-year period that we're looking at here and it just burned oil for a short period of time in 2010. You know, I mentioned about the spikiness of the hourly data. When I was doing the proof of concept work, this was a plot that I made and as you can see, we go along less than 10 most of the time and all of a sudden here's a 95, here's a 78, you know, very spikey in its performance, which does indicate very much things hitting that monitor from a source, so it was - let's see if we can try and figure out what sources are - what sources might be doing this.

The initial method was to use a two-hour average: the hour of the high value plus the hour before and we've selected a 10 part per billion threshold just to try and make it easier to work with the data rather than trying to look at all 8760 hours in the year. The initial cut would be if the wind direction was within a 15-degree either side of the direction from the monitor to the source, we'd attribute it. And the wind direction was determined based on the one-minute data set average over the hour. If the - if we didn't meet the first condition, then we looked at the maximum and minimum one-minute wind directions for the two-hour period and if those fell within 30 degrees of either side of the - the direction of the source from the monitor, we would attribute it.

Now, obviously, you know, this is - we're trying to get just a rough cut, so we had a third criteria. If the first two conditions don't work, look at the one-minute data itself for both hours and see how many of the values that's within the plus or 15 -

plus or minus 15 degree sector of the source and if it was greater than 10-percent or more, then you could say, "Yeah, that source probably did have and it was having an impact on that value."

And then our final cut was if none of that worked, professional judgment or some additional analysis, which I will show you one value that was extremely high that we couldn't get rid of that went through - we took through this process and it really demonstrates just what this one-minute data can really do in examining this.

Our initial results when I was doing proof of concept, I was working with one month of data and you could do it in an excel spreadsheet. When we started looking at three years of data, that really wasn't going to work and the folks at TRC have developed some computer programs that we're still refining to do the analysis of all the data. However, we went in through this initial process, we did find some shortcomings and we've made changes and as I mentioned, one value from 2009 was taken to its ultimate conclusion since steps one through three didn't tell us anything about this value and it was up - I believe it was like a 79 value. It was right up around the standard. And when we did this, it showed the shortcoming of using everything based on just block hour values, but it also shows the capability of using the one-minute data and some of the things that you're capable of doing.

Our initial results on the one-minute - on the steps one through three came out with when we looked at - if - if there - if we screened out all the upwind sources, the three-year 99-percentile went from 66 to 14, so it really, really helped.

Then if we look at this high - the high value that we couldn't get rid of, here's that 75 I've mentioned, and it was - it definitely raised some curiosity - what was going on that that 75 was still hanging around after we put it through all this. Well, one of the fellas - and I'm not sure which one it was, but from up at - yeah - went through it and start - did the initial hour block looking at Martin Lake, which seemed to be one of our favorite sources to pick up at that monitor, came out and just went a few kilometers around the monitor, which started raising some questions and they started putting in an offset, working down into that first hour and when they put a 20-minute offset in, it just about ended up with a direct hit on the monitor.

So it really does go to show that when you can refine and have a higher resolution of the MET data, you can see things that just hourly MET data really doesn't show and it's not that there's anything wrong with the hourly MET data, it's just the resolution isn't fine enough.

After we went through this first round, we started looking at okay what works and what doesn't work? One of the things that we started to look at very early on was we're only using two hours and the wind speeds may not be all that high, so should we look at more hours and we - when we started doing that, we started to find transport distance and the travel time are very important to being able to identify what sources are impacting a monitor to given time. And we also have taken the method to consider a percentile of hours in the source bin as well as simply the percentile of just filtered daily high values for determining candidate background values.

Now, here, this is looking at a travel time instead of just a two-hour block, up to 12 hours and when we take a look at all valid concentrations, we find that the three-year average is 85 with upwind sources. When we're looking at the maximum with no upwind sources, that 85 becomes 25 and that's at the 100th percentile. When we look at the form of the standard - the 99th percentile of daily maximum and here's our 66 design value - when we - with the filtering, it drops to 11.

And we were talking a little bit earlier today at lunch about that 11 value and it was mentioned that there - if we wanted to start doing more of the step 4 analysis, we could probably knock the 11 down by a good bit. And we also look at it just from the percentile of the valid hours in each case and when it's looking at all valid concentrations, the 99th percentile value is 15. After we filter it, it drops to 5. So there you know it is - we're just believing this is a very good methodology for filtering the data. If you don't have multiple monitors in an area and have one that really is sitting in a fairly clean spot. And this is a table where we're looking at just who's doing what at what time looking at all valid - just the all valid hours characterization and this is year-by-year data and using the 15 degree sectors. We ultimately concluded that really 30 degrees is probably the better way to look at this. But when you see year-by-year - when we look at no upwind sources, what you end up seeing if you're looking at the 99th percentile values, something akin to the form of the standard, you know you're seeing - here's roughly 6, 6 -- we look at 2010 - okay, it's up to 7. And when you look at the three-year average of it, it's 6 parts per billion looking at the 99th percentile of the valid hourly values with no upwind sources. When you open it up to 30 degrees: that value drops to 4.6. And the 100th percentile value drops to 32.6 from 82.2, so you're definitely screening more hours out and being able to say, "Yes, there was a specific source that was in our - under consideration that was impacting that hour."

What's our conclusions? Well, the use of - and here I'm calling it the 65 data sets, which that's the format - is very - is viable. It's - we believe this is a viable technique that transit times up to 12 hours should be considered to maximize the capture of source impacts on one-hour monitored values. If you're considering upwind sources with a long transport time, the angle between the average wind direction and source direction should be 30 degrees in order to allow for the plume meander that's going to occur. And the Step 4 analysis, it should be available in the methodology because you may not be able to find every hour and have it conform to the, you know, the block hour requirements as we saw earlier. And consideration should be given to grouping sources and this would - we think that would be useful if you have a large number of sources within a 10 or 15 maybe 20 degree sector coming out - possibly grouping them rather than trying to say well, it was this source here when 15 or 30 degree sectors we're using are overlapping so much amongst the sources. And while we did this work for one-hour SO2, we think it should be valid for any pollutant with a one hour averaging time and quite possibly for other short - fairly short averaging times. You know, obviously, if we're looking at 12 hours, we're not really doing something with this that's going to be real - probably real good for 24 hours, but that might be something to test at some point in the future.

Thank you.

MR. BRIDGERS: All right. Do you want me to turn the microphone on?

MR. COLOMBARI: Do you want me to use that keyboard?

MR. BRIDGERS: Either the up or the down microphone.

MR. COLOMBARI: Okay. Thanks. Hi, I'm Toni Colombari with Trinity Consultants and I'm going to be giving a presentation a little bit different than others. Were' not going to look at a specific situation but sort of a broad category of all the different things you can look at to help you meet the

NAAQS and some of these that maybe we haven't looked at that much in the past.

So I'm just going to give a really brief background and then I'll jump right in to the list. So this is sort of the three main categories of things that we'll put into the model. We have all our source parameters, which includes you know stack height, emission rate, velocity, building height, building location and that brings in downwash and all those things. Then we have our MET data and I'll have wind direction, wind speeds, low wind speeds, temperature and then we have our receptors, ordinance on those, terrain data, elevation receptors and all those come together and that's how we get our model concentration.

So we're going to sort of walk through a list of how these five different things and really have some sub-categories of what in those original blocks we can change.

So the first one is to re-characterize ambient air as not being ambient air. Some of these seem pretty obvious, you know, put up "No Trespassing" signs. Put up a fence line so that you can count your property line as your fence line. I know there's been a little bit of discussion. I haven't seen this gone too far about what happens if your worse-case receptor is over open water and then one of the more unique ones would be to actually look into purchasing land where your maximum model concentrations are showing up if that's nearby and then you can just fence it off or maybe purchase the sort of clean air mineral rights to that land and then you could just exclude those receptors from your analysis.

Number 4 is to re-evaluate your MET data or your monitoring data like we just saw if you just - you have all this background data but maybe your sources that you're modeling is actually contributing to that and sort of take a look and try to pull out the ones that are already being considered in your model.

We also looked at considering on-site MET data, although that may cost you a million dollars.

Look at whether - you know, don't always look at the closest airport. Just because it's five or ten miles away doesn't mean it's the best MET data for where your facility is. If you're in really complex terrain and the airport is in a really flat area, then that may not be very representative and sometimes you have to look a little bit further away.

And then the last one would be to look at pairing background concentrations and time, especially for the 24-hour standard, but even with the one-hour standards, your model concentration and your monitoring concentrations are not always going to overlap at the same time and you can really see huge reductions in your you know modeled results.

Number three is to look at building changes. For a green field facility, you can look at rearranging the buildings before you actually build them to see what is our downwash going to be and try to minimize that as much as possible. You could also look at just knocking down some buildings. If you're getting a lot of downwash and you don't really need those buildings, just get rid of them. And then one of the more unique ones is actually look at increasing building height because that will increase your equation 1 GEP height and not necessarily adding a whole story, but something like a parapet to the top of your building could actually let you raise your stack a little bit higher.

You can look at pursuing alternative models or switches within the model. We heard a lot about using CALPUFF for complex winds and we know that justification for that is really difficult, but it's something you definitely could pursue.

And then one that's not thought of a lot is rethink rural versus urban coefficients. A lot of times a state agency will just block out areas and say, "If you're here, you need to model urban coefficients with "X" population, but if you have really tall stacks and they're sort of releasing above or the plume rise is going above the urban boundary layer, then you may want to rethink whether the urban or the rural coefficients are really appropriate and that can get really complicated since that urban boundary layer is going to be changing by hour, so that can get really detailed analysis, but that's just one more thing that can sort of come into play and could help facilities sort of show lower modeled concentrations.

And then the number one is to actually look at - and this may sound crazy - but lowering stack height. If you have really tall stacks that aren't having a lot of downwash, they're having impacts really far away from the facility, they're all going to be heading probably the same area, so you know, each one of your stacks are probably having three times the impact since they're all hitting the same area. You lower one or two of those stacks, their individual concentrations might go up, but the overall impact to your facility could go down by you know a factor of a third - half to two-thirds, maybe less, depending on how many stacks you have. Or you know the alternative to just chopping off part of your stack, you could just look at selectively adding control equipment to just a few of the stacks or one of the stacks, such as like a wet scrubber that's going to decrease the amount of plume rise you get. That could have the same affect. You'll have a lower emission rate, but you'll also have lower flow rate, lower temperature, lower plume rise and the plume should sort of separate and not really hit that same area and that can sort of have the same impact of really lowering your facilities total impact.

So just some conclusions: As we've seen, and people have been saying time and time again, these new standards are really tough. We'd love to see some updates to Appendix W and some new models and some changes to GEP equations in the model, but right now this is what we have to work with and we need to also look at just all these other inputs and see what else can we change, what else can we sort of manipulate to you know pass the model - to pass modeling, to pass permit modeling with the models we currently have. A lot of these alternatives aren't going to work, you know, you can't always just increase your boundary. Your property line - you can't always chop off a stack, but some of these might and there's a lot more we didn't include here. This was you know at first a list of ten but I had to chop it down. And then just make sure you apply any and all reasonable means and maybe some unreasonable ones - try to make them reasonable. So just a few acknowledgments: just some other people in Trinity that helped develop this list and many more that we didn't include.

Made it quick for you.

MR. BRIDGERS: Okay. Up next we have Justin. I hope you - do you have a power point - okay. That was the one presentation I didn't have so let me just put up a stoic slide. So up next we have Justin Walters with Southern Company.

MR. WALTERS: All right. Good afternoon. I have the - I think the distinction of being the last?

MR. BRIDGERS: The last requested.

MR. WALTERS: My name is Justin Walters and I'm speaking today as a representative of Southern Company, a leading energy supplier in the Southeastern United States. As a company that generates electricity, Southern facilities are frequently the subject of air quality modeling for regulatory purposes: From PSD permitting to state implementation plans to federal regulatory programs.

Over the years, we have witnessed an increasing reliance by EPA on models. Models have historically been used in the PSD world in a deterministic manner, but were primarily used to address standards that were well above background and with long averaging times. Now, with short-term standards that approach values that are closer to background concentrations, the models require a level of accuracy and precision beyond their current capability. Surprisingly, models are increasingly being given greater deference than ambient measurements. Continuing to apply modes with existing but outdated guidance is inappropriate. As an example, EPA's September 22nd, 2011 draft guidance for one-hour SO2 NAAQS SIP submissions requires that all major So2 sources demonstrate through modeling that they are not causing or contributing to violations of the one-hour NAAQS regardless of the presence of monitoring data that may show attainment. For sources that cause or contribute to model exceedances of the NAAQS, federally enforceable emission limitations are required that when modeled demonstrate attainment of the standard. Such a prescriptive requirement for such a stringent one-hour standards necessitates, among other things, the use of an unbiased model.

AERMOD has not been thoroughly evaluated for performance in simulating the maximum daily one-hour concentration at the 99th percentile, nor has it been evaluated for the 99th percentile in the case of NO2.

Prescribing emission limits for such a stringent standard from the results of an unproven model could result in significant and unnecessary cost and burdens on Southern Company, the state's in which we operate and our customers.

Should EPA finalize and or codify its modeling requirements for SIPS and middles for the one-hour SO2 NAAQS, EPA should give states discretion to use modeling and analysis tools to help demonstrate situations where sources have a low probability of violating the standard.

EPA's draft guidance requires that sources model at their highest potential to omit for all hours of the year. The vast majority of sources do not operate at full load 24 hours a day, 365 days a year. States should therefore have a discretion to allow sources to demonstrate perhaps through application of the emissions variability processor or MVEP that you heard about on Wednesday: The less restrictive emission limits will still achieve the NAAQS.

Okay. Switching topics: Modifications

to preferred models such as AERMOD that are bug fixes are welcome and we applaud that effort, but modifications that result in changes to model concentrations must go through notice and comment rule making before being implemented in preferred models. Appendix W states that, and I quote: "A preferred model should be operated with the options listed in Appendix A as recommendations for regulatory use. If other options are exercised, the model is no longer preferred. Any other modification to preferred model that would result in a change in the concentration estimates, likewise, alters its status as a preferred model."

The guideline also states that: "For the preferred AERMOD model, building wake affects are simulated for stacks less than good engineering practice height using methods contained in the PRIME downwash algorithms, therefore, the recent change in AERMOD noted in model change bulletin number four that alters the prime downwash algorithms to apply to stacks that are greater than or equal to the GEP formula height relegates the model to non-preferred status." Because as we have seen earlier today, this change has been shown to cause significant increase in modeled concentrations, therefore, this is not simply a bug fix, but must be subject to notice and comment rule making. Along that same line, EPA needs to evaluate the performance of AERMOD with the AERMINUTE processor.

On Tuesday, during a presentation on the CALPUFF model, Roger Brode made the argument that for demonstrating the performance of CALPUFF for complex winds, the CALMET processor is an integral part of the CALPUFF modeling system, since that model is the source of the complex wind fill input. Similarly then, for evaluating the performance of AERMOD, EPA must consider the entire AERMOD system, including AERMET, and since EPA has now recommended its use, AERMINUTE. We have seen evidence at this conference that AERMOD significantly over-predicts at low wind speeds. The use of AERMINUTE often leads to a significant increase in the number of low wind speed hours modeled. EPA's justification for implantation of AERMINUTE and AERMOD is that it is more technically sound. However, the scientifically or technically justified change to a model component does not always lead to overall model performance. EPA should avoid the appearance of a double standard when it comes to model performance evaluation.

If EPA considers that CALMET is an integral part of CALPUFF and that the two must be evaluated together, then it stands to reason that prior to EPA recommending its use, AERMINUTE should be included in a performance evaluation of AERMOD, demonstrating that its conclusion leads to improved model performance in a wide range of meteorological conditions, particularly, the low wind speed and stable conditions that are most affected by the AERMINUTE processor. Once its evaluation is complete, the model changes should be submitted for public notice and comment.

Finally, EPA should wholeheartedly engage not only at the regional, state and local agencies, but also the private sector including consultants and industry to achieve goals in improving the performance of both preferred and alternative models. We have heard a number of times, including at this meeting, that the air quality modeling group is understaffed and underfunded. You have willing partners in the modeling community. Please work with the modeling community in implementing model improvements and updates.

During this conference, we have heard several presenters discuss their efforts in desired improved guidance, models and modeling tools, many with similar frustrations, that their effort is mired in the model of clearinghouse process. EPA should engage and provide a clear path forward for approving tools and model improvements that come from the modeling community such as the tools and model improvements we heard discussed during this conference. In fact, evaluation of the performance of AERMOD in conjunction with some of these tools could be incorporated into the evaluation that EPA must conduct on the performance of AERMOD with AERMINUTE.

I appreciate that opportunity to speak today. Meetings of the modeling community and EPA such as this one are increasingly important since there are so many pending modeling and regulatory implementation issues to address. We encourage EPA to engage the modeling community on a more frequent basis. Unfortunately, the private sector has not been invited to participate in this year's regional, state and local modelers workshop, however, we do look forward to being included in such opportunities to engage EPA in the future.

In conclusion, with standards becoming so stringent and with an increasing reliance on models in the regulatory process, we feel that it is imperative that EPA assure that the performance of its guideline models is unbiased and does not lead to gross over-predictions that could force costly and unnecessary actions to be taken. Second, if EPA determines that modification to the model formulation beyond bug fixes or modifications that cause changes in model concentrations are warranted and necessary, EPA must perform a model performance evaluation for those modifications and submit it for public notice and comment.

Finally, we encourage the air-quality modeling group to be transparent and open and engage in C-collaboration with the modeling community to leverage resources and promote community model development.

Thank you.

MR. BRIDGERS: Well, thank you Justin for your comments. I think that - do we want to take the 10-minute break or do we want to open it up? I mean that's kind of up to us. If we want to take a 10-minute break, just come back at 4:10 and we'll have open mic for anybody else that wants to make public comments and then we'll close the conference. So let's take the break.

(WHEREUPON, a brief break was taken.)

MR. BRIDGERS: We've about finished our break time. As everybody is taking their seat for this last session, I'll offer as an opportunity for anyone in the audience or others that have thoughts or comments during the course of this conference to make statements. Please understand that we do welcome all comments to be submitted to the document, so you don't have to speak today and I think Tyler had made the comment earlier to someone that they're not officially a part of the document until we submit - at least your comments are not officially part of the document until we submit the transcripts and probably what we'll do is once we get the transcripts back from our contractor here, as we review them and identify individuals that spoke and gave comments or presented, we'll share those with you so you can scan those too before we put them in the docket. You know, you know what you said better than we did. So at this time, the microphones are open. Please identify yourself, whatever corporation you're with and have at it.

MR. LEBEIS: Hi. I'm Mike Lebeis with DTE Energy and actually I had a question earlier in the conference related to intermittent sources and the different categories of intermittent sources that you know were suggested in the March 1st, 2011 memo. Well, it turns out that one of our plants we had an issue where the diesel generators got - like dragged along with the one-hour NO2 standard came along and as an interim measure, we had to agree to extremely high stack heights on some diesel generators that average operating maybe like five hours a year, typically. With the March 1st guidance, we were able to go back in, remodel them at an annual average emission rate and actually it turns out that I had to come down here for the permit to get issued, so it's interesting how when you finally leave town, that's when everything gets finalized and so I actually got the good news on Tuesday that the permit did get finalized after going through a 30-day public comment period, also some response to comments and things like that, but those things were resolved and the permit was issued on Tuesday. So there is hope for intermittent sources out there.

MR. BRIDGERS: Thank you.

MR. SCHEWE: George Schewe, Trinity. How long will the interim SO2 and NO2 seals be interim?

MR. FOX: This is Tyler Fox, OAQPS. I just want to be clear, this isn't a question and answer period. This is a public comment period, so you can submit comments, but we're not answering questions.

MR. SCHEWE: Okay. We'll play Jeopardy. We would like to comment that we're concerned about how long the interim seals may be interim. Thank you.

MR. BRIDGERS: Thank you for the comment. Going once, going twice. Do you want to just check one more time?

MR. FOX: Yeah, I'll make one more request that the microphone is open for anyone that does want to make a comment to be submitted to the docket when the transcripts are submitted. And if not, like I said earlier, please feel free to make written comments to the docket. Do you want to say anything in closing?

MR. BRIDGERS: I just want to thank everybody for their active participation and have safe travels back to where you're going and we appreciate everything and we'll be following up with, as George said, any follow up on the transcript itself and expect that you'll hear back from us in terms of any response related to this conference and we appreciate all the input and again, we'll be in contact soon.

And I officially close this public hearing.

(WHEREUPON, the hearing was concluded.)

CAPTION

The foregoing matter was taken on the date, and at the time and place set out on the Title page hereof.

It was requested that the matter be taken by the reporter and that the same be reduced to typewritten form.

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