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ENVIRONMENTAL PROTECTION AGENCY

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8:30 a.m.

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<u>F U R T H E R   P R O C E E D I N G S</u> 8:33 a.m.

Mr. Bridgers: So while everybody is taking their seats, I'll go ahead and call the public hearing and the conference back to order. I hope that everybody had a pleasant evening. The weather last night was phenomenal. I forget to mention before everybody broke for evening that there was a meteor shower last night. I guess it is August. I did get home late, but I didn't look up.

Nonetheless, thank you for coming back this morning. I know we lost a few people but we have ten more public presentations this morning, and I already know that we have at least a couple of slide decks that have been requested during the open forum session that will follow right before lunch and then right after lunch.

So I did want to go through--this is just some real quick logistics. Most of you that were here yesterday, this is old hat, but I just wanted to reiterate that this conference is a public hearing. It's a public hearing both in the context of Section 320 of the Clean Air Act, which requires us to have triennial modeling conferences but it's also a public hearing with respect to the proposed rulemaking for the guideline--for the revisions to the *Guideline on Air Quality Model*.

So again, everyone that speaks today needs to identify themselves and their affiliation. As such, I'm

George Bridgers with the Air Quality Modeling Group here at the USEPA. Everything that is said is going to be transcribed, and that will become part of the record and submitted to the docket.

We will not allow any question and answering. And anyone that has not requested a spot to speak, which will be the first ten presentations, out the front door here Nan has a sign-up sheet that you can put your name on, but I'll go ahead and say that later in the after lunch period we'll have a session that--not assigning myself to any religious affiliation, but the Quakers, where if the spirit moves you, you can come up and speak at the mic. All of our presentations during the open forum, we will respect them the same as we have our other speakers, and they will have up to 15 minutes, if they would like, to use that time.

After the conference, if you would like to submit--I'm sure everybody that probably spoke will be submitting final comments in writing to the docket. You have--I think it's 74 days before we get to October 27.

We have a brisk schedule. It's not ridiculous, but we're going to do like we did yesterday. Each speaker again has 15 minutes and maybe a few seconds here or there, but we need to keep people on track, and so I just ask for everybody to be respectful.

And so we that, we'll actually be ahead of schedule, and we'll go ahead and call Beth--is Beth here, she is--to the podium. And while Beth is coming up, again, if there are any questions or issues, find me and I will try to resolve them. So without further ado, Beth, the forum is yours. The podium is yours.

Ms. Barfield: My name is Beth Barfield. I'm with Environmental Resources Management. We call it ERM. Today I am speaking on behalf of the National Ambient Air Quality Standards Implementation Coalition. Last afternoon the last speaker was Rob Kaufmann, and he gave you the comments that were more policy oriented than the ones that I'll be presenting today are the more technical oriented, the ones that we're talking about, the specific comments to the proposed revisions.

Yesterday also Rob mentioned a song of Mighty Mouse. And I was kind of surprised because we have further at ERM shortened that acronym from NAAQS Implementation Coalition to NIC. So I thought he was going to do Mickey Mouse, you know, because we're N-I-C, and we're saying why. We like to see the revisions, and that is really the theme of my presentation. Before I went into my singing, I should have asked how to do this. Do you do this? Okay. There we go.

Okay. The study objectives that we had when we were preparing even--not even comments, but recommendations for what we would like to see for revisions for the Appendix W, we looked at the challenges that the members of the NIC were having when they were doing their air permitting and their compliance demonstrations that were dependent on dispersion modeling, and we wanted to identify the key technical changes that we would like to see in the Appendix W revisions.

The--NIC, see there, that's what we're calling it--is comprised of various trade organizations and individual member companies who were having these challenges in demonstrating their compliance when they were doing their permitting and even just regular ambient compliance modeling that would be required by the states.

What we came up with, the three that I'm going to focus on today, are the recommendations on how the background concentration should be handled, also the suggestion that the low wind beta options be changed to default instead of non-default beta, and similarly, that the two refinements should become default as opposed to non-default beta options.

What I'll do today is just present to you the general approach that we took in preparing our study and then how we develop the virtual sources and then the results that we have that support the recommendations that we made.

This is really again not so much a list of things that I would hope that you could read during this presentation, but just to show you that there is a lot of different guidance that has to be followed, and we would like to see these things compiled in one Appendix W. It looks like it's going to be.

We began with three hypothetical types of facilities. One was a gas fired electrical generating unit--these are combined cycle and simple cycle units--also then a gas refinery and some industrial manufacturing, just to look at three different types of facilities.

These were again virtual facilities. Nobody is planning to build any of these. They were a compilation of different units that are in different facilities throughout the U.S. by the member companies. There was quite of a negotiation process about what our facilities should look like.

So here with the electric generating unit, as I mentioned, we have both combined and simple cycle. And then the refinery we have all the process heaters and flares. We tried to make all of the facilities look like they were very--generally well controlled, not brand new but existing units; and then a manufacturing facility.

Similarly, we picked three different types of location, ones with flat terrain, ones with complex terrain, and one with rolling terrain. The reason that we took specific locations is because we wanted to have actual meteorology and ambient monitoring data that we could use in our analysis.

And again, none of those sites are really being considered. They were empty sites. We chose them as empty because we didn't want the background concentrations to include something that was already there that we were going to be modeling on top of.

So there's our site in Louisiana. And you can see the relative location of the site with the refinery, the power plant, and the manufacturing location. And then here is--the Montana site was our complex one. And in North Carolina King's Mountain is out there.

The first comment that we wanted to make on the changes that the EPA is proposing is from the 8.3.2 c.iii that "For short-term standards, the diurnal or seasonal patterns of the air quality monitoring data may differ significantly from the patterns associated with the modeled concentrations."

And we found out that this does occur frequently. If you look at the table here, we have the averaging period for $PM_{2.5}$ and O_2 and SO_2. We presented the design value--and this is for the North Carolina site--the design value if we take just the annual numbers, and then also how when we paired it with the modeling results, you can see that $PM_{2.5}$ didn't go down much, but NO_2 and SO_2 really made a significant difference. And in our cases, it was the difference between demonstrating attainment and not.

The next one is the modification to the AERMOD system. This has been discussed earlier, yesterday, and that was on changing the low wind from a default--to a default from the non-default beta options. And in our cases, we found that the North Carolina--or all the results changed more significantly in the fugitive emissions than in the cumulative impact, so I've put both of those here. You can see the difference. And with the annual average, similarly we see more of the results are significantly impacted with the fugitive emissions, not so much with the whole cumulative.

And I think, now, Bob talked too about the stacks yesterday. And we found a similar difference between those. And this is again using the low wind option 2. We haven't done this with 3.

The third is how the treatment of nitrogen dioxide would be in. In the preamble it was mentioned that the ARM2 would be replaced. And we had discussions yesterday about this in more detail. Rich Hamel went through that. The difference--now, what we had used was the .3 instead of the .5. We haven't redone it for those numbers. And what we would like to see is this to be changed also to default so that we don't need to spend the time and the money coming up with a site specific demonstration.

This is the differences that we found. You can see I listed them all for North Carolina, Montana, and Louisiana with the Tier 1, 2, and 3. You can see with the--even with the Tier 2 ARM2 that there was a significant reduction in the proposed--in, sorry, the calculated concentrations.

So in summary, we would like to support the EPA's revised changes on how background concentrations should be determined. And by that we mean using the diurnal--seasonal diurnal type of background calculations instead of a one number design value.

And that's because, as we showed, that there is a low likelihood between the time and the place that the--or the time, I should say, that the monitored value would be peaking and the model value would also be peaking.

We also support the designation of the low wind speed options to default in both AERMET and in AERMOD. And finally, we support also the change of the non-default beta options for the NO_x refinements to default options. And I will take questions offline at another time. Thank you for your attention.

Mr. Bridgers: Thank you, Beth. We're zooming right along. If David--is David--oh, David is here, yes. All right, David.

Mr. Heinold: Good morning, everyone. My name is David Heinold. I'm an air quality meteorologist with AECOM. And I'm speaking on the behalf of the American Forest & Paper Association and the American Wood Council.

This morning what I'd like to talk about are some issues that are out there that have been out there kind of forever for low level emission sources. When we mean low level emission sources we mean things like fugitive dust, emissions that are essentially non-buoyant from low rise buildings that are very common to industry throughout the U.S.

So one of the characteristics of these types of sources that make them particularly problematical in terms of modeling, well, location of these sources relative to what are--have been considered historically off-site receptors is often very close.

In other words, some of these are smaller facilities that are--that are nearby fence lines so that if you have emissions close to the ground, you could easily have model concentrations that are disproportionately high in reference to the annualized emissions from those sources. So because of that modeling those sources and characterizing them are extremely important. They are also many times emitted in--among the flow obstacles like buildings and tanks and piles and whatnot. Again it makes them characterizing very important.

And more recently, because we now have tighter NAAQS--that is the background concentrations are getting perilously close to the ambient air quality standards, especially for example for annual $PM_{2.5}$--if we model them with excessive conservatism, they're often likely to demonstrate a model that's not in compliance when there really isn't an issue, especially if there isn't an issue really where legitimate off-site receptors should be located.

So based on that, there are essentially two issues that I'm going to talk about today among all of these. The first is the identification of what has been characteristically identified as fence line receptors as a standard way of establishing what receptors should need to be modeled.

And the second is taking a look at issues that have been brought up at previous modeling conferences regarding PM_{10} and $PM_{2.5}$, and the rate that they are transported off site and the physical dynamics that affect those concentrations as they go off site.

So placement of near-field receptors; so the idea that we're discussing here is in 2014 EPA came up with risk assessment guidance that has been essentially long-standing but clarified, and it talked about how to characterize off-site risk, and in that they stated that one should be evaluating exposure where exposure can actually occur, essentially put in a nutshell.

And so if we take that concept and address it in terms of the NAAQS, we should consider for the particular NAAQS, say it's an annual average, can exposure occur for an appreciable portion of that. Is it possible? And if it's not, is that a legitimate receptor?

And for the short-term NAAQS, say the 24 hour $PM_{2.5}$ or the NO_2 or the SO_2, one hour, could a person be at that location 98 percent or more than 2 percent of the days for $PM_{2.5}$? Could they be in that location more than 1 percent of the days in terms of SO_2? If it's impossible for someone to be in those locations, those should not be legitimate receptors.

So the examples that we'll be talking about will be things like railroad right-of-ways, controlled access highways, public roadways, inaccessible terrain, perhaps rivers with rapids where it would be difficult for someone to be there located. And we're suggesting that this be evaluated on a case by case basis.

So what we're now seeing is a hypothetical example of a facility along a river that is--also has a restricted access highway, say an interstate, to its north and to its east it has a railroad property, a railroad right-of-way. And what we display here with the yellow stars is what would typically be the set of receptors that would be used for modeling. And each of those red dots essentially indicate a low level source of emissions so that one would expect concentrations to decrease rapidly with distance.

So what we're showing here is a different way of looking at the receptors depending on the particular NAAQS. If we look on the river side, we have an annual $PM_{2.5}$ NAAQS of 12 micrograms per cubic meter. And at the fenceline for this hypothetical example, we had a concentration of 14 micrograms per cubic meter, which is above the NAAQS and across the river it's low, similar relief on the northbound side.

And the legitimacy of putting that on the other side of the river is that is someone--could someone possibly be on that river for an annual period. Now, if there was a--if there--if boats were docked there along that river, then clearly that would be a possible receptor, but for instances where it's not a river that's really accessible or used for that purpose, then it's not legitimate really to put receptors along the river.

The limited access highway, we know there's laws in place that don't allow people to loiter on the highway for nearly a large part of 24 hours, so those receptors perhaps should be on the north side of that. And even for the railroad property, we know we're not supposed to be there at all because you're going to be hit by trains and for walking the tracks, there are other laws regarding--you know, independent of the Clean Air Act or any other kind of environmental act that prevents people from being there.

And we're not necessarily talking about relative risk but just the fact that they're breaking the law being there. And should that--is there any legitimacy to putting receptors on the tracks, and we maintain that there is not legitimacy.

So now I'd like to go to a different subject matter, the second topic that we want to discuss, is that fugitive dust studies have shown over the years there are mechanisms when you have high concentrations of emissions from a fugitive dust source, say like from a haul road, that would affect the transport off site.

And what we're discussing here, not to go into details, but this was previously discussed by Dr. Chatten Cowherd at the EPA's 10th Modeling Conference, and essentially there are three aspects of this that we want to reiterate.

Number one, roadway dust from industrial sources is different than standard highways in that you would have individual sources that are moving along a line rather than a whole string of vehicles that would actually represent a line source, which is the way we model it in AERMOD.

And so there are refinements that could be applied that account for that transient nature. And the indications of previous studies have indicated that within the 100 meters you could have a sizeable decrease in the modeled concentration due to the lateral dispersion and the long wind dispersion of a moving source.

And the two effects that affect off site transport are the agglomeration of the particles when they're in very high concentration so that the $PM_{2.5}$ will essentially stick together and become PM_{10} and some of the PM_{10} size or the coarse particles would agglomerate to some extent and get out of the PM_{10} range. And that's been shown to have an effect for very high concentration sources which are associated with fugitive dust.

And if the sources--if there's buildings and vegetation near the sources, it's also been shown that the vegetation especially has a large effect on scavenging the dust as it begins to move off site, and that can be a very large effect. And the rate of scavenging is much greater than it would be say estimated by just using the standard deposition algorithms in ISC, which dates me--in AERMOD, which uses the same methodology as ISC used, by the way.

Okay, so just some pictures of what we're looking at in terms of intermittency. So on the left we see a representation of a roadway segment and how it would be modeled in AERMOD as a series of line sources. And here we see that would represent something like the roadway we see that's below it where you have, you know, a string of cars or vehicles in a row so that a line source fairly represents it.

And on the right side we see something more like a haul road in this case. It looks like some kind of a dirt road in a rural area where you have very intermittent traffic, and so it's really a traveling point source, so more of the representations say that FAA uses in modeling aircraft where it looks at individual moving sources might be more appropriate than using a line source in AERMOD.

For the second effect, this is an illustration of--we can see vehicles had traveled along these roadways and we can see the residual dust and we can see the transport as it approaches the vegetation. But we also can note that the vegetation--there's the ability to scavenge out much of those emissions. And if one has a roadway that is--where there's quite a bit of vegetation surrounding it on one side or the other, we should be able to account for that in the model so we're not indicating off-site transport of the $PM_{2.5}$ and the PM_{10} and account for that degradation.

So where are we now? Well, since the 10th Modeling Conference, AISI and Dr. Cowherd have presented the concepts to OAQPS and there have been further discussions at annual workshops. And EPA has indicated at that time they aren't really interested or think it's appropriate to actually change the AERMOD because that's--in other words, if we want to evaluate these effects we should look externally to AERMOD to figure out how to perhaps characterize the emissions in a more exact way that would account for these types of effects. So an industry group is currently-- presently investigating the feasibility of incorporating these concepts into refined modeling procedures for fugitive dust sources.

So what's going on right now is we have current activities to review the literature and the basis for these theoretical studies and see what types of degradation factors are appropriate. We're looking at the characterization of the fugitive dust emission reduction factors, that is what parameters are important to take into account in calculating those factors, and then also developing a conceptual design of how they could be implemented in AERMOD into either pre or postprocesses that wouldn't affect the dispersion calculated in AERMOD, but it would essentially accomplish a correction to the emission rates that are being simulated.

So possible future activities, then, would be to design and implement the algorithms, identify existing field studies that might further support the use of the algorithms, and if deemed necessary perhaps develop more field data.

So what are we talking about in terms of the requests based on these types of effects? Well, essentially we suggest that the Appendix W discussion regarding receptors gives some--a lot of the users and regulatory authorities to consider the potential for public presence either by physical barriers or by other regulatory or laws that prevent exposure in critical cases where the receptors should not legitimately be placed, and perhaps at a minimum at least Appendix W should provide some indication that local regulators in regions should have the ability to kind of be free thinkers, not be lockstepped.

As was noticed previously in the placement of receptors by EPA when they said we don't want to use the puzzle book anymore as the way to do things necessarily, we would suggest the same thought process should go on in the placement of near-field receptors.

We'd also suggest in the discussion in Appendix W regarding fugitive dust emissions that, again, flexibility be given to the characterization of the sources to account for factors that are not addressed directly in the model such as discussed here. And that is the end of my remarks. Thank you.

Mr. Bridgers: Thank you, David. So next up we have Zach from NCASI, and I'll let you announce all your coauthors.

Mr. Emerson: Hi. I am Zach Emerson from the National Council for Air and Stream Improvement for the pulp and paper industry. We call ourselves NCASI. Coauthors on this talk are Tim Hunt with AF&PA and Dave Heinold, who just spoke.

I'm going to talk a bit today about what are some of the impacts of data handling on modeled impacts. I'm not a modeler. I work in emissions data and emissions measurements. But what we see is that a lot of times as modelers pick up an inventory or pick up emissions data, they don't consider some of the underlying uncertainty that went into that data when evaluating what the impacts are. So hopefully what I'll talk about today can be useful going forward.

I'll talk about some measurement uncertainty and what it is and what it means, and then a specific example of measurement uncertainty due to measurements that are really close to zero, really close to the detection limit. The example I'll use will be $PM_{2.5}$ emissions from paper machines, and then we'll look at some of the modeled impacts of different decisions on how that data is handled. I'll talk for a second about emission variability at the end, but I think the next speaker will hit that more.

So with measurement uncertainty, measurement-- obviously when you take a measurement, you've got some uncertainty in what the actual number is. So if you look at measurement at some arbitrary level here--we'll say that's at the practical quantitation limit, PQL--it will have an uncertainty that's that dotted line.

It's a Gaussian line really, and really the error in that measurement is such that you've got 99 percent certainty. If you've got a number at the PQL or a number--a result at that number, you've got a 99 percent confidence that it's under that curve. Each method will have its own curve really, and really every test event will have its own curve based on how confident you are in the result.

You can see as you move this curve closer and closer to the blank level--in this case, we'll call the blank level zero--then you'll reach a point where this curve will overlap with the zero result. The point at which that curve exactly hits zero, that's actually what the method detection limit is. That's the point at which you have a 99 percent confidence that your number is statistically different from zero.

And this is really the minimum number at which you even know that a pollutant is there. If you're doing a measurement for PM and you're below the method detection limit, then you have no confidence that PM is actually even there.

The PQL is another statistically defined term. That's the limit below which you have any confidence in that value as a number. Values between the MDL and the PQL, you know it's there or you have 99 percent confidence that the material is there, but you don't have 99 percent confidence in the actual number itself. You just know it's there. And these are all kind of classically statistically defined.

So this matters when you've got low pollutant level concentrations. You could see going back that there's a spread in that curve, and if you're closer to the lower level, then that spread--the spread is usually a constant number, then it's a high percentage of your actual response. So if the level is close to the detection limit, your uncertainty may be 50 percent or higher of your actual measurement.

So as I said, data below the MDL, you can't even say that the pollutant is there. Now, when you're dealing with PM, if you measure long enough, you'll probably measure something. You can always get a number if you're measuring a filter or measuring the goo at the bottom of the beaker.

So the example that we'll talk about are paper machines. And these are processes that are usually located inside of a building with a lot of different vents. The vents have high flow rates with very, very low concentrations. They're also usually relatively low-- they're low relative to boiler stacks, obviously. And they have a high flow rate, low concentration. They're usually relatively cold. So they can have a pretty high impact on the model results.

Although this kind of analysis really has applications for a lot of different kind of sources, a tank farm or some other stuff, anything where the emissions levels are thought to be low, and the actual emission source is low or close to the fence line, then it'll--this will matter.

Here's some example test data. I hope that you can tell from the colors. So the orange test data, these are all measurements on different vents on a paper machine. In this case, there's eight vents although the machine itself probably had more, and they ran for a number of hours, and this is the collected mass at the end of that run.

So the orange results are all cases where the analyzed mass at the end of the run--this is $PM_{2.5}$. I didn't say that. All the measured mass at the end of the run was less than the method detection limit as defined by EPA. And the other results are all cases where the mass was greater than the detection limit, but still less than the PQL.

So you can see that for these examples there's one that there's not a lot of mass collected at all and most of it is less than the detection limit. This is also our--kind of our response because a lot of times when we bring these issues to a modeler, they say, well, get better data. Well, these are cases where we went 8 hours, 4 hours, 16 hours on a paper machine. And this is--it's pretty good data. We just--there's nothing there.

So then when you roll all these results up--these are all vents on a--say on a single paper machine. When you roll all these results up, you'll get an emission factor for the machine, which then you figure up what the emission rate is. In this case, we'll look at tons per year. Let's see if I can move that. Okay.

So in this case, we took the results from--this says a small paper machine and a large paper machine. Really that's a small--that's a machine at a small facility and machines at a large facility. The large facility--this is probably four machines here with an annual production or a daily production rate of about 1800 tons per day of either tissue or linerboard. The small facility would be about 400, I think is the number.

So look at, in this case, this tissue and linerboard. So I'll go ahead and direct you for the tissue machine. If you--this is the impact of different data handling procedures. If you look at the previous data on the previous slide and everywhere that those result less than the MDL and use the zero, the annual production rate--or annual emission rate is $PM_{2.5}$ or 1.4 tons per year.

For--if you use at the measurement detection level, which a lot of regulators will tell you would be what you should do, you would get about 30 tons per year. If you use the detected mass--that is, you use the actual number that's here and that's probably your tester, that's probably what they'll do if they give you the result--you'll get about 18 tons per year.

You can see there's a big spread in the difference here for tissue. It's a lot more dramatic for tissue than for linerboard, although it's still--this is a pretty big difference. If you look between 2.4 and 3.6, that's still a 50 percent difference relative to the data handled with MDL equal to zero.

Now, for the large machine, because really all we've done is scaled up the emission--or scaled up the emission rate based on the production, the impact is about the same. You go from about 6 tons per year to 140 tons per year for the machines at a large facility for a tissue mill.

So why does this matter? Okay. So for--a facility, a paper mill, has a lot of different processes. It's got the power to run the unit. It's got the processes to make pulp, to reclaim the chemicals in the process, and then to actually make the paper.

The actual emissions associated with--this is a--these are for the tissue mill. To actually make the fiber or to--I mean, excuse me, to make the tissue, it's about 8 percent of the total emissions for the facility with the balance being 92. That comes from power, recovery, and other processes.

When you put these into a model facility, and this was done by Dave Heinold, because of the--one, because of where the machines are and because they're low, then they can have a pretty big impact on what the actual model results are.

So the design value concentration--this is the annual 2.5 impact--I've lost my cursor--due to the machines is about 60 percent. So it's a big difference between 8 and 60. And one, this is due to the fact that these are usually close to the fence line and they're pretty low.

So why does this matter? So we've got--these are our model design concentrations for a variety of facilities, large and small tissue and linerboard. The purple bars in this graph are the rest of the facility. This is the fence line impact for annual 2.5 due to the rest of the facility, and then the bars--the blue, red, and green bars are the--due to the paper machine--or due to tissue or paper machines under different handling procedures.

So the blue bars, if you just took the MDL, used it as zero, or took all the data that's less than the MDL and used it as zero, you can see that there's a big difference between the blue and the green bar. I'll point out that this is--you go from about 1 to 20. That's bigger than the actual standard. So your uncertainty in the impact is bigger than the standard.

So even for a small tissue mill, the impact is between less than 1 to about 2½. If you've only got 2 to begin with between the background and the modeled impact, then that will be important. And the linerboard, as I said, is--the impact is smaller, although still significant.

Now, EPA released guidance in 2011--this was a memo written by Westlin and Merrill--taking data from the Boiler MACT exercise to figure out what the detection limits are, and in that process outlined that you shouldn't use data that's less than the MDL for anything, really for any regulatory action, so we think it's appropriate to kind of exclude it on this basis.

I'll talk just a second about emissions variability. I think the next speaker will hit it more. I just want to point out what emissions variability actually is when you look at it. When you model at maximum concen-tration--this is CEMS data. This is hourly SO_2 CEMS data for recovery boilers. It's actually two recovery boilers ducted to the same stack, so really the variability should smooth out a little bit.

The permitted value for this stack is 800 pounds per hour. So the max here that they ever saw was about 800 pounds per hour, but the mean was 80. The median was 7. So you can see it's a big impact if you take that permitted value. Even if you take the 99th percentile value, that's still one and a half times--or the max is still one and a half times the 99th percentile value. The 99th percentile value is one and a half times the 95th percentile value.

So we think it's still very conservative and very protective to use the 99th percentile or 95th percentile emission rate instead of modeling permitted values. And that's not modeling actual. That's still modeling a very high rate relative to where they actually run, but it's just a little more reasonable when you put that into the model.

So some of our conclusions, you can't ignore the uncertainty and measurements. You can't take that number as gospel and pass it forward when you're doing these modeling exercises. As Bob said, the models themselves have an uncertainty or assumed uncertainty of 10 to 40 percent, and the measurements probably have at least that. And if you're up against the standard, if you're one over the standard, you really have no confidence that you're actually over or under the standard.

So our recommendations are that the update to Appendix W should include some guidance on how to handle data or the AERMOD implementation guidance should include that, and the guidance should provide some flexibility to regulators and the modelers in how they handle these data, especially for measurements that are really low. We think that those measurements should be excluded under previous EPA guidance; and also some guidance on how to handle sources that are either intermittent or highly variable. And that's all I've got.

Mr. Bridgers: Thanks, Zach. To that end, yesterday, I was always wondering why we were running ahead of schedule. A few of you heard me make the joke that it was because we weren't having Q and A. That was the odd thing. We always have that.

But the other thing that was oddly different yesterday is there was no clapping. So that was also just unusual; I guess formal public hearing, or at least all the public hearings I go to are pretty drab. No offense to any of the speakers, especially the EPA speakers from yesterday morning.

Okay. Bob, are you really on the schedule again? Is Carlos--are you presenting or is Carlos presenting?

Mr. Paine: We're both presenting.

Mr. Bridgers: A tag team; okay, awesome.

Mr. Paine: Tag team.

Mr. Bridgers: So AECOM is going to offer some comments.

Mr. Paine: And ERM.

Mr. Bridgers: And ERM, excuse me.

Mr. Paine: Okay. I'm back, Robert Paine from AECOM, but I'm going to have Carlos do most of the talk. I'm going to--we're going to talk about EMVAP, the Emissions

Variability Processor, and an application, sort of a hypothetical application of that tool, basically what this slide says.

Especially with very short term ambient standards, the issue of very short term emission rates comes into play, and that's what sort of made us design this tool because we foresaw this issue back even before the 2010 ambient standards were promulgated.

So intermittent sources do present modeling challenges, and obviously if you assume a peak emission rate all the time, we know that you will overpredict the concentrations of a model. Now, the Technical Assistance Document for modeling does provide that for existing sources actual emission rates should be modeled.

And that reminds me that in the proposal for modeling off-site sources, there are three factors in the proposal, and only one of them has been changed from allowable to actual. They all should be made actual. There should not be the word "allowable" in that table for background sources. So that should be fixed in the final proposal, or in the final rulemaking.

But now let's go back to EMVAP. We're going to show how an actual distribution of emission rates can be turned into a way to model the variability of emissions in a model. Here's an example of a time series or 8760 hours of an emission source. And you can see that the emissions can go up to over 130 grams per second on this graph.

We turn that into a cumulative frequency distribution in the next graph, and this is what we use as a tool to provide input to our model because we basically put on several boxes, each with a discrete amount of frequency. What you can do then is you can model each of those boxes separately in a model and put them on a shelf, put them on--and then access those concentrations in a random number generator processor called EMVAP.

So the basic--the basic approach is to characterize this emission variability based on actual data or data from a similar facility and then use that as a tool to select the concentration in your processor. And notice these boxes are such that there's room on top of these individual bars so there's some conservatism in the EMVAP processor, also the fact that we--if we randomly select the high emissions.

Usually high emissions are clustered, and with the form of the standard being, you know, the highest hour of a day, we tend to spread out those high days--those high hours over several days in the EMVAP processor. So the conservatism was both in the characterization of the empty space above those emissions in each box and also the clustering.

So EMVAP is--you may have heard from other presentations is a Monte Carlo model that randomly takes those boxes after you run the model for each of those categories and puts the other simulated annualizations of the emission--of the concentrations at all the receptors you'd want to model and then applies the appropriate summary statistics for the one hour SO_2 or NO_2 standards. You can also take sources that do not vary as separate runs of AERMOD and add those together in EMVAP.

You can also--let's go back to this slide on the--if you model the actual emissions and then you compare the results to modeling EMVAP, you can determine what is the percentile of all of your results that would be conservative. And it turns out that the 50th percentile is often satisfying that requirement.

So for example if you ran 1,000 simulations, annual simulations, you might get a maximum daily one hour result that varies according to this simulation up to 1,000. If you then do a cumulative distribution, we often find that the 50th percentile is conservative relative to modeling actual hourly emissions, and that would be the result you would pick from the EMVAP simulation to determine the design concentration.

The available information on EMVAP is freely available from the EPRI web site. We've seen this link in several applications. You can also get SCICHEM for example from this or the distance debug AERMOD model. We presented this last year in much more detail at the modeling workshop, and there are peer reviewed journal articles available in the December 2014 issues of both the *Environmental Manager* and the *Journal*. I'm going to then provide Carlos for the rest of this presentation.

Mr. Szembek: Hello. I am Carlos Szembek. I'm with ERM in the Boston office. And so I'm going to sort of talk about a different, an alternative, way of using EMVAP from this article that was discussed in the *Journal* for using the--for using permitted emission rates to achieve a permit.

So just a little history. In the past, EPA guidance has been to have--if you have an averaging time for an emission rate, it should be the same as the averaging time of the applicable NAAQS that you're applying it to. Hence the initial approach with the one hour standard was to--for an emission rate that would be also one hour.

It was put forward by EPA to have this idea of a critical value, and this critical value would be an hourly emission rate that would be--that would show attainment with the NAAQS. But there is a complication in all of this. We have--how do you actually use a one hour emission rate that would actually handle emission--highly variable emission rates?

So there were some caveats placed. And EPA expected it's possible for the states to develop control strategies that would account for the variability in one hour emission rates. And what was put forth was that this could be accomplished by actually having averaging times that are longer than one hour but still demonstrate the attainment of the one hour NAAQS.

Okay. So I'll read some of these. EPA expects that any emission limits with an averaging time longer than one hour might need to be less than the critical value to compensate for the loss of stringency inherent in applying the longer term averaging limit.

In terms of variability becoming zero, so constant emissions, the long term average would exceed--would equal the critical value.

If the frequent emissions above the critical value are sufficiently sporadic, they will have little effect on the longer term compliant emission rate.

But there's still a challenge, so how to demon- strate that with an hourly emission variability the longer term emission rate is still protective of the NAAQS.

EPA has this example for dealing with that. So it seems the variable patterns after the control measures would still be the same. You would first actually calculate what that critical value was, so in this example, 600 pounds per hour.

You would then analyze your emission distribution and find the 99th percentile of your one hour emissions, in this case 800 pounds per hour. You would then obtain the percentage of the--it should be 30 day rolling average emission rates, and it would be 720 pounds per hour.

You would then find the ratio of those two numbers, in this case .9, and multiply--use that to scale the critical value to 540 pounds per hour, and that would be a 30 day rolling average limit.

So EMVAP--the EMVAP approach is an alternative to this EPA approach that actually provides additional confidence in protecting the NAAQS because it actually is a modeling demonstration and it encompasses by design the emission variability.

So here I'm going to discuss quickly the stepped process of how to apply EMVAP. Some of these I'll actually go through rather quickly and you can kind of review the nuances in the slides later. So--but it's a four step process.

So first you would actually find the critical value. Then you would--you would analyze your facility's emission distribution that will be used in EMVAP and then run AERMOD to generate inputs into EMVAP, and then finally you're ready to run EMVAP to find out what your compliant emission rate would be.

Okay. So I'm not going to run through all the details here, but this is one way of obtaining the critical value, so this would be a straightforward AERMOD run with a constant emission rate that would show compliance with the NAAQS.

Then you would look at your emission variability. So in this case, you know, you can see it's a highly variable emission rate. And, you know, here we've marked the long term average of 97.7 grams per second. And it's worth noting that here actually the peak emission rate is more than twice the long term average.

You would then--as Bob showed, you would organize those in a cumulative distribution. And here we actually did more gate points to establish the bins that would be run on these boxed emissions. We've also marked where the critical value is in relation to these emission rates.

Then step 3, and you would actually now take each of those bins that were generated here and have a separate AERMOD run that you would then run at just 1 gram per second and would later be scaled up in EMVAP.

And then run--here you would be running EMVAP in a default mode--there was a critical value mode, but in this case you'd be running it in default mode, and using that distribution of the boxed bins. And as long as you can--the main issue here is you're confirming that the results at the 50th percentile are in compliance with the NAAQS. You can then proceed.

And you'll note here that EMVAP adjusts the emissions, that if it didn't work, you would actually have to adjust your emissions downward until you showed compliance, so it sometimes can be an iterative process. For this example, we're assuming that the run actually did show compliance at the 50th percentile.

So what we're showing here is that the long term average is just sort of noted here to show that it's actually below the critical value that was found. And one thing to note is that what we're--what we're, you know, highlighting here that there are only three bins that are at or above the critical value. These will actually be--you'll go forward and using those for the permit language.

So to kind of generate this coupled short term limit for a hypothetical source, and I'll kind of just read through this, with a critical value emission rate of 120 grams per second, suppose the bins at or above 120 grams per second as shown were 200 grams per second with a 5 percent bin width--and so you can see that. That's the tall, slender one. Then we have--the next bin is 150 grams per second at 10 percent bin width. And then the third is at 120 grams per second, which is our critical value, and that's also at 10 percent bin width.

So the remaining lower emission bins have emission rates below the critical values. These actually tend not to drive the main exceedances of the NAAQS. And as you see, as Bob had noted also, you know, again there's conservatism, so there's a lot of area above the curve captured by these bins.

So for this case the permit language would be as follows. The hourly emission rate will never exceed 200 grams per second. The hourly emission rate will be at or above--or at or below 150 grams per second for at least 95 percent of the hours in the long term period. The hourly emission rate will be at or below 120 grams per second for at least 85 percent of the hours in the long term period.

All right. So the overall conclusions, the two-- the second and third are main take-aways--SO_2 and NO_2 one hour NAAQS are probabilistic, so EMVAP is consistent with that approach. It's a probabilistic model. EMVAP can be used with an emission distribution with a rigorous modeling approach to demonstrate that a coupled short term limit would be protective of a one hour NAAQS standard. And again, here's the source for EMVAP, and our contact information-- four, three, two, one. Okay.

Mr. Bridgers: Well played. So we have two more presentations before our first break in the morning. Both of these are by CPP, and first up is Ron Petersen here.

And I hope this works, Ron.

Dr. Petersen: Good morning; Ron Petersen, CPP. I'm going to talk about building downwash, some problems, solutions, and what I perceive as maybe the next generation downwash model.

Basically, why is this important? Well, let's look at--we've got, you know, the needs of society, the environment, industry, and really to have all these needs, stakeholders, reach the best decisions, you need an accurate model. It's someplace in the middle. You want it to be the most accurate so you can make the best decisions.

And kind of an overview of some of the problems with building downwash right now, the theory is based on research done before 2000, basically. The original research used a limited number of solid building shapes.

Schulman and Petersen at the 10th Modeling Conference presented some information illustrating some of the problems with certain types of buildings, long and wide buildings. The theory is not suitable for porous, streamlined, wide or elongated structures.

CPP's evaluation recently has identified some problems in the theory. And there's also some recent and past model comparisons with observations that show problems in the downwash arena.

So some examples here of these problems from the

10th Modeling Conference, and Lloyd Schulman presented a very interesting talk at that conference, and you can't really see the charts from that, but in summary what he found that for wide buildings he got factors of 3 to 14 increases due to building downwash when the width of the building was four times the height. For long buildings, he had concentration increases of 4 to 10 when the length was four times the height, and that was at GEP stack. So you were getting significant concentration increases. He also mentioned an ALCOA field study where the model was overpredicting by about a factor of 10 for a very long, big building.

More recently Keith Baugues at IDEM showed some results where AERMOD was overpredicting by a factor of 2, and also when paired in space and time very poor correlation with field observations. And again, these were relatively tall stacks compared to the building, so could we have a building downwash problem going on here?

A while back AECOM had a field study where they measured concentrations on a nearby high-rise, and the model was overpredicting by about a factor of 10. And when equivalent building dimensions were used in place of BPIP inputs, the concentrations still were higher by a factor of 4. When you actually ran with no building inputs, the model still overpredicted by a factor of 2, which suggests really in this case the plume was probably escaping the building downwash zone entirely, so due to the plume rise.

So, you know, I kind of was thinking what's causing some of these problems. So I got to digging into the theory that's in the model formulation document written by Schulman and Scire, and it's based on a technical paper, so there really is no formulation document. It's a technical paper.

So why is the model overpredicting? We've got--one of the problems, we create these artificial buildings. You can see the blue shape there. That's the building in plan view. BPIP creates that gray building that's much longer than the real building.

And so what happens, the wake starts growing much further upwind. And the point that we're going to have the maximum turbulence occurs much further downwind than the real situation. So you're really having the wake being characterized incorrectly in the model.

Some of the turbulence calculations in the wake are flawed. And I'm not going to--I just have the equations there for reference and documentation, but they're in the paper. Basically what the model is assuming is that the turbulence enhancement, or the building downwash increase, is constant all the way up to the top of the wake. And that's really an unrealistic assumption. And it's making an assumption of uniform approach flow, constant wind field with height.

This is kind of what we found here. The top slide shows what's going on in PRIME. And you've got the wake growing. You can see that white line. That's the calculated wake height. And the enhanced turbulence doesn't start until the lee end of the building where the red zone is. So the maximum turbulence starts occurring right at the downwind edge of the building, and it goes all the way up to the top of the wake, which is the white line.

What we found in reality is that the enhanced turbulence zone really doesn't go much above the top of the building, and it decreases quickly back to ambient levels. So this can explain why some of these taller stacks are getting higher numbers. They shouldn't be really impacted much by the high turbulence in the wake zone.

Another problem that we found in this area is that under stable stratification the turbulence in the wake is enhanced by a factor of 10, while under neutral conditions, it's only enhanced by a factor of about 5.7, unstable by about a factor of 3. And so that really is not--there's no evidence supporting this relationship is a function of stability. Everything that was done was done under neutral conditions to develop the theory.

We did a little limited research on our own at CPP, put in a building, a 1:1:2 building, which means it's two times as long as its height and its width is the same as its height. We measured the turbulence enhancement in this plot. What you see here on the horizontal axis is the turbulence increase is a function of distance downwind of the building. You can see that black object there.

And the maximum turbulence enhancement you can see is below the top of the building where that value of 1 is. Above the top of the building you have very little turbulence enhancement. It goes back to ambient very quickly, so kind of supporting that little chart I showed you based on wind tunnel measurements. And the decay of turbulence enhancement is much quicker for this particular building than the theory.

We also did some large eddy simulation runs using CFD. And--well, that should have been running, but--let's go back to that. Yeah, you can see the blue there is the velocity deficit zone in the wake of that building, and you can see it kind of confirms the wind tunnel measurements. It's the same building basically that we simulated in the wind tunnel. And it shows most of the turbulence enhancement or velocity deficit is occurring below the top of the building.

So what are the problems we found in the downwash model? The streamline calculation comparison that's in the model formulation document--when we went through the calculations the example showed the streamlines going downward. When we actually followed the procedure in the manual, we calculated they should all be going horizontal, so there seems to be some problem in what's written in the document, so I'm not really sure what the model is actually doing for that case, but the example doesn't match what's in the paper.

The second one is there's a discontinuity in the streamlines right at the leading edge of the building. Right there in that figure where you see the zero on the building there's a discontinuity. Right at zero the slope is a factor of 4, and immediately upwind of that is a factor of 2. So there's a factor of 2 change in the trajectory of the plume right at that point.

Another weakness is the streamlines for all structures are calculated assuming the buildings are solid. So like a lattice type structure where the flow goes through it--you know, oil platforms, things like that--the flow will more likely be horizontal. A real quick fix to that would be to change the streamline calculation in the model to be horizontal, and that would improve model performance dramatically for that situation.

The top picture is a lattice structure. The middle one, no structure, and you can see is very similar to the lattice structure. And the bottom one is what it looks like in AERMOD. It assumes a solid building.

So the solutions really to these problems--the short term fix really is--one that's being used currently is equivalent building dimensions where you do a wind tunnel study to find the building dimension to input to get the correct dispersion.

I should mention that even that is not fixing the problem completely because, as you notice, there are some theoretical problems, so even with the equivalent building dimension you're going to tend to overestimate the concentrations for the reasons I just went through.

So really the next generation is to develop an improved AERMOD model, which actually--or which actually this would be the PRIME algorithm, which also is in SCICHEM, and you would have to do some modifications to BPIP too to make everything work correctly.

To do this, you'd really need a collaboration between EPA and industry to get this done in a quick manner, and it's something that I really think should be done outside of the current regulatory framework. It's just something that should be done to improve the model. Current science--as was mentioned earlier, we need to advance the state of the science. That should be the goal of all us here.

The short term fix, as I mentioned, is equivalent building dimensions. Those are fairly expensive, time consuming, but that is a tool that can be used in the interim to give better predictions.

If you find you have a problem--lattice structures, long buildings--really the only fix right now is equivalent building dimensions, because right now for these types of structures--hyperbolic cooling towers let's say--the model is overpredicting by a factor of 2 to 4, 4 to 8 for short buildings with a large footprint, 2 to 3.5 for lattice structures, and 2 to 5 for very long, narrow structures. So currently the model has some significant overestimates going that the only way to correct is through a wind tunnel modeling study at the current time.

So of course why does--equivalent buildings don't solve the problem, they do help a little bit, but why do they help? You can see this picture here on the bottom is a very long structure. You can see the high turbulence zone right at the lee edge of the building, so a plume could hit and go off that building, immediately hit that zone, be mixed with the ground. That's kind of that yellow color in the picture.

The equivalent buildings we usually find are much shorter, and so the wake height is much shorter, which means when you put that into the model, even though you're still intersecting the wake zone incorrectly, the concentrations hit further downwind or lower. So it is an improvement, but even if the model were fixed, you'd even get lower concentrations than using EBD.

Kind of just another picture, this is the BREEZE downwash analyzer that we can kind of visualize what's going on with the building. It kind of shows the building and then the BPIP input, which is in blue, and you can see how much bigger the building is.

Well, we ran that case in AERMOD just to see what it looked like with this building and then with an equivalent building, and here's kind of what the plumes look like close in. You can see the real building, that blue line there is the wake, top of the wake, and you can see the plume, 1 sigma-z, how much wider it is than with the equivalent building, the top picture, because on the top picture the plume is just slightly above the top of the wake, so you're getting very little enhancement in the plume, further illustrating what I was saying here how the model is overestimating the enhanced dispersion downwind of the building.

Of course, there are cases where the model is underpredicting, so you really need to--you know, for a sustainable type of situation, you want to cover everything. You've got to make good decisions, bad, or--you know, overpredictions, underpredictions, whatever. You've got to go both ways.

The corner vortex is an issue where the model is probably underestimating by a factor of 2. Upwind terrain effects are not in the model either. And the model is probably underestimating by a factor of 2 to 6 for that situation. So these are all things that could be developed. These latter two are probably more complex or are going to take a little more research, but the ones that we talked about first are pretty easy fixes.

So what do we do? Correct the bugs, fix the known problems, incorporate the current state of the science. We need to advance the state of the science, expand the types of structures that can be accurately handled.

We really need a well documented and verified model formulation document and code for PRIME, and a section needs to go into Appendix W that we can update things quickly and create collaboration with industry and EPA to kind of move on this path forward to develop a more accurate building downwash model. Thank you.

Mr. Bridgers: Thank you, Ron. And for our last presentation before the break, we have Sergio.

Dr. Guerra: Thank you very much; Sergio Guerra with CPP. In the last two presentations, we've seen new understanding of the science, and it's very important for us to figure out how we can incorporate that into the model as soon as possible so that we can continue advancing the science.

I'm going to be covering background concentrations and the need for a new system to update AERMOD, so we're going to go through what is in the guidance right now related to background concentrations, an alternative approach, and then going to Appendix W, what have we learned and what would be a new method to go forward.

So going to the draft guidance, Section 8.3, there are a few options that you can use to get your background concentrations. You can--if you have a source that is impacting the monitor, you can exclude the 90 degree downwind sector from the source in question, and that was--that's in the current guidance and it's also in the proposed.

The second one is a new one. You can modify the ambient data record with monitor--when the monitor is impacted by unusual events. I call them exceptional events, but these are things like the Canadian forest fires, like construction, things that are not reasonably controlled that can be excluded for other purposes in designing concentration calculations. You can do that by removing hourly or daily data or you can do it by scaling or adjusting, basically multiplying by a factor or adding or subtracting.

The guidance also talks about pairing monitoring and modeled data on a temporal basis. You can do it by season, by hour of day, and on rare occasions on like the paired sums approach, on an hour by hour basis. And then the fourth way is you can use a regional scale photochemical grid model to give you the background that you can use.

The first one, the 90 degree downwind sector, this is some peer reviewed research done by Doug Murray and Michael Newman, and they did exactly that for the Kincaid database for SO_2 and SF_6. And what you would expect is that when your downwind--your receptors are downwind from the source, you would exclude all those receptors and just take the receptors around it.

In the case of SF_6 there is nothing else that would contaminate those samples. However, we found--or what was found in this research is that the 99th percentile for example for SF_6 is 247, and then outside of the sector you still have 188. So it's a good idea, but unfortunately it does not work. Winds are not stable enough, and you do have some impacts outside of that 90 degree sector downwind of your source.

Excluding unusual events--I have to commend EPA. I've been talking about this for a while, you know, that we shouldn't be including exceptional events on the data set that is used to calculate the background. And EPA kind of responded to that by the draft guidance.

This is a satellite image from I think June 29 of this year when all these forest fires were basically coming down into the U.S. through Minnesota. In fact Minnesota had a lot of--along with other states had a lot of health type of events due to these forest fires. I was not in Minnesota at the time, but even in Fort Collins we got the effects from this plume. They say that you can run but you can't hide, so that's what happens in some of these exceptional events.

But why do we care about these exceptional events? We care about these exceptional events because they basically make a normal distribution be skewed, be stretched to the right like you can see here. And when you stretch that distribution you basically pull with it all your percentiles.

So whereas your 98th percentile in normal distribution might be 5 micrograms, when you have an exceptional event, that pulls the whole distribution to the right and it may be 10 micrograms, 50 micrograms.

This is from a peer reviewed journal article that I wrote for $PM_{2.5}$ showing that same trend. And as you can see here, anything above maybe 20 is statistically an outlier. For NO_2 I did the same thing and we see the same pattern. And for SO_2 I guess that's the most dramatic where you have very few observations that are really, really high, that are really pulling that distribution to the right and making the 98th percentile, or the 99th in the case of SO_2, be much more--much higher than what it would be normally.

Another thing that you have to consider here is that you need met data in order to address some of the--in order to subtract those days that are impacted by exceptional events or unusual events. But in many cases we do not have that. We have a monitor, but we do not have met data or a met station that is close by or representative. So an alternative method is necessary when there's no met data available.

Now, let's talk about temporal pairing. This is the current Section 9.1, Model Accuracy, and it basically says that models are more accurate for the annual standard than for the one hour standard and that we are reasonable in knowing the magnitude of the highest concentration, but we do not know the exact time or the exact location of it.

This is absent from the guidance as far as I could tell, and I think that if there's evidence to the contrary that this is not true anymore, that evidence should be released to the public. Otherwise, it should be included so that we know basically what are the limitations of the model and we can use the model correctly.

This is what a perfect model would do on an hour by hour basis. You have one monitoring observation the first hour, and if you have a receptor at that hour you compare the two and if they give you the same number, it would create this 45 degree angle, this 1 to 1. However, this is what we find in real life. This is again the Gibson generating station, a peer reviewed journal, Kali Frost.

And it's showing two things: first, that that is not following that pattern, so basically there is no relationship between the monitored concentrations and the predicted concentrations. And the second thing is that--a little bit more troublesome--is that when the monitor is showing really high concentrations, the model is showing very low and vice versa. So when we do pairings in time and space, you have to keep this in mind, are you comparing the right values or not, and according to this it's not the case.

This is the same article that I showed by Doug Murray and Mike Newman, and it shows the same pattern for a different station for the Kincaid database. And then they took it a step further and said, well, let's just pair them in time, basically any receptor. Let's try to see if any of the receptors match with the monitors, and it just had like a slight improvement, but obviously that pattern is not correct like it should be.

So how do we evaluate AERMOD? Most of us are used to seeing something like this. These are Q-Q plots. You rank them from highest to lowest and you compare the highest on the monitor with the highest with the model and then the second highest with the second highest and something like that.

So basically you compare them. You uncouple them from time and space. And that's how we evaluate the model because it's probabilistic also in that sense. We are looking at the probability of having an exceedance somewhere at some hour throughout your evaluation study and evaluation time. So basically temporal matching is not justifiable based on the information that I've seen, so it should be avoided.

And just a word about the photochemical grid model, it's something that--it's a complete different scaling, complete different parameterization, different assumptions, chemistry.

So we're not comparing apples to apples; it's like apples to oranges, watermelons. They are very different, so I think we should reconsider that, and even some of EPA guidance says that we should take photochemical grid models in a relative fashion, and you know, sort of remember we looked at this at some point--well, let's use these data for background. And we couldn't use it because it was way, way higher than the standard itself. So I think this might not be ready for prime time yet.

An alternative to pairing background and predicted concentrations--if we look at the probability of these two events happening, the 98th worst concentration from the model happening at the same time as the 98th worst concentration from your monitor, the probability is equal to the product of both probabilities, in this case .02 times .02, as long as both distributions are independent from each other.

And as I showed earlier, if we had something like a 45 degree angle pattern to the data, we could say that there is, but there really isn't. So in this case we had .0004 probability or a 99.96 percentile combined probability of those two events happening at the same time. This is obviously more conservative than the model if you do it for the 99th percentile, and it's also one exceedance every 27 years.

So I've been advocating for the 50th percentile. I'm glad to hear that for EMVAP it's working as well. But the 50th percentile is a good measure because it's not heavily influenced by these exceptional events. It's more toward the middle of the concentration and it's still conservative because it's being pulled just like the other percentiles. So when you look at that, the combined distribution and the combined probability is even more conservative than the form of the standard.

But now let's talk about the *Guideline on Air Quality Models*. It was published in 1978 and its purpose was to streamline and to create consistency across the country and across industry, basically one model for everyone to use. The critics back then said that the rigidity of the rules would inhibit innovation and would render the *Guidance* obsolete as technology and science advanced.

It was a very heated argument, from what I understand. It was very controversial at the time. I was not there. I was only 2, so I did not know enough about dispersion modeling, but nonetheless there is a record of it.

So what I'm proposing is in line with what the APM Committee has proposed, the establishment of a Technical Review Advisory Committee, TRAC, with the ability to evaluate, approve, and incorporate new methods without the need to undergo a long and infrequent rulemaking process.

TRAC would be composed of the leading experts from EPA, industry, and academia with one purpose, to evaluate new dispersion modeling techniques and incorporate scientifically valid methods to the regulatory model in an expedient manner. The APM Committee from the Air and Waste Management Association can provide a good framework for TRAC.

And why do we need to do this? Well, first of all because of timing. Updates to the guidance require a long and complicated rulemaking process. I mean it's taken us ten years to come here again. Current system results in a lengthy time gap between the proposal of new and advanced techniques and their implementation for widespread use. And again, the current mechanism does not allow for an expedient update of the model to incorporate fixes that we are finding like Dr. Petersen mentioned about downwash or new techniques.

Now, let's talk about the rulemaking process. To keep up with the new methods and science, EPA was supposed to update the guidance through the rulemaking process, basically through the formal public comment. Instead it gave us--and again, it's really hard because EPA has their plate full and they have a lot of responsibilities on top of that, so this is really stretching them to the limit.

So instead what EPA has done is issued nonbinding guidance or technical assistance--technical assistance documents without formal evaluation or public involvement. However, as the courts have said, if you have a nonbinding document that dictates what you have to do in order to get a permit, for all practical purposes that document is binding. So that's from the *Appalachian Power v. EPA*, D.C. Circuit, 2000.

But in reality--the court is another thing, you know, but what we have really done is we have circumvented the evaluation process that is supposed to go into the model, and many times we make changes to the model with good intentions that end up having very unfortunate type of situations or results.

The other thing is we need to take action because it might take ten years to update the guidance again, so we need to do this right now. The science is constantly evolving and we need to make sure that we promote the science so that we can incorporate it and we can improve it every time more and more.

And we need to recognize also that the current system is not working. It's not working for EPA. It's not working for industry. It's not working to protect the environment. And all the stakeholders need to take charge of this and be involved and collaborate because only by collaboration can we do something. We cannot wait another ten years to see what happens.

And then one of the main things is create consistency. For example there are some beta options in AERMOD, but at the same time there are many updates that have been made to the model. AERMOD was updated 11 times, AERSCREEN five times, AERMET six times, AERMAP three times, AERMINUTE three times. AERSURFACE even got its update. BPIP, like Dr. Petersen mentioned, hasn't been updated, but we're working on that.

And then another thing is there have been updates that have been enhancements like when we did not have a form of the standard that the model could give you, that was a very welcome enhancement. But there have been also bug fixes that are necessary, but there have been miscellaneous changes such as the change on calculating downwash above GEP stack height, that whereas it is true that there is downwash at 40 percent, what Dr. Petersen showed is that the downwash is being overstated, so there was not a proper evaluation to identify this problem and now we've created another problem by trying to fix a problem.

So basically it's not clear what changes can be made by EPA and what changes need to wait until the rule-making process. That's why we need something like the TRAC that I'm promoting.

And basically the Technical Review Advisory Committee would promote collaboration, share responsibility among all the stakeholders, result in a more efficient process and improve the timing of implementation of the science into the model. And this will create consistency, which is the goal of the model--of the Appendix W, excuse me.

So again, the APM Committee from the Air & Waste would be the ideal framework because the major players are part of it already, and as technology and science advance we need to evolve with it and we need to make sure that we can lead again, you know, because in this case we've seen models like ADM like used in Europe that are basically having better science than the one that we have here. So let's prove the critics of 1978 wrong and let's update Appendix W so it can work efficiently, as it was intended.

And in 30 seconds I have a summary of my comments. I'll just say about the 90 degree downwind sector, that's not solving the problem, unfortunately. Unusual events need to be excluded from the monitoring data and we need to make sure that we provide enough tools so that we can do that in an efficient way.

The use of the 50th percentile that I've been advocating for is one of those options that should be considered. And then the statement about model accuracy for long and short term averages should remain in the updated guidance, and otherwise evidence should be provided that it's no longer valid.

And the temporal matching is not justifiable because AERMOD's accuracy is suspect on a temporal basis. And background values from photochemical grid modeling should be reconsidered until we know for sure that it's something that is technically feasible.

And again, the main thing right now is we need to form a Technical Review Advisory Committee with the ability to evaluate and approve any changes to the model because that's an urgent need at the time. Thank you very much.

Mr. Bridgers: Thank you, Sergio. So we have reached the point of our first break. What I do want to say--I'm not going to talk for five minutes to fill the time, but what I am going to say, there's going to be some changes--slight changes to the agenda right before lunch.

We have received two requests for additional presentations that would be given, quote, during the oral comments. And so I'm going to have those two presentations right before lunch, and then we'll have the open forum immediately following lunch, and so that's just shifting that just a little bit. And that way for folks that happen to need to leave at lunch for an airplane flight or whatnot, they'll see all of the PowerPoint presentations that are going to be given.

And so with that, it is 10:00. I'm going to adjust the time, so we need to be back at 10:20. Thank you.

(A recess was taken from 10:02 a.m. to 10:20 a.m.)

Mr. Bridgers: So thank you for everybody coming back after the break. We're going to jump right into the presentations. Oh, just a real quick note. I know that there are some in this room that are looking for rides to the airport. And so there's a white drawing board over here that the ride list is starting to form, so if there are extra spots in cars that are going over to the airport, you know, see if you might help some of your colleagues.

Next up we have David Long. And David, I'll let you present your topic.

Mr. Long: Good morning. My name is David Long. I am an engineer with the American Electric Power Service Corporation, which is the technical services organization for American Electric Power. And this morning I'm going to talk about the use of data collected by the Clean Air Markets Division, or CAMD for short, in its application to air quality modeling. Under the one hour SO_2

standard, we're seeing a lot of this data source trying to be used, and there are issues with this data simply because of its design.

When the 1990 Clean Air Act amendments were passed, EPA was told to develop a program for monitoring $S0_2$ and NO_x from the utility industry as part of what was known as the--what's known as the Acid Rain Program. This data was intended when it was put together as an accounting program for the use and consumption of allowances that were assigned to the various electric generating units. The data management system is therefore set up to overstate emissions if there's any errors in measurements.

And the regulations implementing this monitoring program are found in 40 CFR Part 75. And they do go into a great deal of detail on how the monitoring systems are to be operated and what data is to be collected and reported.

Most of the data in CAMD is an hourly database, which for an hourly program people look at it and say, "Wonderful, we have hourly data." And the typical data that's reported includes gross load on the generating unit, typically in megawatts, but not all sources reported megawatts; SO_2 emitted in both pounds per hour and ppm; NO_x emitted in pounds per hour, pounds per million Btu and ppm; stack flow rate in standard cubic feet per hour, and that's a critical issue that it's standard cubic feet; a diluent gas,

CO_2 or O_2, so you're not always going to have CO_2 data, but with some of the changes coming based on the Clean Power Program, CO_2 probably will be added to the mandatory list; and then a calculation of heat input in million Btu per hour.

Obviously with all the data that's been collected over the past 20 plus years and reported, there's a massive database out there, and it isn't overall terrible data. For Part 75 purposes in fact it's very good data.

However, when we've tried to apply this to one hour SO_2 where trying to get an accurate picture of the emissions impacts on an hourly basis, the data have--we start to find the shortcomings in the accounting program reporting.

One of the other problems we have is for modeling to get the plume height correct, we need hourly temperatures to temporally match up with the flow rates and to back those standard cubic feet per hour values into actual cubic feet so the velocities are correct.

Guess what we don't have to report? Temperature. Even though the data is captured because we have to use it to take the actual cubic feet that is typically measured by the CEMS systems and converted into standard cubic feet, CAMD doesn't require us to report temperatures at this time. In order to get the modeling accurate, you need accurate temperature data, so that's another shortcoming.

Another shortcoming, as I mentioned earlier, this was set up as an accounting program. There are extensive missing data substitution--invalid data substitution protocols in Part 75. Basically you get a value every hour regardless of whether the CEMS are working correctly or not, and it may be substituted.

There are flags when data is substituted or even when it isn't to tell you that this is actually what the CEMS system read. When you go to CAMD and request an hourly data set from them, you don't get the flags typically unless you specifically ask for them.

So you're looking at this massive list of hourly flows and hourly emissions, and how much of it's actually what was measured and how much of it was actually substituted and may be substituted using any number of different protocols you can't tell just looking at the basic data set CAMD issues. And to find some of that, you either have to get into the flags themselves or go down to the sensor level data that is actually reported.

Now, one of the things that also is included in the CAMD data--and again, for accounting purposes, it's what's called a bias adjustment factor. Bias adjustment factors by their definition in Part 75 cannot go below 1.0. And these are based on RATA tests on the CEMS systems that are done annually or semiannually depending on the accuracy determined during the most recent RATA.

If the monitor is reading below the actual value and you flunk the calculation for bias, you then put in a bias adjustment factor on the monitor data greater than 1. If it goes the other way where the monitor is reading higher than the test method, you don't go below 1. It stays at 1 so you're again overstating emissions and consuming a few extra allowances under the accounting rules that were set up. It doesn't help us, though, a lot if we're trying to get accurate representation of emissions from the unit.

Another issue that I've hit on several times already, temperature data. We don't have that in the CAMD data. One of the interesting parts is for units that use dry control technologies, the temperatures vary much more wildly than you set on wet scrubbed units, although you do see temperature variations there too, and those can--and that can affect the plume performance in the model.

Temperature for the modeling studies, you can obtain it from several sources. One would be a state permitting or an inventory database. However, the temperature you get there is typically a single value and it's based on full load operating conditions that don't necessarily represent the hourly variation and unit operation. So you have no guidance on how to adjust the flow rates back from standard conditions to actual conditions based on that data source.

You can also go back--and one of the things that we've been doing with the data we've been putting together for the various state agencies we've been working with is we pull the sensor level data, where we do get the hourly values that were used to adjust the actual measured flows back to standard, and then we can bring them back up to the actual flow rates and get a more accurate representation of the exit velocities for the model. You know, obviously how you recognize the temperature data can impact the exit velocity and lead to improper dispersion if you don't get it right.

Flow calculations: now, here we see how the bias adjustment factor comes into play. You know, the bias adjustment factor is applied to the flow sensor data, not to the temperature data, the flow sensor. So if the flow sensor was determined to be reading low, it gets raised, and in this case it's probably an appropriate increase. But if the sensor is reading slightly high, it doesn't go down and that can introduce some error into the calculations.

Emissions, and I do have a slight error in this equation. I'm just showing a single application of the bias adjustment factor, and it's actually applied to both the value of the SO_2 concentration and to the flow individually, not as a single factor on the emission.

So you can get into a situation where if you have an error with--or I shouldn't say an error, but a bias adjustment on both the flow and the SO_2, you can significantly increase the emissions possibly in ways that aren't completely realistic. And again, if there's substituted data, it occurs at the sensor level, not at the emissions level. And substitutions can be very interesting.

If you have just a limited amount of missing data, typically the technique that is used is called hour before/hour after, which if the unit is sitting there operating in a stable mode, it's probably going to be a pretty accurate estimator of what happened during the hour or two of missing data. If the unit isn't stable because the load is being shifted, it's hard to say how that would impact you. But the impacts of an hour before/hour after are still probably going to be relatively minor.

The other extreme of data substitution is what's called the maximum potential concentration. And if you get that value thrown in, it can send you all over the place. And typically if you're just doing a real basic QA of the data, and just looking at it, you can see these because you'll get a--you'll be going along and all of a sudden you get a very crazy number showing up in the data set.

Some of the good examples I've seen of this are where you're going along with a unit sitting at a stable condition with an FGD system running, no evidence of any equipment problems, and the emission rate suddenly jumps from

.2 or .3 of a pound per million to 2 or 3 pounds per million or higher. So, you know, that can just totally drive an air quality model nuts when you throw that kind of a change into it. And exit velocities can jump to two or three times the design value of the stack and potentially go well into the very high velocities that you just don't design a smokestack for in most cases.

Hourly temperature data, you get into needing to select the proper sensors to obtain reasonable temperatures. If there's a problem with it, if you have too low of a temperature, it will again affect the dispersion of the plume and give unreasonable and incorrect results.

One other special case of data substitution, and this is where you see--where we've seen a lot of the MPC data show up, is in a start-up of a new stack when a wet scrubber is started up on an existing facility. Yes, some of the data is substituted using stack testing, which is a very good source of data when you have it.

But you don't have that from the minute the units are fired up in this case. It's some hours later, and you may not see it continuously for the several weeks it takes to get the CEMS systems up and running. So you end up with substitution based on the old stacks, which are unscrubbed, and it just is very crazy and you have to manually correct all of that data.

You know, the recommendations we would offer at this point is that the modeling group here at OAQPS should be involved with CAMD as we move into the fall as they're planning on revising the emissions data reporting program that's used for the utilities to report data and to work with them to try to include parameters that are needed to help improve our ability to model using their data set, because again, it's an hourly data set, and there are a lot of good possibilities of what you can do with that data. Sometimes it just takes a lot of work and quality assurance to make sure that the data is something suitable for use in air quality modeling.

And, you know, again, remember, this was set up as an accounting program, not as a program to collect data for air quality modeling. We're just using it for that. And we have to keep that in mind as we work with the data and QA it prior to it being included in an air quality model. Thank you.

Mr. Bridgers: Thank you, David. Up next we have Chris presenting for API. Chris?

Mr. Rabideau: Thanks, George. My name is Chris Rabideau. I am with Chevron, but today I am speaking as the chair of the American Petroleum Institute's Air Modeling Group.

First of all, I just want to--we appreciate the efforts done by EPA and the IWAQM3 group in developing the proposal package. We know there's been a lot of work going into that package. We also appreciate EPA's review and the consideration of all the API sponsored research that have gone into some of the improvements that are in the proposal package as well.

I also want to acknowledge EPA for opening up the annual modelers meetings to the stakeholders for at least, you know, one day for the last couple of years. That's also, I think, been very beneficial to some of the progress of the projects that we've been working on.

Also, since we had to do these presentations a couple of days ago to get them into George, obviously this is based on our initial review and understanding, which, you know, some has changed based on the presentations yesterday and interpretations of what we were seeing. So obviously you know we'll be following up with written comments. They'll be a lot more detailed.

So today's comments are basically what we had--what we saw at the beginning and at least what our recommendations are. Obviously based on yesterday some of these things are in there. We just want to make sure that--I just want to clarify that at the beginning.

As we undertake the process to revise Appendix W--and again, as many have said before, you know, this is ten years in the making--you know, we need to consider a structure that's more timely for the incorporation of the model updates. We should not be delayed for several years on model updates.

We know in the next year there's going to be some new, more robust NO_2 evaluation databases. You know, there's--two NO_2 studies being done right now that API has been supporting will be WRAP NO_2 drill rigging and the PRCI project that's going to be starting here very soon.

You know, these data sets are going to allow more rigorous evaluation of these models. And, you know, these data sets could also suggest that model refinements need to be done. And we need to have the ability to timely incorporate those technical advances.

And it has been suggested--you know, there's a tier structure which I'll get into, and also a subject that we had brought up at our last--API's comments at the last modeling conference of a technical advisory panel.

Again, some of the lack of clarity has been addressed with some of the presentations yesterday, but basically--first of all, I also want to make sure we understand that the low wind project that Bob Paine had mentioned and that was mentioned in some of the EPA comments was not just an API project. It was also supported by UARG, so it was a joint low wind project as well.

But we support the adoption of the U* option. Again, we understand the beta option,but the plan is to go to a default option with the promulgation of Appendix W. We still haven't evaluated LowWind3,but we plan to do so for our written comments.

With respect to the NO_2 improvements, again, we support the adoption of ARM2. Again, we appreciate API--or EPA working with API in getting that--getting that into the system. However, it's unclear as to what steps would be required to allow the use of a lower in-stack ratio.

As we saw yesterday by Rich Hamel of ERM, you know, only 4.5 percent of the in-stack ratios in the database are greater than .5. So if an applicant has site specific in-stack ratio that is less than .5, we're going to suggest that adjustments should be allowed, you know, without the need for additional approval or going through the modeling clearinghouse. There should be--there should be a way to set that up since, you know, 95 percent of what we think is going to happen is going to be less than .5.

We support the classification of ARM2, OLM, and PVMRM2. Again, I think the understanding there has been clarified, but we haven't had time to evaluate PVMRM2 so we plan to do so for our written comments.

Again, and I think these issues have been addressed in a number of the presentations before mine, but, you know, there are still some outstanding needs with AERMOD. There's obviously the building downwash issue that's been touched both on the long and narrow as well as the GEP height issue, also on the background. And again I think some of these things have been addressed and some of them have not been.

Again, with modeling nearby we want to make sure that the actuals are being used, not allowable. Also, we think there should be some more flexibility in the use of monitoring data to not just characterize the contribution from nearby sources but also potentially in the place of modeling. And then also we want to make sure the background excludes anything that's impacted by exceptional events.

With respect to CALPUFF and long range transport, again, I think we agree with some of the statements made that I think EPA is underestimating the times when a long range transport is going to be required. I think there still is--there's still a need for a long range transport model. We have a concern that there is no consensus on acceptable--on an acceptable model, which we feel there probably will be significant delays in permitting.

I think we've got a typo here. In the absence of a preferred long--we're suggesting CALPUFF version 6.42 that has the advanced chemistry, you know, should be allowed as a refined screen model, again in some of the work that we have done in the past on it, putting--improving the chemistry in CALPUFF. So we'd like to see that potentially be used.

Again, also, if there is--I know there's been some prior concerns with CALMET, so obviously WRF/MMIF can also be used to be--to input into CALPUFF. So hopefully the use of CALPUFF should not be an issue there.

With respect to the ozone and secondary $PM_{2.5}$, a lot of unknowns. We know that there's going to be, you know, further rulemaking on this. So with MERPs, you know, in principle we think that's a reasonable approach. However, we really can't comment on how it's--what the numbers are because we haven't seen anything yet, so obviously there's a lot more to come on that, so it's hard for us to access the appropriateness of whatever, you know, this approach is going to be, but in principle the idea of a MERP does sound like a good idea.

With respect to the first tier, again, there's questions about where is this--where does the data exist, because obviously there's still some more guidance that needs to come out with the proposal. There's still not a--I think as somebody said yesterday, it's still--the story is still n not complete yet. It's still out. So we still have a lot more details that need to be--need to be addressed on this.

With respect to the second tier, again, what's classified as only, you know, saying--being used in special situations, which is an application of more sophisticated chemical transport model, I think--again, I think this is going to be used more frequently than EPA is presuming in the proposal.

And I think obviously developing photochemical grid model databases from scratch is costly and time consuming. And again, that's--for the ones that are going to need to do the permitting that's obviously an issue for us.

I think this was also mentioned yesterday. We need consistencies in the regulatory modeling. For far-field modeling, development of the common databases that have been preevaluated and tested and could alleviate some of those issues.

And I know that with this last bullet there's some concern there that we're saying there may be--now, there's a role for Lagrangian photochemical models using a range of realistic background concentrations to identify potential worst case ozone and secondary $PM_{2.5}$ impacts. You know, some could say to get a realistic background you need to run the photochemical grid model, so why don't you just do the photochemical grid model.

So maybe--what we're proposing is maybe like a tier 1.5, somewhere in the middle, where you could use Lagrangian and, you know, put in some worst case background so that you don't have to do the full-blown photochemical grid model. So maybe there's some flexibility there. Obviously we'll follow that up some more in our written comments, but that's just a suggestion.

Obviously this--George presented this yesterday on Model Clearinghouse. It seemed like reading through the proposal that there's a lot of places in there for going through the Model Clearinghouse for approval. And, you know, George showed the letter yesterday of a month.

Our concern is all the stuff that goes up before that. There's all the work with the state and with the regions and trying to get them to get to the Clearinghouse. So again, if everything has got to go through the Clearinghouse, obviously there's concern with the process, also just with the issues of being responsive because of the staffing resources that EPA has to deal with. So we just feel that there's potentially a lot of things that could go to the Clearinghouse. I'm not sure if that's the--you know, the right answer for everything.

You know, this is--I think Sergio talked about this, and I think Air & Waste also brought this up yesterday, and I just want to recognize Steve Hanna, who--basically this was his brainstorm when we presented our comments at the 10th Modeling Conference of the need for a scientific advisory panel.

I think an external panel could strengthen and expedite some of these model improvements and could be used as part of this tiered structure that was suggested yesterday by Air & Waste. Again, the panel can review the proposed methodologies, can review any specific model revisions, you know, model--the panel can review the beta options before the release to help, you know, with the bug fixes so that there's not numbers of bug fixes after each other. Again, it's just a--and also the panel could provide input on any guidance that's issued prior, you know, to that.

So again, I think, you know, we suggested this, or Steve suggested this, at the 10th Modeling Conference and I think it's still a premonition. I think it sounds like there's other people that are also behind this issue, so I think, you know, this is one of our suggestions.

And just to kind of also wrap up here with the tiered approach that Air & Waste brought up yesterday, I think it's a good idea. We also support that. I think with the one--one added suggestion is that I think for the one year testing and debugging period, knowing how long it took to get ARM2 and some of the low wind projects through the system, I think our suggestion would be that the testing and debugging be concurrent with the EPA assessment.

So when a project comes to EPA and it's ready to go, instead of--while they're also reviewing it, put it out there for testing and debugging so that the modeling community can also look at it at the same time and try to shorten up their review process, because you're still going to have--as I suggested, you're still going to have a 90 day comment period to take in any comments on any of the--any improvements. But at least maybe some of that testing and debugging could be done concurrently so that we can kind of shorten up the time period on that. And that wraps it up.

Mr. Bridgers: Thank you, Chris. So we'll transition from Chris to Cindy Langworthy. And Cindy is going to offer some comments from UARG.

Ms. Langworthy: Thank you, George. I'm Cindy Langworthy of Hunton & Williams, and I am pleased to have the opportunity to speak with you on behalf of the Utility Air Regulatory Group, UARG. UARG is a voluntary group of electric generating companies and national trade associations. The vast majority of electric energy in the United States is generated by individual members of UARG or other members of UARG's trade association members. UARG participates on behalf of its members in proceedings under the federal Clean Air Act that affect the interests of electric generators.

Air quality modeling has an impact on many of the activities of UARG's members. For example, such modeling influences the siting and design of new power plants, and it affects the operation of existing power plants. To ensure that business decisions concerning these facilities are based on accurate information, it is vital that EPA's recommended models and modeling tools realistically estimate, not greatly overestimate, the impact of power plant emissions on air quality. EPA's proposed rule on revision of the modeling guideline explicitly recognizes this. It says that the use of modeling practices that are overly conservative may unnecessarily complicate permitting.

But overly conservative modeling practices do more than just complicate permitting. Overly conservative practices can lead to predictions that air quality problems exist when in fact they do not. And that in turn can force expensive and unnecessary facility redesign or emission reduction measures. It can even lead to cancellation of planned facilities or shutdown of existing ones in order to address problems that do not exist in the real world. And the more stringent EPA makes its air quality standards, the greater the problems caused by overly conservative models. There is simply less room for error.

UARG plans to submit detailed written comments on a broad range of issues raised by the proposed changes to the modeling guideline. My comments today, though, are going to focus on one overarching concern: EPA's continued reserva- tions about accepting model improvements developed with support from industry groups in order to make timely improvements to EPA's preferred models.

And I will say that I've heard some stuff here. I'll try and take it into account as I'm talking. I'm sort of in the same position as Chris--that is, this was written and approved, you know, before we heard the discussions here.

Industry groups have repeatedly shown that they are willing at their own expense, and sometimes considerable expense, to retain recognized air quality modeling experts to undertake rigorous, well-vetted projects to develop tools that address identified inadequacies in current models and modeling techniques. They coordinate with EPA on such projects, seeking and responding to feedback received from the agency.

But when industries then provide EPA with improved modeling tools, EPA does not necessarily, and I would say as a matter of course, act promptly to incorporate the improved tools in the modeling guideline or in guidance. Instead it has been our experience that approval of new techniques developed by anyone outside of the federal government are delayed.

Often the best that happens is that after months or years EPA will label those techniques as non-default beta options, which means they cannot be employed unless the user is willing to undertake burdensome, time-consuming, case specific demonstrations of the worthiness of the new techniques.

Thus, in addition to the cost of retaining recognized modeling experts to carefully develop and test model improvements, the regulated community faces the cost of regulatory delay and uncertainty when seeking a permit to build or to modify a source. And a climate of delay and uncertainty discourages businesses from building new sources or upgrading existing sources. Not only does this hurt the economy, but it can also mean that existing sources with higher emissions are not replaced or updated.

This is what has happened with industry developed techniques to improve the prediction of short-term pollutant concentrations during low wind speed conditions. EPA took a step in the right direction in 2012 when it incorporated three improved techniques, LowWind1, LowWind2, and U*, into AERMOD and AERMET. Since then, however, these options have been treated as non-default beta options, and running AERMOD and AERMET with non-default beta options turns those preferred models into nonpreferred or alternative models.

In other words, in order to use an improved technique to model short term concentrations during low wind speed conditions, users find themselves turning the preferred AERMOD and AERMET into nonpreferred versions of the model. And that burden--they then have the additional burden of having to demonstrate to permitting authorities the appropriateness of such nonpreferred alternative models from both a theoretical and a performance perspective. And I'm concerned that this already cumbersome process is about to get worse because EPA has proposed that written approval will be required from the Model Clearinghouse each time an alternative model is going to be used.

UARG members had hoped that the proposed revision of the modeling guideline would signal a major change in EPA's attitude towards model improvements developed by those outside of EPA. In particular, we hoped, indeed we expected, that EPA would announce that it would no longer treat the techniques for addressing AERMOD model overprediction under stable, low wind speed conditions as non-default beta approaches but would instead consider them acceptable by default. As you have heard repeatedly at this conference, these techniques have been shown to improve AERMOD's performance significantly.

Language from the preamble to the proposed rule encouraged us to think that EPA was in fact proposing such action. The preamble states that EPA is proposing to--proposing updates to the AERMOD modeling system to address a number of technical concerns expressed by stakeholders and goes on to explain that among the updates are proposed options to address AERMOD model overprediction under stable, low wind speed conditions. Great.

But the user's guides for AERMET and AERMOD still refer to them as non-default and/or beta options, which means their use would still require approval of an alternative model. And we were encouraged yesterday by comments from EPA that this may not be the case when the current rulemaking is complete, and we certainly hope that's true.

And it appears that other industry-sponsored model development work has not been incorporated into the default models, although it may be. Although a modified version of the ARM2 screening technique developed with industry support has been incorporated into AERMOD, again, the AERMOD's user guide indicates that this technique is a non-default data option.

And an industry developed improved chemistry algorithm for CALPUFF has not even been considered, as EPA proposes to downgrade CALPUFF to the status of a screening model. We were told that that would happen, that consideration would take place, during this modeling conference.

Furthermore, EPA does not seem--seems to have determined some limitations on how Lagrangian models can be used, which means that the SCICHEM model, a sophisticated Lagrangian model with photochemistry that we heard about yesterday, that industry developed at considerable expense with significant feedback from EPA will not necessarily have a role in the modeling guideline. We certainly hope it does.

But the preamble to the proposed rule indicates that a Lagrangian model may be the type of model to be used on a case by case basis for second level screening assess- ments for Class I significance and cumulative increment analyses. And again, on a case by case basis means it's an alternative model.

In these times of limited budgets, EPA should take advantage of the fact that industries are willing to undertake at their own expense model improvement projects to address identified inadequacies in the current suite of tools. EPA should be able to review and approve the use of such techniques promptly and should not let the improvements linger as beta options for years.

The above described options to address AERMOD model overprediction under stable, low wind speed conditions have been beta options for three years. Three years as a beta option is more than long enough. The modeling guideline should be revised now to give appropriate stature to reviewed and validated tools that recognized modeling experts have developed with financial support from industry. Such improvements should no longer be relegated to alternative model status.

Moreover, it should not be necessary for the regulated community to have to wait more than ten years in order to see improvements in the modeling tools incorporated in the modeling guideline. Nothing in the Clean Air Act requires that a proposal to revise the modeling guideline be tied to one of the triennial conferences, which, by the way, are triennial, not every ten years. Nothing says that changes to the guideline must be made all at one time instead of as each new tool or option becomes available.

In summary, UARG continues to urge EPA to adopt a more agile approach to updating the modeling guideline to ensure that it keeps pace with the needs of all stakeholders and with the efforts of stakeholders to provide the agency with well-conceived, well-vetted improvements to existing modeling techniques. UARG encourages EPA take full advantage of model development work being performed by recognized experts and funded by the regulatory community--the regulated community.

And if I could just add a little aside here, I understand that the modeling guideline is a rule and that revising it requires notice and comment, rulemaking. I'm a lawyer. I believe in this. What is not clear to me, however, is what constitutes a bug fix that does not require rulemaking, and what is a change that does, and I suggest that EPA clarify the distinction.

In any event, the agency should now revise it's proposed--the modeling guideline by classifying the helpful new techniques, including those for low wind speed conditions, as acceptable by default, not as non-default or beta options. And Model Clearinghouse approval for the use of such new techniques should not be required. It may well be that EPA can take this action at the end of the current rulemaking, and we would welcome that.

Even if taking such action would require the current proposed Modeling Guideline revision to be re-proposed, however, that should not stop the agency from taking that correct step. UARG believes that any delay caused by a re-proposal to include the fact that these will become default options would be offset by the time that would be saved by the ability to conduct modeling analyses without the need to go through approval for an alternative model.

Furthermore, UARG recommends that EPA revise its modeling guideline much more often than once every ten years. EPA should put in place a mechanism that ensures the agency will revise its modeling guideline whenever new modeling techniques have been shown to improve model performance. And the agency should consider whether revisions to the *Guideline* are needed each time it revises a NAAQS, and if they are, it should proceed to make those revisions promptly. Thank you.

Mr. Bridgers: Thank you, Cindy, and UARG. So now we're going to go off of the agenda that you have in front of you and we're going to go with two more presentations, and if there's time we also have already requested one oral comment that we'll squeeze in before lunch. So pardon me just a minute to pull this over.

(Pause.)

There we go. So now I'll yield 15 minutes to George Schewe--where's George--oh, there's George, with Trinity Consultants.

Mr. Schewe: Good morning. My name is George Schewe. I'm a meteorologist with Trinity Consultants in Covington, Kentucky. And I'm going to keep this very simple. A lot of you know I teach some classes for Trinity and a few other places and I can speak for about an hour from one slide, so I've got 18 slides, so we're going to cut it a little shorter today.

One way to keep it simple is I'm kind of speaking here on behalf of all the little guys that aren't here, the local agencies, some of the state agencies, Chris Beekman from--he's a meteorologist up in Ohio EPA. He called and said, "Can you kind of be paying attention to what's going on for me? We can't afford to get down here."

The city of Cincinnati has air toxics problems, and one of the things that they still do--they don't have all the CAMx capabilities--are some of them still run AERSCREEN. Some of them are still running SCREEN3. And so we've been trying to wean them away from running SCREEN3 and just running AERSCREEN.

So what I did was I just looked at AERSCREEN and I said is it really conservative, okay? So that is the whole thing I'm going to talk about today. Here's the quote from the existing Appendix W. I won't read to you all of it, but the purpose of screening is to eliminate the need of more detailed modeling so it will not cause--so it's supposed to give you a conservative answer.

I couldn't find that in the new one, but what I found that was close was in Section 4.2.1(b), and it says, "As discussed in paragraph 2.2(a), screening models or techniques are designed to provide a conservative estimate of concentrations."

So if I've got AERSCREEN, and I've got an air toxics program in my state and I'm not going to run CAMx for an air toxics of 1 ton of formaldehyde from somebody in town, I want to know if it's going to be conservative enough to protect the standard so that I don't have to tell that little mom and pop operation that they've got to run AERMOD and it's going to cost them more to run AERMOD than it is to produce whatever they're producing and put down a little bit of formaldehyde. So that's what I tried to do.

So what I did was, unfortunately--I did this couple or three months ago--I used some older versions of the model. So one of the things that I need to do is rerun this with the newer versions of the models to see if it makes any difference, so AERSCREEN 14147, the current version of AERMOD which is not current anymore, 14134. I used the current versions of BPIP and AERSURFACE and AERMAP and basically set it up using some of the default and suggested sources that have been in some of the examples for AERMOD, ISC, and all of the other models over the years, okay?

That one little bullet there at the end, which you in the back can't see because it's way down on the bottom, is that I've actually compared SCREEN3 to AERSCREEN, and if you use all the defaults and do the best job you can do on defining the Bowen ratio and the surface roughness and the albedo that you get fairly similar results between SCREEN3 and AERSCREEN, at least for the current--the previous version.

So what was my methodology, seven different source types, and basically four different locations: Orlando, Florida surface data, LaGuardia, Bowling Green, and Dalhart, Texas. Those are really different types of meteorological stations. These are little windroses for each of those stations on an eastern U.S. map just to kind of show the general patterns of wind that are going on there. And you can see LaGuardia, New York has a very diverse pattern, a big component from the northeast and from the northwest, which is not unusual.

So what are the differences between some of these stations? I took the two that were the most diverse from each other and just put their little graphical Google Earth image on here. LaGuardia in the top is situated near water, but it's also situated in a very highly urbanized, industrialized and commercialized area, as compared to Dalhart, Texas, which is in the middle of a lot of circular watering systems, watering plants for agricultural, so very different in terms of that.

So what did that mean in terms of assigning albedo? So I downloaded the AERSURFACE, the 1992, which is now 23 years old, data and ran the AERSURFACE model on that to get the albedo, the Bowen ratio, and surface roughness, which I then put into AERMET to run the AERMET meteorology data for using in the AERMOD part of the analysis but then also used these inputs for the AERSCREEN part of the analysis because you have to put those in.

The biggest difference there is, well, first, Bowen ratio and the surface roughness. Those are quite different from LaGuardia. So I just want to point that out because when I show you some results in a few minutes, you'll see how that affected the results possibly.

These are the sources I selected. Again, these are derived from some example problems that are available for AERMOD and have been kind of historically used over the years, so four different stacks, from a little short stack up to a GEP stack, and then an area source that was square, an area source that was circular, and then a volume source that was kind of a nominal dimensions, about the same size as the area source because a lot of us use those for roadways and things like that, so just put each of those into the models.

For receptors, the last bullet, no terrain; I didn't want to have terrain affecting the analysis here, so everything is flat. We're in eastern or western Illinois. But then I've got two different receptor grids. For the AERMOD analysis I just had a circular grid, which we don't use much anymore, but I used a circular grid to kind of simulate all the different 10 degree sectors of wind for AERMOD. And then for AERSCREEN I just kind of took every-thing to the east of each of these facilities because that's the way the wind blows most of the time in AERSCREEN, from the west.

Okay. The rest of the methodology, no downwash, so no buildings or anything that's going to affect any of our analysis. I ran AERMOD with each source for each receptor for each meteorological location. And then I did one hour, 24 hour, and annual block averages--24 hour and annual block averages, or I guess 24 hour is block average annuals, though--everything and then all the one hour values. And then I ran AERSCREEN/MAKEMET with each source for each receptor for each of those and then used all the same inputs for the AERSURFACE data that you need to run MAKEMET.

So what was my expectation? My expectation, based on reading the two Appendix W, were that I would always get higher concentrations with AERSCREEN for each situation. And that would be something that I could use as a screening tool.

I had a second expectation on this slide and I took it off because I still use AERSCREEN occasionally just to model some sources to find out where's the hot spots. And as my friend Ryan Gesser--Ryan, are you still here--said to me years ago, "Why should I run AERSCREEN? Why don't I just throw everything in AERMOD and run AERMOD because then we can put in multiple sources." Oh, yeah, you can only do one source at a time. I didn't run all seven of these at once, okay? So, but I took that expectation out because I was limited to 15 minutes.

So here are the answers. These are for one hour. I know you guys in the back can't see these. But AERSCREEN is in the third column, the concentration you got for one hour from each airport. And then AERMOD is in the fourth column. And then I just hit a simple yes or no, was it conservative or not, okay? I'll highlight a few of these for you.

So if we look at Dalhart, it looks pretty good except for the square area source. If you look down here at

Orlando, it looks pretty good for all the sources. It was pretty conservative for all the sources, and it was generally above AERMOD. Then you look at Bowling Green, and for Bowling Green neither one of the area sources passed. All the point sources still passed, so that was good. AERSCREEN was still conservative for the point sources. But then you look finally at LaGuardia, and it's all nos except for the 35 meter stack. So for LaGuardia we did not get AERSCREEN to be conservative, so I was a little concerned about that.

The last three columns over there were my little hot spot analysis. I was trying to again figure out if it really picked where the hot spots were, and again the same kind of mixed results, did a pretty good job, but--so I decided, okay, I'll plot these and just kind of see what they look like.

So the northwest corner for all the meteorologists there, that graphic for Orlando, AERSCREEN was always above the AERMOD models, so it was conservative. But then if you start looking at some of the other sources or locations, Bowling Green, Kentucky, there's our area source there. It was underestimated. And if you look again at Dalhart, it was underestimated. If you look at LaGuardia, just about everything again was underestimated. So it really wasn't screening--a good screening tool for doing sources in the LaGuardia type area.

The same kind of deal in the 24 hour concentrations, and if you look at all the locations there, all of them on a 24 hour basis--and I didn't give you a tabular view of the 24 hour. All of them you did get higher numbers. Even at LaGuardia it did pretty good. You can see down at the bottom corner here there are a few that were still below at some of the source types, and I didn't pick out which kinds those were.

On an annual basis, I think in this case all of the answers were above the screening level--or the screening level was above what you would get in AERMOD. So it did good job on the longer term averages. As somebody earlier this morning pointed out in one of their slides, the model does pretty good for longer averages somewhere in space and time, but when you get down to the shorter, one hour periods, maybe it doesn't do as well.

Okay, so what are our observations? AERSCREEN is not always higher than AERMOD for one hour concentrations, especially--they're always higher for stacks in rural situations, though. That was pretty good, I guess. But they're not always lower for--and they're lower for most source types in an urban situation. And again, I only based that on one urban situation, that being LaGuardia, so--and AERSCREEN is generally higher on a 24 hour and annual basis.

So my conclusions were AERSCREEN provides a good, basic tool for consideration of sources very quickly. It's not always conservative. It does better in rural situations, meaning it's conservative. It does well for rural and area sources. And it doesn't do as well--it underpredicts for area sources--I'm sorry, in most locations. And it does pretty well for longer averaging periods.

So as--Bob and I talked about this just a few minutes before--or this morning, and, you know, one consideration is rerun it with 15181. Another question, though, I perhaps have for our panel of modelers here for OAQPS is can you in MAKEMET, and maybe James--I think you were the AERSCREEN guy yesterday--I can ask questions from the podium, right?

Does MAKEMET have an option to include U* or the low end options? And if we make it default in AERMOD, will it also become default in AERSCREEN? So that was kind of one last question. So I thank you very much. Have a good day.

Mr. Bridgers: Thank you, George. It's nice to have another George up on the podium. So we have one last presentation that has prepared slides, Mr. Jeff Bennett. And Jeff, I am going to turn the podium to you.

Mr. Bennett: Good morning. As George told you, my name is Jeff Bennett. I'm an air quality engineer for Barr Engineering out of our Jefferson City, Missouri office. I'm going to be giving a few comments today on the ozone--single source ozone modeling component to the Appendix W proposed rulemaking.

And what I--I mean George asked for people to say who they're representing, and I would tell--I've been a regulator for a long time. I was a regulator at the state of Missouri for about 18 years, and so I've never really been able to say--I'm representing myself today as a concerned citizen, so there you go. That's who I'm representing--a concerned air quality modeler may be better.

So just a quick, very quick, summary of what EPA is--what appears, what my read of the initial--my initial review of the Appendix W rulemaking and what they're trying to do, they want to develop some MERPs. They're above what the existing SERs are on a national basis, which, you know, I think Chris from API said people would generally support. I think that's true.

They want to utilize photochemical modeling of areas around the project as a means to arrive at a calculated ratio of precursors to downwind ozone impact, so basically existing air quality data, photochemical modeling data to use to develop these calculated ratios of ozone to precursors.

The third and the fourth ones are sort of combined I think a little bit in the rulemaking, but a new photo-chemical modeling analysis, and a lot of yesterday was talking about the Lagrangian piece, to determine specific projects will either have impacts above or below the SIL to avoid a NAAQS modeling analysis, a cumulative NAAQS modeling analysis.

And the last one of course is the one that's sort of exactly the opposite of what George was just talking about. It's not air screening. It's the most complicated thing you can probably do in terms of modeling analysis, which is full photochemical modeling.

All right, so regulatory context. As I said, I'm an old regulator from the state of Missouri. I was working there for about 18 years. And we went through a rulemaking in about 2004, 2005, and it was called Missouri 10 CSR 10-6.345, and it was affectionately called the Upwind NO_x rule. So we've already gone through--"we," this is the old "we."

When I was working for the state of Missouri, we went through a--went through a process as part of our NO_x SIP component of our ozone--our ozone SIP, I'm sorry. As part of our ozone SIP we went through a process and developed an upwind NO_x rule for five counties upwind of the St. Louis area. It was difficult. I don't begrudge EPA trying to go down this road. I think it's going to be a long and winding pathway.

But the idea was we wanted to--we wanted to sort of alleviate some of the control requirements on existing sources within an nonattainment area by trying to limit the amount of impact that was coming in from upwind areas. So it was a tradeoff situation. And so the concept was we were going to develop this de minimis threshold, right?

And so there's a lengthy rule. It's been--subsequently the rule has been revoked because it was never used, quote-unquote, used, and the St. Louis area got redesignated so there was--there was not a need for it at the time. They've been redesignated back into nonattainment now, but I guess the air standards are getting more stringent. You can move back and forth; right?

So the idea is that you can go check out the rule and see what it says, but the question you always--that comes immediately to everybody's mind is what was the number, what did you come up with, what's this upwind NO_x rule, what's the MERP or the--what I call the local MERP.

It was 900 tons of NO_x per ozone season. May through September was the ozone season. It still is. And that is equivalent to about 2100 tons a day, a lot better than the 40 tons a day for the SER, and so we had--we had developed that process. We went through the evaluation to generate that number.

What I'm not doing today is suggesting that that's the right number. That's not what we're talking about. It's not necessarily 2150 tons. It's not necessarily 900 tons for ozone season, but it does--the process we went through raises a lot of sort of more detailed technical issues that the rulemaking and the subsequent guidance don't really get into, and so that's what I'm going to focus on a little bit today.

So area specific considerations, and you can read that stuff. I mean the photochemical models are great because you include all the sources in there. It has back-ground in it. You don't have to worry about adding in other stuff or combining models. It's a single model to predict concentrations to the extent it has the capability of doing that, which you have to go through an evaluation exercise to get a SIP approved, and so there was that benefit.

But in St. Louis we had gone through that exercise and so we had the capability of going down the pathway of using this existing data set to go through the analysis that EPA's talking--that EPA is considering doing as part of the Appendix W proposed rulemaking.

The fact is that that's a difficulty, though. When you attempt to use a photochemical grid model, which takes a long time to run--everybody gets that--for single source analysis, it's impacted by all the sources that are around it, the background, the ratio of VOC to NO_x.

I mean there's a whole pile of existing informa-tion that has to be accounted for every time you run the model and every modeling step. And so it changes every time that the model kicks over to the next meteorological period. And so the idea is that it causes--and not only is it a benefit, it also causes some potential problems.

And so what did we find out? From a VOC/NO_x ratio perspective in St. Louis, the reason why it's not called the upwind VOC rule was because the St. Louis area is extra-ordinarily NO_x limited. It's got a whole--Chet probably remembers this from back in the day, but it's the biogenic volcano in the Ozarks, which is south of St. Louis. And so there's a whole pile of additional biogenic VOC that's hanging around, and so the VOC controls doesn't make any difference.

And so there wasn't an upwind VOC rule because you could have changed a 10,000 ton a year VOC source or 100,000 ton a year VOC source and it wouldn't have made any difference. And so that's why it's not called the upwind VOC rule. It's called the upwind NO_x rule.

And so the idea is that that's going to be the case in a lot of areas. You're going to have this same problem in every--in every--in every county, every state, every area, every rural or urban area, and you're going to have these discussions. And so this is a specific issue that EPA is going to have to address and deal with, I think.

So the next issue--so it's the next level. So you've got levels of this stuff. So the first level on the decision making process is which model am I going to use? What episodes am I going to evaluate. Am I going to use source apportionment or am I just going to use a brute force method?

I mean those are all sort of first level questions. What's the form of the model concentration that we're going to compare to the SIL? There's all these sort of specific things that are sort of rudimentary in terms of moving past the initial hurdles that EPA is talking about here.

And so the next levels--there's several levels of conversation. It's about stack heights and VOC to NO_x ratio and specific ozone speciation, and that's what this slide talks about. So the reaction chemistry for all ozone models, all photochemical models, is set up on profiles.

And so there's a grouping there. You don't model every VOC species. There are groupings of species, right? And so each one of those have got a different reactivity. If it didn't have a different reactivity, why would you have a speciation profile? You're going to have to--you have to have it--you have to have it show up as different species in order for the model to predict appropriately. And so that's the idea. You need to speciate stuff.

And so what does that mean for downwind ozone impacts? Eladio yesterday in his presentation went through and was looking at highly reactive species versus somewhat unreactive species, I think. And so his results would be consistent with what I would have expected, which is you're going to have higher--more reactive VOC species show up with additional impacts and less reactive species, and so VOC tonnages, one ton to the next, are not the same, and so you're going to have to address that issue as well.

But generally, my overall comment--and we're trying to get out of here on time for lunch, George, so there you go. I'm doing what I can for you and everybody else. The idea is that you guys have done a bunch of good work. I think that you've evaluated this process and went through a constructive and difficult arena to get down to the path of trying to propose a MERP. Ultimately that's really what we're talking about, I think, for a lot of sources that are going to be out there.

The problem is that the level of detail that's in the rule right now and the guidance isn't sufficient to talk about a lot of these issues that are--that I didn't even bring up today. There's dozens of issues like this you're going to have to get through.

And so to provide substantive comments, and I think a lot of folks have said this already, you're going to have to come up with additional detail. The concept is solid, we believe, or I believe in general, but you're going to have to--you're going to have to come up with additional things to provide the ability to provide comments from a more--on a more detailed basis. That's what it amounts to. And that's all I've got, with five minutes to go. I think I set the record.

Mr. Bridgers: So since we have got into the slight bit of clapping, I think now that we have finished all of the presentations, at least with prepared slides, I think that--for the EPA presenters yesterday and then all the public presenters from the stakeholder community, I think we should all give a round of applause.

(Applause.)

Mr. Bridgers: And EPA graciously appreciates the effort from all the stakeholder community to provide those comments to us in slide form, and we look forward to your written comments by the 27th.

Considering it's 11:30, and we still have a little bit of time before we break for lunch, we have had one request for oral comments. I think it would be appropriate to put that in before the lunch break, and depending on how time goes, if there are some others that feel that they want to say something, I think we can run up to say 11:45 or 11:50. We'll just see how it goes, and then we'll break for lunch.

And immediately following lunch, I will open the forum back up for anyone else that may have a desire to speak. I'm not going to put forth any expectations. My anticipation is it's probably going to be smaller, but all the comments will be transcribed, so any that have to leave for the airport at lunch, you can catch that as soon as we post it to the web site in about a month.

So Bridget, are you--so next we have Bridget Lee. Let me find a good slide to put up, Bridget, maybe this one. Yeah, forget the times but the slide looks pretty good. So Bridget Lee is with the Sierra Club, and 15 minutes.

Ms. Lee: I wish I had had this slide on Wednesday morning when I was trying to find my parking spot. Good morning, everyone. I'm Bridget Lee. I'm an attorney with the Sierra Club. And I expect that we will be submitting detailed written comments in October, but since I'm here, I'll just share a few very brief thoughts.

First, the Sierra Club is very concerned with the proposal to incorporate the LowWind3 and U* beta options as regulatory defaults. In 2013 we submitted to EPA an evaluation of the performance of the beta options, which demonstrated that they decreased model performance and increased the variability of impacts from tall stack sources as compared to the accepted regulatory defaults.

The possibility that the adoption of these options as defaults could lead to underprediction of air quality impacts causes great concern within the environmental and public health community. We question the reliance on the Idaho Falls and Oak Ridge studies, which were excluded by EPA during the initial evaluation of the model. Moreover, these four year old studies are not likely to be representative of air quality impacts from tall stack sources, and we don't see any compelling reason that the agency should rely on the studies now.

Second, with respect to the tier demonstration approach for addressing single source ozone and $PM_{2.5}$ impacts, we're still in the process of assessing this approach but again have concerns about whether it would fulfill EPA's obligations under Section 165. Thank you for your time and attention.

Mr. Bridgers: Well, thank you, Bridget. So we now have a few more minutes before we break for lunch. And we--typically we have a podium microphone set up in the foyer, or excuse me, the aisleway there, but it's going to be easier for transcription if you come to the podium. So if there are any people in the audience that would like to offer any oral comments at this time, you have 15 minutes of time if you want it.

(Pause.)

Mr. Bridgers: We needed to pay for some *Jeopardy* music or something. We'd have to pay copyright, though. Merv Griffin gets his. Well, seeing no immediate interest in providing additional oral comments to the record, I will reiterate for those that will be leaving for the airport over the lunch break that the comment period now extends for--I think it's 74 days to October 27th.

I will during the lunch break update our web site. I wanted to take the registration link off because that becomes sort of invalid, but also to make sure all the presentations are clearly posted. I know that there is one or two that we had little slight corrections that are not changing the record, but there's just some plots that need to move around. But they'll be available.

And as said, it will be three or four weeks at the minimum before the transcripts are ready, but as soon as the transcripts are provided to us and we review them, we will put those on the web site and put those in the docket, so there should be at least--I'm figuring at least 45 days left in the comment period for those that need to review the transcripts and prepare their final comments.

And so with that, I will suspend the conference and public hearing until 1 o'clock, and we will reconvene exactly at 1 o'clock for those that come back, if you would like to provide oral comments. Otherwise I wish those that are traveling safe travels, and see you later.

FURTHER PROCEEDINGS 1:00 p.m.

Mr. Bridgers: Okay. I am going to in just a moment call the public hearing and conference--oh, everybody got quiet, so I'm going to go ahead and do it. I'm going to call the conference and public hearing back to order. I hope everybody enjoyed a pleasant lunch.

So really quickly, just because it's a formality, I just want to reiterate things that everybody in this room probably already knows, but since this is the oral session, I just want to make sure that everybody knows that this is a public hearing.

Everything that was said is going to be transcribed and put in the docket. We're going to require that everybody identify themselves and their affiliation--George Bridgers, USEPA, here, Air Quality Modeling Group in RTP.

We're going to have a session for oral comments. At such time that we do not have anybody else requesting a speaking spot, then I will go ahead and close the public hearing and conference. But that being said, written comments for the next 74-ish days can be submitted to the docket by October 27th. And on the information web site for the conference and public hearing, there's a docket link. If not, you can search for it--or it's even in this presentation here.

The other thing--this is just a side note. Over the lunch period I updated the 11th Modeling page, the web site, so all the presentations are now more clearly posted. They're not embedded in links--I mean they're still embedded in links in the agenda, but there's a page now that has all of them listed, and so you can just go to the 11th Modeling Conference page and see that.

If there are any nonsignificant--let me make sure I say this right--changes that need to be made by any of the speakers--I know there was a couple of plots that didn't print right with the .pdf--we can accommodate those, but if there's any other substantive changes in the presentations, they were given in a public record and I can't change them now, including ours.

Let's see; what else? Other than that, I think that we'll start--we do have one requested oral comments here. And I will go back to the presentation I closed just to have a nice pretty background on the screen. So Peter, I'll give the floor to you. And just like everybody else, you have up to 15 minutes if you need it.

Mr. Guo: Hello, everyone. My name is Peter Guo. I work with Apex TITAN. We are a consulting firm in Texas. I'll just take a few minutes, you know. We are still working on the detailed comments about the proposed regulation, so right now I'll just make quick brief comments.

We do have some concern for the additional requirement of the $PM_{2.5}$ secondary modeling requirement and replacement of SCREEN3 with AERSCREEN. I think that everyone--I think many people already talked about the secondary--$PM_{2.5}$ secondary formation modeling, so I won't repeat our comments. I will just discuss the use of SCREEN3 here real quick.

As I mentioned, we do the permit modeling for the oil and gas industry in Texas, and we have been using SCREEN3 for more than a decade, so SCREEN3 have demonstrated a very useful tool for--you know, for the permit modeling in Texas. So as proposed, SCREEN3 will be replaced with AERSCREEN at this time, so we just suggest that SCREEN3 should be still kept on the list of the preferred models, you know, give the industry the option to use simple and, you know, quick tools to do the permit modeling, you know.

We know, you know, AERSCREEN will give us a more accurate result, but actually, you know, AERSCREEN will require additional, you know, information such as, you know, detailed meteorological data. You know, you have to collect, Bowen ratio, you know, rough surface, or other, you know, terrain, even the terrain information or building downwash information, so--and in the meantime AERSCREEN will just give a similar, you know, result compared with the SCREEN3 result, you know, for the oil and gas, you know, permitting project.

And so overall, so I would suggest, you know, still, you know, the SCREEN3 should be kept on the list of preferred models, and I think that's all I have today. Thank you very much.

Mr. Bridgers: Thank you, Peter. And so at this point having no one that has requested time through Nan up front, I will, like I did before lunch, open the floor up. And probably what I'll do is leave the floor open for two or three minutes. I might not stand up here in front of you because we don't have the Merv Griffin music. But the microphone is open, and if anybody feels that they would like to offer some oral comments to the record, the floor would be yours.

(Pause.)

Mr. Schewe: Good afternoon. My name is George Schewe. I'm not going to speak for 15 minutes. I just have a few things to say. I'm with Trinity Consultants. That's our advertisement part.

The data requirements rule, the SO_2 consent decree--we're in the middle of doing a lot of stuff for a lot of clients, and the Appendix W changes coming right in the middle of this is not helping us, as of course no time would Appendix W changing help any of the analyses we have to do for permitting or for air toxics or anything else.

So just having said that, I thought I'd leave you with that thought as you're going down the road of trying to get some of these studies completed. Thank you.

Mr. Bridgers: Thank you, George. We still have plenty of time till 5 o'clock today. I think George just wanted to get the last word in.

(Pause.)

Mr. Bridgers: It's as awkward for me as it is for you guys. At least in the other meetings I manage, I just wait for someone to make a motion that we close the meeting, all in favor.

As the hearing officer, at least I'm not seeing any motion toward the microphone, so I will make one last call. Raise your hand if you're thinking; if not--going once, going twice. Okay.

With that case, I will call a close to the 11th Conference on Air Quality Modeling and for the public hearing related to the proposed rulemaking for the *Guideline on Air Quality Models*. We appreciate all of your participation, and as I've said many times, safe travels and all that.

But I will also say that feedback, good, bad and otherwise--I've said that to a few people--we welcome through the comments formally on the proposed rulemaking, but also with respect to the conference. We're not going to do a formal suggestion box or a survey, but if there are things that for future modeling conferences that you would think that we should improve upon or change--coffee; yeah, can't do it, unless we do it somewhere where you offer free coffee.

But nonetheless, I appreciate feedback, good, bad and otherwise. So if there are things that you would like to see changed in future modeling conferences other than question and answer sessions--can't do it unless we're not doing a rulemaking--but please send them along. You should be able to find my e-mail address without any problem.

The other thing, and Tyler was good to remind me, I know we've said offline to a bunch of people we are planning or are in the preliminary planning stages for next year's Regional, State, and Local Modelers Workshop.

I will not say that we are completely set upon a destination because we have to get a bunch of things approved, but it would be a city that in 2005 did have an encounter with Katrina, if all goes to plan. And we also did have a Regional, State, and Local Modelers Workshop there that same year prior to Katrina.

But nonetheless, we're looking right now at planning a weeklong meeting, so the first half would be dispersion and the second half would be photochemical modeling with an overlap day. And not that we've completely set on it, but the thoughts would be as possibly the middle day where we have an overlap between both curricula or both disciplines is that we would also have invited stakeholders come in. But it will happen in September. It will happen after that we finalize the rule package, and so I think that would be a forum that we could have a lot more interaction than over the last couple of days.

So again, we appreciate everybody's participation and just have a great rest of your day if you're staying here enjoying the weather, or if you're traveling, I hope your travels are safe.

STATE OF NORTH CAROLINA

COUNTY OF WAKE

C E R T I F I C A T E

I, Kay K. McGovern, do hereby certify that the foregoing pages 251 through 361 represent a true and accurate transcript of the proceedings held at the United States Environmental Protection Agency in Research Triangle Park, North Carolina, on Thursday, August 13, 2015.

I do further certify that I am not counsel for or employed by any party to this action, nor am I interested in the results of this action.

In witness whereof, I have hereunto set my hand this 10th day of September, 2015.

/s/ Kay K. McGovern

Kay K. McGovern, CVR-CM
Court Reporter