# U.S. ENVIRONMENTAL PROTECTION AGENCY

# 10TH CONFERENCE OF AIR QUALITY MODELS

## DAY TWO

**U.S. EPA**

**109 T.W. ALEXANDER DRIVE**

**RESEARCH TRIANGLE PARK, NC**

**MARCH 14, 2012**

**8:30 A.M.**

GUEST SPEAKERS:

Bret Anderson, US Forest Service

Kirk Baker, US EPA - OAQPS

George Bridgers, US EPA - OAQPS

Roger Brode, US EPA - OAQPS

Dan Dix, All4, Inc.

Tyler Fox, US EPA - OAQPS

Ryan Gesser, Georgia-Pacific, LLC

Steven Hanna, Hanna Consultants

Ralph Morris, ENVIRON

Robert (Bob) Paine, AECOM

Mark Podrez, RTP Environmental Associates

Erik Snyder, US EPA - Region 6

James Thurman, US EPA - OAQPS

**U.S. ENVIRONMENTAL PROTECTION AGENCY**

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**MARCH 14, 2011**

**MR. BRIDGERS:** Well, good morning everybody. I think we'll get started here in about 30 seconds, so if we could take our seats, it would be appreciated.

While everybody is taking their seats, I wanted to just hit a few logistics that we went over yesterday. I know there may be a few new faces in the room. For those that are new this morning, welcome. For everybody who was with us yesterday, I hope you had a pleasant evening and a restful overnight period.

I wanted to remind everybody that this is a public hearing. All the presentation material that's on the screen and everything that's spoken is being recorded. Be mindful of that.

And to that end, during the Q and A sessions, if you would like to ask a question, we ask that you use the microphones and identify yourself before you ask the question.

Also, I guess it goes without saying as you look at the agenda that we've got 20ish, 25 presentations; however you want to count it across the course of the day. We've got a full agenda. So, we're

just asking for everybody, especially the presenters, to try to be mindful of the schedule and respectful of the other presentations, but we did pretty good yesterday staying on time. We actually got out a few minutes early.

So, I am not going to say anything else because Tyler Fox is my boss and I'm not speaking for him.

**MR. FOX:** Well, welcome back. It looks like everybody decided to come back and then some. As George said, we have a packed agenda for today, but I thought it would be useful for everybody if we went through the process and the scope of what we're talking about in terms of updating Appendix W to provide context for today in particular because today we'll be going through the current draft of the PM2.5 guidance which we still have to release and George will go through that. Our apologies ahead of time for not getting that out, but we needed to finish up some things and get some internal review before we put that out and that will be forthcoming, I believe, either by the end of the month or early in April to facilitate comments as part of this docket and this meeting on that.

We'll also be talking about the

challenges with respect to the 1-hour and NO2 and SO2, NAAQS, and all of those issues are things that may be ripe for consideration in terms of updating Appendix W and, at the end of the day, we'll be talking about emerging models and techniques.

At the 9th Conference, we introduced the source apportionment techniques and other uses of photochemical models to account for single source impacts. Now, we have a commitment based on the grant of the Sierra Club petition by Gina McCarthy in our agreement. It's not as if she's forcing it upon us. That we address chemistry in Appendix W and update Appendix W accordingly to account for those ozone and secondary PM2.5 impacts and that also overlays with the ongoing evaluations that we've been undertaking with the Federal Land Managers on long-range transport models.

So, I should have started with a review yesterday in terms of our current regulatory models and status and updates. There are aspects, as you all heard, that would be ripe. So, I want to make sure that we all understand the scope. There may be issues outside of this that people want to comment on, but in terms of updating Appendix W, everything is fair game for the most part and we really need your input in

terms of that process.

So, in terms of the current regulatory models, as I said, there may be improvements or updates augmentation to the model formulations. There were a number of issues yesterday, but there may be others to consider that you all have. We'll hear maybe perhaps tomorrow in the public session. There's also the suitability of the current performance evaluations for regulatory purposes that we would welcome input and comment on and perhaps modifying.

As we discussed yesterday, the need for field studies or to take existing field studies and add them to the suite of studies that are used to evaluate our models as we go through the regulatory process. And, as we ended yesterday, we talked about the new beta release of the Mesoscale Model Interface Program to facilitate the use of prognostic data for CALPUFF, AERMOD, and SCICHEM. As Brett indicated, we would need to, you know, provide guidance in terms of the use of that model if put into the regulatory arena, but it may be an issue, and likely an issue that we would need to address in terms of updating Appendix W and codifying rulemaking.

In terms of the 1-hour NO2 and SO2 NAAQS, we've been dealing with that for a year and a

half or so. They'll be presentations about aspects that are relevant to consideration for updating Appendix W.

I believe Bob Paine's going to talk about an approach to account for emissions variability given the form of the standard. The averaging time has provided challenges to us. We've issued guidance on March 1st on the treatment of intermittent sources. More clarification or specificity there may be something to consider as we update Appendix W.

And then the current three-tiered screening approach which served us very well as we moved into the 1-hour NO2 standard. The first approach, a conservative approach, of full conversion is what it is, but perhaps updates to the ambient ratio method which we'll hear about later today. Perhaps pursuing refined model status for either OLM or PVMRM as techniques within AERMOD and, you know, that gets to both improvements or augmentation of those model formulations as well as doing sufficient evaluation of those techniques to provide the confidence necessary in order to establish it as refined techniques.

The benefit of that would be that it wouldn't be a case-by-case justification. There would be things that -- that you could just take and use. Of

course, those techniques, as you all know, in terms of dealing with the standard have a number of additional input requirements that would need to be addressed sufficiently and we have current defaults and always defer to case-by-case local or source specific or area specific type of information in terms of in-stack ratios and the background ozone, but those are things that may also need to be considered as we move forward. And I believe that we'll hear about an evaluation of PVMRM this afternoon as well.

In terms of emerging models and techniques, we've been working with FLMs putting forth both the statement of program needs which I'll cover this afternoon, as well as a new evaluation paradigm for the long-range transport models and the chemistry models which we feel is appropriate and more in line with how they're used in the regulatory arena. The fit for purpose type of paradigm in terms of we need to demonstrate the models can do the things that they're required to do under the regulations and make sure that we and you have confidence in their ability to do that.

We also have been undertaking a number of model inter-comparisons and evaluations to inform that process of what models are viable and what techniques are viable for assessing single source

impacts on ozone and secondary PM2.5. I'd also say for visibility and deposition purposes, in terms of AQRVs, so we look forward to working with the various model developers in the future as part of this phase three effort as we both provide you the information, as will be discussed in detail this afternoon on our evaluations, and what models and techniques we have been looking at and putting forth to the community for consideration.

As we go through the process as was done in the original IWAQM phase one and phase two efforts, starting with that broader landscape and then narrowing it down to those models that are viable and, if necessary, determine what areas or aspects of those models will need further develop, research and development improvement, such that they can meet the needs that we have both at the EPA and for the Federal Land Managers.

So, in terms of process, as I introduced yesterday at the first modeling conference that I was part of, the 8th Modeling Conference, that was a month before we actually promulgated AERMOD and that was the last time we updated Appendix W. The focus was to incorporate AERMOD into Appendix W. There are a number of other changes, perhaps housekeeping type of items,

that were taken care of, but that is the last time Appendix W was formally updated through notice and comment rulemaking.

So, you're all on notice that the next update officially starts now with this 10th Modeling Conference. As I've mentioned, we've committed to doing that in a grant of the petition to Sierra Club and in that, we explicitly stated that we would expect that at the next conference in three years, the 11th Modeling Conference, that we'd be discussing proposed rulemaking and changes that either the EPA is considering in an upcoming proposal or have already proposed and getting feedback in that context.

So, I imagine that most people in the room and elsewhere have the question, okay, so what happens between now and then? That's a good question. Hopefully, I have a good answer.

So, between now and then, there is no engineering ABC formula for this. Obviously, at the tail end of a promulgation process to get a proposal out, we're going to have to go through our bureaucratic and administrative procedures of review both within our agency -- we'll determine whether or not this rulemaking is significant or not to go through review by OMB. Previously, they have not gone through that

review process, so there's some internal things that we're going to have to work out that will take up time at the tail end of the process in getting a proposal out of the door. Those are things that you don't have to worry about that we have to worry about quite a bit and also they take up time in terms of getting something out of the door.

But, for the most part, what we're looking forward to and what we've been stressing throughout and will continue to stress throughout today and tomorrow is that we need to take into account your public comments. So, we need your information both, you know, here and now, as well as in the coming month or so. I believe that we are going to formally extend the comment period to the end of April -- the 30th of April, to allow for comments from you all. That'll allow, not only time to digest the information that we've provided here and the presentations and the background information up to now, hopefully it will allow you time to get the draft PM2.5 guidance and provide comment on it, as well as the two outstanding reports on the AQRV assessment and evaluation, as well as the plume chemistry evaluation and those reports will be out, I believe, in early April.

So, hopefully, that provides sufficient

time to look at those things. That said, that doesn't preclude any consideration after that fact, but we really do need your input so that we can then go through a process of identifying those critical or priority items from our standpoint, summarize those public comments and priorities later in 2012, and get that out back to the community and engage with you through the appropriate venues, either through individual trade associations or other types of venues to vet that to make sure that we've both heard you and are, you know, in line intersection in terms of some of the priorities that we have and that you have in the community.

And then we'll continue our efforts to inform the process and provide information. We will issue public reports, EPA reports, and the like, either solely by the agency or in collaboration with the FLMs. We'll be submitting journal articles to promote peer review of the evaluations, the models that we may be tweaking, if you will, or updating. And then we'll participate actively in workshops and conferences that are upcoming.

At the tail end here, I mentioned suggesting a specialty conference perhaps late in 2012 or early 2013. I believe AWMA or other organizations

have regularly or periodically had such conferences and we've actively participated in those and we would welcome that type of venue to have much more detailed discussion and focused discussion on specific items so that we can make progress.

And then, as George mentioned, this year we're having a week-long -- our annual workshop with the regional, state, local modelers, is going to be a week-long workshop that involves both the permitting side as well as the SIP side, so we won't be able to facilitate a public session as we did last year, but I would imagine or I would anticipate that in 2013, in the late April, May, early June time frame, we would have our annual workshop and we would, again, extend an extra day or two to have discussion about the items that we're focusing on and working on.

Again, we're trying to provide as many avenues as we can to allow for the type of interactions and more full disclosure and transparency in those interactions. And so, the other aspect to help us coordinate that, would be the third bullet there, is George mentioned that we had established a technical coordination workgroup that was really born from the fact that here we would get various meetings with different trade organizations and the like on a

periodic basis and we would meet with certain subsets of the community. In most cases, we're hearing the same thing across the community, but we learn of different efforts that are going on across different parts of the community and one aspect of that was the cross-fertilization across the community of those efforts to make sure that people are aware of what is going on and connecting the dots there and so it makes it easier on us to work with you all, but also to make sure that if there are collaborative efforts that we can take advantage of, that we're aware of those. We can inform that committee of the things that we're doing by meeting on a periodic basis and have those representatives be able to disseminate information and feed that information throughout the community.

So, we've established a technical coordination workgroup. They helped and were a tremendous help in getting the agenda solidified, the invited speakers, and the like. So, we greatly appreciate that effort and we really feel that that is a good way to continue coordination as we go through this process. So, we can talk more about that and get your thoughts and ideas. Again, it's one of those things where everybody probably wants to be part of the workgroup. What we stressed in initially forming it is

that we needed people who were going to be active participants and had been, kind of, deemed representatives for a broader group and the like, so they did have a responsibility and an obligation to take that information back to their host organization and so we can make sure that everybody in the community is represented in that fashion, but it's a very effective and efficient way to coordinate, in addition to the other aspects that I mentioned here.

So, obviously there's a lot of time and effort that will be taken to try and coordinate and communicate, but we also need to take the time to get the job done in terms of the work that we're doing.

Hopefully today, as you see the information that we're providing, you'll gain a better understanding and appreciation of the time and effort that folks here have put forth to put these things in place in addition to dealing with case-by-case situations on permits and the like and dealing with other aspects of the EPA workload.

I should mention that this is one aspect of what my group deals with and so we're also dealing with federal rules and other types of things doing the assessments and the work, writing guidance for other purposes, and applying these models in other -- for

other purposes. So, these folks are doing a wonderful job, but they do have quite a bit on their plate. So, what we'd ask is that we try and find an appropriate process that respects both the community and our time and resources so that we can move forward in the best way -- most effective and efficient way possible.

So, before I turn it over to George, I guess, what I'd like to do is open it up and make sure that, in terms of the process and the scope, everybody's understanding. If there's any questions, I would welcome them now because I want to make sure as we go into today and go through all the details and more substantive things, that we have an understanding of that process and scope and you all know what we're expecting of you all in that process and if there's any questions or clarification, please let me know now.

No questions?

**AUDIENCE MEMBER:** Bob Paine, AECOM. Just a question on the length of time to comment on the PM2.5 guidances becoming smaller and smaller, we were hoping for I would think 60 days, but it looks like it would be less than 30 days, perhaps.

**MR. FOX:** Well, so, in terms of the guidance, I mean, what we really need and want is comments through this formal process, but that doesn't

preclude comments after this formal process. So, I wouldn't think of it as that the door is slamming and then we're going to move on. I would think of it -- that, it's to everybody's advantage to get comments in sooner rather than later so that we can take those into account and then, as we modify and update that guidance throughout the year and then finalize it near the end of the year, we can take those considerations and comments into account.

So, you know, same thing with those other two reports in terms of the evaluations. They're kind of seeing it as an evolving process and the like. Again, it's to all of our advantage to get comments sooner. If they're put together in this, you know, context, we can deal with them in aggregate and then work through them. If they come in afterwards, that's fine, in terms of the PM2.5 guidance and those reports we'll deal with those, but what we're trying to emphasize is that people use the current process. But by no means do I mean that that shuts the door and then we're not going to consider anybody's thoughts or comments. In fact, you know, we would welcome additional analyses or other type of work that people may do as you hear more on the guidance and as you hear about potential techniques this afternoon, they may

spark some ideas that you could do that could then inform that guidance.

And that's very similar to the way in which we have updated the SIP modeling guidance. We put something out there for review and we've gotten, you know, a huge amount of input from the states and others and assessment by the states that were very valuable in coming up with that final guidance.

**AUDIENCE MEMBER:** Is there a schedule for that guidance to come out final?

**MR. FOX:** We, at this point, we'll be talking about that in Chicago in late April with the regional, state, and local modelers. Obviously, we have a new old ozone standard and may have a new PM standard at some point in time. We're trying to align and update to that guidance -- that SIP modeling guidance, with those types of requirements and it's not clear, given the existing level of the ozone standard, whether or not the current guidance is sufficient or not. We're addressing those types of issues and we'll let people know. I wouldn't anticipate any update to that guidance before the end of the year.

**AUDIENCE MEMBER:** That was -- no update to the SO2 guidance before the end of the year?

**MR. FOX:** No -- okay, I thought you were

talking about ozone, PM, regional haze, SIP modeling guidance.

**AUDIENCE MEMBER:** Yeah, I wanted to bring up the issue of the SO2 guidance that came out last fall, I think it was. Is there a schedule for that to become final?

**MR. FOX:** The agency got comments on that overall guidance and right now, we and senior management, and others are determining how to move forward, so I can't give you any answer on that. Any other comments?

Welcome, Raj. Raj Rao, everybody. There were a number of people looking for you yesterday, Raj, so I figured that I would.

**MR. BRIDGERS:** As we transition here, I wanted to thank everybody. The creatures of habit in the room that everybody sat almost exactly where you sat yesterday, so I feel very comfortable as I look out and I see the various people in the room.

I'm going to go ahead and say please accept my pardons. The presentation you're about to see, there was a lot of midnight oil -- later than midnight oil, later than that midnight oil that was burnt last night. So I had a request in the audience to dim the lights. If I fall asleep while giving the

presentation, just nudge me. So, here we go.

So, George Bridgers, U.S. EPA, so we got that on the Record. The topic of my talk today, if I'm not doing spell check which makes no difference to the room, is The Lochness Monster, Sasquatch, my Insta-Blade Brackets, things that everybody's heard about, but nobody has quite seen.

So, as was already asked in the question if this will move forward, we had intended to have PM two and half -- the draft PM2.5 permit modeling guidance out last fall in advance of the 10th Modeling Conference that was originally scheduled for October. We intended to have this out in a timely fashion for the review and comment by the state and local agencies and then also by the permit modeling community.

One of many aspects as to why we delayed the conferences, obviously, this was not the only one, but it did provide us some more time to work on it. As, like everything else, best laid plans, good intentions, whatever you want to say, we didn't get it out before October and it's not on the website today.

Part of the reason for that is, Tyler's already gone through, that we have had a multitude of things that have been ongoing. There's a lot of complexities here with the guidance document that we're

talking about and, to that end, there's been a lot of coordination that has been needed between senior management, the policy division, and OGC. We had actually had hopes -- we were burning some of the oil this last weekend trying to get this document in a final form that we could put out there, but there just wasn't time for the review internally that happened before we could then put it out. As Tyler said, our plans now are to have it out in the very near future, but I did want to make the comment, and this comes back to what Bob asked, is the comments for the PM2.5 guidance are in no way tied to this conference. And that we welcome the comments after the docket and we've already mentioned this morning it's going to be extended to April 30th.

The thought is that when we release this draft guidance, the states and locals are going to make comments. The modeling community as a whole is going to make comments. There may, in fact, need to be a second version of the draft document that's put out sometime later in the year for more comments. Tyler had mentioned the fact that we may be soliciting other ideas or work from all of you. This is a collaborative approach and it's something that, working together, I think that we can put together a quality document when

it's finalized.

The regional, state, and local modelers workshop will be our next opportunity to sit down with the states and have an informed discussion. Keeping to the timeline that Tyler mentioned that we would have it out sometime in April, there probably won't be a tremendous amount of review time by the states and locals by the time they come to the conference, but we will have that as a part of our workshop and we'll also summarize at least what we've heard today, tomorrow, and over the course of the docket with the state, local, and tribal agencies in Chicago. And then, as I said, we're going to put the goal of finalizing this only a year late at the end of 2012.

So, for the rest of this presentation, I have to put this disclaimer out there, one that I'm very tired, but two, the slides throughout the remainder of this presentation -- they're glimpses as to what we're doing. They're by no means the final document and that they have not gone through all of the formal internal review processes. So please, we shouldn't take these as sort of the EPA's formal recommendations or particular endorsements. So, caveats as they are.

I thought it would be appropriate, most

of the people in the room this is going to be old hat and so, pardon the next five minutes or maybe a little bit less, but I wanted to kind of set the stage for everyone, build a little bit of background so we can understand where we came from to where we've arrived to today.

Actually, this kind of dates my history with air quality as well because it was about 1997 when I joined the state agency but, nonetheless, the daily and annual PM2.5 NAAQS was set back in 1997. At that time, there was no monitoring network and there hadn't been any significant modeling done. There really wasn't the techniques or tools in place. So, when this standard was set, just within a few months after it was set -- promulgated, a PM10 surrogate policy was put into effect. And this, for the most part, allowed the status quo to continue with respect to the permit applicants using their PM10 requirements to satisfy that for the PM2.5.

Not a lot happened through the early part of the 2000s. We got past the 2000, you know, the bug with the computers and that wasn't an issue, and 2011 or 9/11 and what not. We got to 2006 and we decided to revise the standard. We only revised the daily standard, moving it from 65 to 35, but we

retained the annual standard of 15.

Things were going along pretty good. 2008, though, was the first real nugget that helps us with this guidance document and, subsequently, with the work that we all have to do in showing compliance. It's that we got the PM2.5 NSR implementation rules promulgated and through this, the first vehicle for helping us was put into place and that was the SERs -- the significant emission rates. In here, for direct PM, the ten tons was set as the SER there and then there was also a precursor of PM2.5's SERs set for NAAQS and SO2. Both of those were set at 42 tons per year which happened to be equivalent to their SERs for them as major pollutants by themselves. There was also this little quirk put in there about this grandfathering provision that allowed the federal permits to continue relying upon this PM10 surrogate policy. There's a bunch of dates that are involved in that. I didn't want to bring it up in this context, but I encourage anybody, if they want to know that history, to go back and look at this May 16th rule.

On or about February 11th, though, of 2010, EPA published a rule to repeal this grandfathering provision and also put an early end to the PM10 surrogate policy. Well, with that action on

February 10th of 2011, we came into the world where we knew that we were going to have to deal with PM2.5 from a compliance demonstration standpoint of view and to aid that, there were a couple of actions. I talked to you yesterday with the Model Clearinghouse update that there was a Region 6 clearinghouse action in -- it was in 2010 -- I think it was February 2010, with regards to a couple of things with background monitor and then the calculation or what model value to apply to background compliance demonstration for PM2.5.

On the heels of that clearinghouse action and based in some degree to the work that had already been laid down for us by OTAQ, there was a lot of work that already had been done in that arena for PM2.5. There was a memo that was released March 23rd, 2010 and it was entitled The Modeling Procedures for Demonstrating Compliance with PM2.5 NAAQS. That's a pretty good name.

Somehow, this has affectionately become the Page memo. Now, Steve Page has signed dozens and dozens of memos but from the context of this community, we've just always come back and referred to this as the Page memo. I imagine the next one that fits in this community, we'll call it the Page memo two or whatever we want -- a do-over or whatever. But at any rate,

there was a couple of important things here. This memo really was the foundation -- is the foundation for where we are today with respect to being able to show compliance with PM2.5 and it's also the foundation of the draft guidance that you've yet to see.

There's a couple of very important things that were addressed here and Roger Brode has showed up and he could probably talk at great length about the aspects that we're now dealing with the probabilistic standard. Previously, it was a deterministic, second high type approach and now we've moved into the world of probabilistic standards and that throws a monkey wrench in the way that the -- the general convention had been for compliance demonstrations.

Another thing is, you know, there was all the complexities associated with the ability for existing models to be able to show the secondarily formed PM2.5. These tools and techniques, there were some out there, but they were not promulgated into Appendix W and they are definitely not at this point.

And finally, there was the recognition that you need to pay special attention to your background monitoring concentrations because now that takes on a more important role than just characterizing

the primary emissions from your surrounding facilities and the background longer range transport, but you're also catching a bit of the secondary forms locally and remotely. So, you just need to understand the context to which you're doing your compliance demonstration.

A few more nuggets that came into place and this was a rather important one, that in October of 2010, there was the PM2.5 increment, the significant impact level, and the significant monitoring concentrations were promulgated and so we moved from the world of some interim SILs and SMCs to a world where we had a defined set of tools that we could use in the compliance demonstrations. I put the table in here. It's worth pointing out the annual SIL for PM2.5 and class two areas .3 and 1.2 with respect to the 24-hour standard. So, that was for reference.

And then that kind of brings us to the modern era. This last year on May 16th, the surrogate policy ended and so, at this point, without question, all the PM2.5 -- or all the PSD compliance demonstrations with respect to PM2.5 had to consider primary and, if applicable, the secondarily formed PM2.5. I said here from precursors but, nonetheless, it's something that needed to be done. It was no longer just an optional thing where you had a policy to

which you could rely upon.

And then one more nugget that actually plays into moving forward is that on the 21st of July, Gina McCarthy signed a memo that effectively ended this interpolluting trading ratios that had been set as presumptive ratios in the preamble to the 2008 rule that we talked about on May 16th.

I think it's worth pointing out that although we removed the presumptions for applicability across the country, it didn't remove the position of the agency that we felt that these ratios could be set on a specific non-attainment area or a regional basis, but that would require local agencies, state agencies, and the like to work very closely with the regional office, develop a technical justification for those ratios that are set.

The memo actually went further and had four different steps as to how one might go through that process and even offered up, I guess, in nine cities around the country that there was at least some data already available to which agencies in those areas may be able to glean information upon. But there is a wealth of regional modeling and other things that are already in place that agencies may be able to rely upon.

While all this was going on, I guess Tyler Fox made the request, but there was a request from EPA made to NACAA. It was basically a plea for help with the states to let's work together in a collaborative fashion to tackle some of the issue with regards to modeling PM2.5, especially from a single source perspective.

So, early in 2010, a NACAA workgroup was formed. There are others in this room that probably can speak a lot more knowledgeably of this workgroup, but I'll give it my bit of a try here.

The objective was to come up with a set of technical recommendations back to the agency with respect to the guidance. It comprised obviously with NACAA you're talking about state modelers, permit engineers, staff that worked with the mission's inventories, and we had also some regional office representation and some representation from here from OAQPS. They worked together. I think initially there was probably 20 or more different items that were put on the table and that coalesced into three main focus areas that are listed here below.

Each of the workgroups that were formed had a specific charge or they formed a specific charge and then they worked across the year 2010 to come up

with a set of recommendations.

Let's see -- one, two, three, four days after I started with the agency here, a report was delivered to my desk which I appreciated greatly. It was nice reading along with all of the training that I had to go through. That was the culmination -- the compilation of all the recommendations from the three subcommittees. That report is available through NACAA but for the interest of this conference, we have posted it on the SCRAM website specific to the 10th Modeling Conference.

I'm not going to read all the names but since this is going into the Record, I thought it was appropriate to have some recognition for all those that worked diligently and provided information back to us. So, I'm going to show each of the workgroups. Briefly, you can read your names and, again, many of the people that helped with these recommendations are sitting in this room. And then I'm going to show kind of the bullet points of what the recommendations were from the workgroup.

So, with the Emissions Inventory Sub-Workgroup, there was a request or a recommendation that there needs to be a new emphasis on the development of reliable PM2.5 emission factors. Until such factors

could be developed, quality assured, and are available, the workgroup recommended to kind of leverage against existing state programmatic work. An example that was given was with CARB.

There was also a request or a recommendation that we provide guidance as to what types of emissions sources are required to include with respect to secondary formation in their modeling analysis. The example that was given in their bullet points was for combustion sources.

The secondary formation from the project source, this workgroup was made up of a large number of people that I've worked with through the number of years with SIP related modeling and a lot of the recommendations that came out of this workgroup are mimicked in some of the slides that I'll give later that are directly found in our draft guidance. So, I'll give everybody just a second to read all those names.

And with respect to the recommendations, at the top level, they had recommended a four-tiered approach to modeling PM2.5 conducting the air quality analysis. They wanted us to reconsider the use of the maximum modeled value for comparison to the NAAQS and that's something that was established in that Model

Clearinghouse memo and reinforced in the March 23rd Page memo.

They requested or recommended the development of offset ratios which reflected the geographic and seasonal variations with respect to the single source permitting, that fits in somewhat the context of what was discussed with the McCarthy memo, and then to complete an evaluation of plume models and, if necessary, clarify the guidance for tier three modeling approach. I have a slide of their tiers here in a second.

They also said that we should consider adding comprehensive chemistry to AERMOD and if it were to be done, then we would have to look at the tier four and their level and also require an update to Appendix W.

I look to Roger Brode, he's typing on his computer. How's that chemistry coming? It's coming. I say that in jest, I apologize for putting Roger on the spot there.

And then with respect to the tier four, the photochemical grid models, there were a whole bunch of issues that were brought up and I'll go ahead and say that these issues have been brought forth from the recommendations they sent to us and that we're also

continuing to solicit comment in our draft guidance document when you see it. Such as: what's the best way to apply the models? There's a varying number of sources whether there's DDM or source apportionment or the like. Should we use plume and grid or some type of sub-grid characterization? Absolute or relative modeling? You know, we got into the world of relative modeling with regards to your state implementation plans, but we still did a very straightforward, absolute type modeling with dispersion modeling. Now, we're bridging those communities. What do we do?

And then with respect to photochemical model, now that you've got a photochemical model, especially if you're using a sub-grid treatment, do we use it for both the primary and for the secondary form because we'll have that information. Or does it still make sense to use AERMOD for your fence line effects and things that are within the near-field and then somehow use the secondarily formed impacts from the photochemical model?

And here's this table. I hope it's a little bit clear. I've tried to make it big so it could be seen from the back of the room, but they followed a typical approach that we had with other NAAQS pollutants where you had a SIL and a cumulative

analysis and then there was four different tiers. Three of the tiers were contained within the cumulative analysis, but in each one of their steps, there was some consideration for the secondary formation from precursor emissions.

Then finally, here's the third workgroup or sub-workgroup and this was with respect to the representation of background concentrations. So, just a second to recognize everyone. I see some names and they're in this room. Mike Kiss. John Glass is here. Leigh Bacon.

With respect to the recommendations that came out of this workgroup, there was a couple of different paired sum approaches that were recommended with respect to whether it was continuous data or whether it was every one and three day samples. That is a problem that's unique to PM2.5 is that we don't sample every day at all locations. Their recommendation here was to develop an analysis technique to help with whether one or more monitoring sites could be used whether we're talking sort of a creeked field approach. What about -- and that kind of plays into this next one where we talk about the infusion of model predictions with observations. And also there was a request to modify AERMOD to read an hourly background PM2.5

concentrations through a file. And then finally, modify Appendix W to accommodate the recommendations.

So, now that we've got the background, how we got to where we are. We've got the Page memo. We've got some other pieces in place. We've got the NACAA recommendations, the charges. Okay EPA, go out and let's get this thing done.

I should mention up front, and this was stated in great detail in the Page memo, that there's a screening nature to everything that we've talked about because we don't have an explicit model that is promulgated in Appendix W to do a one for all, for example. So, I can read the bullets word for word, but, you know, given the potential contribution, the secondary formation, it's not explicitly accounted for, the prominent role of background, certain aspects of the standard. There are things that are with this particular criteria pollutant that aren't with others.

PSD modeling should be viewed as the screening level analysis and that's analogous with what had been done for NO2 and that's in Section 5, 2, 4 of Appendix W. Do a little more RegText quoting with respect to Section 5, 2, 2, 1 (c). The choice of methods to be used to assess the impacts of individual source depends upon the nature of the source and its

emissions and, therefore, with respect to everything that we've talked about, there's this need, a significant need for consultation with your state and local authorities and with the regional office.

I know that the words case-by-case become the most dreaded terms ever, but we're in an arena now that we don't have a wealth of information and so we are going to glean things from every case-by-case situation. What's paramount to this is the modeling protocol. That's the avenue to which the consultation can start and there should be a well-developed modeling protocol that's then been approved by the state and local agency and the regional office before a lot of work gets done because we've seen in several cases that I've actually gone through with PM2.5. I won't say that there were some simpler ways. Simpler makes it sound lesser than it was, but there was probably some more efficient ways that the application could have been done. People kind of get worked up in a frenzy and lose sight of the ultimate goal.

I threw the flowchart in here. This is very typical for NSR PSD. I'm not going to spend much time here. The important point of this is that once you fall into the PSD side of the house and you're above the SERs, we're still recommending in our guidance

document that you're going to go through, at a screening level, the significant impact analysis and also a cumulative analysis. The significant impact analysis being one which you're not including background. You're only looking at the emissions and the secondary formation from your project source. And then the cumulative impact analysis is where you're going to look at sources that cause a significant concentration gradient on or about your source and also you're going to take into consideration aspects of your background concentrations.

Within that framework, we're proposing four different scenarios and these four different cases, if you will, will help further define how you would step through the SIL or the cumulative analysis. And it may be that in one of the cases or a couple of the cases, you may not step through the SIL or the cumulative analysis.

So, really quickly, the first case is -- I won't say it's a no-brainer. We put it in here just for completeness. And this is one where your facility doesn't trigger the SERs. You've got a maybe major facility for CO or what have you, but your net emissions increase or proposed emissions increase from PM2.5 is less than ten tons and with respect to your

precursor pollutants are less than 40 tons. You're done. You don't have to do anything with PM2.5. Celebrate. We can all go have a drink.

Case two, and this one is going to be fairly typical, is where you have a facility that is major. It's a major facility -- a PSD facility. And you've triggered for your primary or direct PM2.5 emissions. They're above ten tons per year. And your precursor emissions from NOx and SO2 are less than 40. And here, this should be the easiest case because we've already got the Page memo that's in place. The groundwork has been done. You're going to do a very typical compliance demonstration using AERMOD. And with respect to here, we're saying that you do not have to make any assessment of secondary formation.

The third case, and this one is the one, this is the big one. This is going to be the meat of the draft guidance when it's out. And this is one where you've triggered for both PM2.5 and one or both of your precursor emissions. And here, you've got to make compliance demonstration that takes into consideration the direct impacts of primary PM2.5 and also the secondary formation. That being said, this sum assessment that has to be done does not necessarily mean that you have to go all the way to a photochemical

model, meaning that everyone's got to run CAMx or CMAQ or the like or some new model that's yet to be formed.

There may be a completely qualitative way that you can go about demonstrating your compliance demonstration, at least from the secondary formation. You're still going to have to go through AERMOD for your primary. But in most cases, there's probably some hybrid qualitative quantitative approach that can be done. And this is where we don't have a wealth of cases and the case-by-case is going to help us. And we really only anticipate in a handful of situations that one would go all the way to the step that a single source itself would be running a photochemical model run. It's not out of the realm of possibilities, but we think it's going to be a rare exception, not the rule.

And then the fourth case, and this one may end up being the most controversial, maybe not to the community, but within the legal world, it may. And this is one where you are not triggering for your primary PM2.5, but you are triggering for one of your precursor emissions SERs, so NOx or SO2 have gone above 40 tons with respect to the net emissions increase. Here, we're saying, well, sort of like case one, maybe no analysis is needed. Maybe you need to make a few

statements, but the pollutants NO2 and SO2, they're already -- their SERs for their major is already 40 tons, so you've already triggered compliance with the 1-hour NO2 and or SO2 standard. They're very stringent when, you know -- well, they're adequately stringent. Maybe I should say it that way.

Nonetheless, what we are trying to get confirmation from within the agency is saying that compliance -- that facilities compliance with the 1-hour NO2 or SO2 standards is adequately protective of that of the secondary formation for PM2.5. We hope we can get this through. This would be something that would be good for the community. All that being said, let's talk about how the devils are in the details. Let's actually go through a couple of the bullets about how we actually go through the modeling process.

I've already said case one we're not doing anything. Case four, we hope we're not doing anything. Maybe there's some discussion there, but at the end of the day we're hoping there is no modeling involved. So, both cases two and three require at least the compliance demonstration for the primary PM2.5. I already said it's your standard fair AERMOD type of exercise. It's the preferred near-field model. You're going to have all your normal considerations. You're

going to have your modeling domain considerations. You're going to have your source impact considerations. You're going to have to think about your meteorological inputs. I could go on and on. We've seen talks yesterday that went into length about the different things that we need to consider.

And then when you get to the cumulative impact, that's where we have to start thinking about the inclusion of background and that could be directly from the monitor and you also have to think about how you're going to model explicitly from the other sources. I could spend a week here and still, thanks to James for providing this, but I wanted to put it in the Record. It's just a review, obviously, of the AERMOD system.

Some things to think about and since our friends from OTAQ are in the room, we do have some considerations to think about with the receptor grid placement. It's unique. You have to think about it. It's unique to your modeling domain depending upon things such as terrain, sources modeled, various other aspects. They should be placed in areas that are considered an ambient air. There's nothing new there. There are some current provisions, though, that say that PSD source impact analysis with respect to the

NAAQS is required only in areas that have an existing PM2.5 monitoring locations as well as locations that are appropriate for comparing predicted PM2.5 considerations to the NAAQS based on the PM2.5 monitoring sitting requirements and recommendations. That's a mouthful. I'm not going to -- there's a lot of nuances here. There's some caveats at the bottom that this is -- that PSD modeling is not required to include receptors that are located at sites that are not "population oriented" and also it's not required to consider effects, at least from the annual standard perspective, of receptors that are considered micro and middle scale.

There's a lot of nuances here and I would say that we can engaged in another venue to talk about some of those nuances, but there's some issues here just to be mindful of specific to PM2.5.

Emissions and source characterization, at least from respect of the primary direct PM2.5 modeling, you're going to follow Appendix W with respect to Section 8-1. You have to look at Table 8-2 with respect to the maximum allowable emissions. With your source characterization, you know, you have to make sure you have all of the source release parameters. I understand that the characterization

actually goes back to the NACAA recommendations. There are a bunch of issues here because this is an area where we don't have as much information, but it's growing with time. In, obviously, things like building locations, urban rule, what have you. Five years of meteorology. I'll just go past that one. For one year on site data. I didn't actually put in this slide -- let me back up in all fairness. The one thing that's not on here is that we talked about the MMIF tool yesterday and so that's something that we need to further think about incorporating possibly down the road.

Just a few comments on the -- once you've moved to the cumulative impact in case two still where we're just looking at the primary direct PM only. Just a few things to think about with respect to monitored background. You have to consider a few things, you know, should -- the monitoring concentrations should account for the contributions of the secondary formation associated with your existing facility and also those throughout your domain. Consideration should be given to make sure that you're not double counting in your background monitor for sources that you're explicitly modeling.

It's likely not going to be quite as

important for secondary contributions -- secondary formation, just because of the lack of spatial and temporal location of that formation and also the uniformity of the background concentrations from the secondary perspective, but you could have some issues if you have a site that's located fairly close to a larger source.

You also may have to take into account seasonal variation and this is one, I think I have the caveat here that you may have a facility with fugitive or low level emissions that, at least from the primary perspective, your worst situations are probably going to be in the winter time when you have a lot of stable conditions or longer stable conditions than you would in the summer. Whereas, the maximum levels of the secondary contributions, especially say in the southeast that are sulfate driven, are going to be in the spring and summer. So, in that type of case, you may have to take into consideration the seasonal variation. And then I put a caveat in here also that the relative composition of PM2.5 also might need to be something that needs to be considered, especially in the western part of the country.

Now, let's talk about comparison to the NAAQS. We're still in case two. Somewhat, we can

think about case three on, at least on the direct side. We haven't got to the secondary formation aspect yet. At least with respect to the primary PM2.5, it's going to be pretty a straightforward approach. Remember that I said we have to take into consideration the probabilistic form of the standard. To that end, when you combine the monitored and modeled concentration, just because the standard is a 98th percentile and this is what was addressed in that February 2010 Model Clearinghouse action, you don't take the 98th percentile model and add it to the 98th percentile background because that would result in something that is less conservative than that of the NAAQS.

So, for the annual, the actual background or the design value concentration that you're going to use is going to be the design value. It's a three-year average of the annual average PM2.5 concentrations. And then for the daily, it's the three-year average of the 98th percentile of the 24-hour average concentrations. It just so happens that it's the eighth highest if you actually had an everyday sampler that had 365 samples, but please reference Appendix N to 40 CFR Part 50. If you had like every three day monitor or a monitor that had a lot of missing data to understand the rank for this specific design value

calculation.

With respect to combining the modeled and monitored, though, with respect to the annual PM2.5 NAAQS at the SIL level. We're just at the SIL level. We're not combining it with the background. You're going to take the modeled annual average of the highest concentrations from each year that's modeled and average that. Or, if you're only modeling one year based on site specific, it would be the highest annual average from that year and you can compare it against the 15 data.

Did I say that right, Roger? Did I read it right, Roger? This is Roger's.

**MR. BRODE:** Well, it's the annual average -- average across the number of years at each receptor to the highest --

**MR. BRIDGERS:** Right, at every receptor and the highest -- so, you would average the highest at each receptor across the five years and you would pick the highest of those, right?

**MR. BRODE:** Right.

**MR. BRIDGERS:** And then when we look at the cumulative analysis, this is where we bring in the background concentrations. So, once again, you're going to use the same step where you're going to look

at the highest annual average at each receptor averaged over five years and pick the maximum of that and then add it to the design value. So, I'll make the corrections there. Again, midnight oil, with respect to NAAQS and SIL.

And then when we talk about the daily, here we're going to do the same type thing only we're going to look at it from the model 24-hour average. So, again you're going to have your receptor grid. You're going to find the highest daily PM2.5 concentration at a receptor averaged over five years. Pick the maximum out of the entire receptor grid. Then that's what you're going to then compare against the SIL and then, not the NAAQS, sorry. And then with the cumulative, then we add in the background concentrations. The background concentrations here, though, we have a first and second tier. This is also outlined in the Page memo. So, there's already information out there that you could run and look at later this afternoon.

So, let me talk about the first tier here. For applications where the impact of the primary emissions aren't really temporally correlated, fairly steady state throughout the year, the first tier modeling analysis would be exactly as advertised. It's

that highest at a receptor for the year, averaged over five years, the highest receptor point then compared to the SIL. But when you have cases that are a little more challenging, where you have seasonal variability, then we have to look at something that's quite different where you might take into consideration a seasonal or quarterly basis. And that would be considered a second tier.

It's probably going to be more of an issue with the daily standard than it's going to be with the annual standard, but that doesn't exclude that there may be one or two cases where there is some impact on the annual standard.

And then I put down some bullets here with respect to that second tier where you would go through and determine four seasonal background values that then would be combined with the modeled concentrations. Then that would then be compared respectively to the SIL. So, I've got a few edits here to make in the slide.

Important thing is the AERMOD now has the capabilities to track all this. So, Roger has done all the dirty work. And there may be some future updates and some post-processing that would need to be done to help a little more inform the PM2.5 analysis.

Is that a fair assessment? Okay. Let's switch gears. We talked a lot about case two where we were talking primarily about the direct PM2.5 analysis which was already somewhat straightforward, but now let's talk about the assessment of the secondarily formed PM2.5 and this puts us in case three where we were both triggered the SER for the primary PM2.5 and also for the precursors.

Here, some level assessment has to be done. And we've already talked about that. That it could be a completely qualitative, it could be some hybrid quantitative qualitative, or it could be full blown modeling. I can't stress enough the consultation is paramount here. And one of the things I didn't mention about the modeling protocol earlier in the presentation is one of the most paramount things in that modeling protocol is the conceptual description. You really need to understand the nature of PM2.5 and the nature of the environment around your source to be able to effectively develop your strategy for modeling.

So, what's a qualitative only approach? There are situations and this is one example. This isn't the only example. One example where your precursor emissions are only marginally higher than the SER, so maybe you've got, I don't know, 100 tons, just

an example. Maybe that's a bad - maybe 85. Let me just throw a number -- 83. That's when the Wolfpack won the last National Championship.

**AUDIENCE MEMBER:** 40. Precursor in terms of 40.

**MR. BRIDGERS:** They are, but I mean, they could be 41. They could be 162, but nonetheless, it's not 10,000. Your background levels are also very low. So that when you look at your -- back in case two, when you look at your direct PM2.5 concentrations, their modeled impact compared in the cumulative sense with background concentrations, you're still halfway to the standard. And so now, you know, the statement is, okay, in reality is 83 tons of NOx going to create in my environment 20 micrograms of PM? And I challenge anybody to go find in any documentation where that's happened. I imagine it has but, nonetheless, that's what we're talking about. You kind of look at things objectively. You see if it even fits a real case and then you make a technical justification for not going further into a full blown photochemical modeling exercise.

We have recently dealt with this in Region 10. Now granted, the OCS off in the Arctic Ocean and Beaufort and the Chukchi Sea is unique to the

whole country. But, nonetheless, they put together what effectively was a weight of evidence type of discussion that their background was, I forget, it was like 15 and with their model it was, you know, whatever it ended up being, like in the low 20s, and so they were so far removed from the NAAQS. It was ammonia limited, so there wasn't the right chemistry to make a lot of ammonium nitrate and it just wasn't reasonable. And that was the end of the story.

That's not going to be the case for most of the country and this is where we get back into this hybrid qualitative quantitative approach and this is probably going to be most situations where we've got higher background concentrations. I'm thinking the industrial midwest. I'm thinking the southeast. Your primary impacts are fairly high. You're pretty close to NAAQS to start off already and so this is where the consultation process really kicks in. It's trying to understand how far are you removed from the standard and what type of analysis would be most appropriate moving forward and what tools do you already have in the war chest? Has the state just recently done a SIP demonstration or was there regional modeling that you could leverage upon to show? Maybe there was some sensitivity modeling that was done that could be

brought into the fray.

One of the recommendations that came out of the NACAA workgroup and that we -- although the McCarthy memo said that we removed the presumptive offset ratios, that didn't in any way exclude states from going through and making that type of justification and it may very well be that that's a good policy for states to go through, at least on non-attainment basis areas or on regional areas that have similar types of emission density and traffic patterns and the like to create offset ratios that then could be applied by the various applicants.

So, the modeling demonstration would be your direct PM2.5 with AERMOD. You apply the offset ratios. If everything is below the SIL or, if you have a violation and that combination shows that you don't cause or contribute to that violation, you're done.

And then there's going to be a talk later this afternoon that Ralph Morris is going to give. There's something that was -- that was actually a Greg Yarwood presentation that was given at CMAS last year. Now, the presentation specific to ozone, but we really feel that something like that screening tool approach could be done for PM2.5. It does require some agency, some body, to do some level of PM2.5

photochemical modeling, but once the tool is in place, then it can be applied across the region.

And then we get to the chemical transport modeling. The middle bullet is probably the most important one. We hope that this is the rare case, especially in light of the compliance requirements with the 1-hour and NO2 and SO2 standards. The NACAA recommendations have this basically as their tier three and tier four cumulative impact assessments. We're, I guess, tier three with them you have Lagrangian models and tier four had the Eulerian models. We think that there's a lot of promise. SCICHEM, for example, may be another solution and that's something that will be discussed this afternoon in the emerging models and techniques session. And also there will be some discussions on the Eulerian models. CAMx, CMAQ, they've been widely used for years for SIP attainment purposes, but now we're starting to look at them from single source specific. Now, they're resource intensive. We fully understand that. But I did want to provide and I think Kirk Baker -- I don't know if Kirk's in the room, but he provided some of the information here as we were putting together this document. He's been helpful here.

So, just a few things with respect to

single source. There's various different applications of brute forth method which is the old tried and true zero out. You've got source apportionment techniques that have taken on a more prominent role in recent years, especially with some of the rulemakings that have come past.

You've got DDM and there's, somewhere on the horizon, higher water DDM. And then, we talked about a little bit earlier about the sub-grid treatment and if we go down the path of sub-grid treatment, do we get into the realm where we're also thinking about the photochemical model applying to both the primary and the secondarily formed PM2.5.

As NACAA did in the sub-workgroup two readout, we also continue to be soliciting from the community various issues. With respect to photochemical models, do we model five years? Is there some episodic type of meteorology that we would use in this criteria? What would be appropriate with respect to secondary formation?

Emissions input. Now, earlier I said follow Table 8-2 -- maximum allowables. It makes a lot of sense when you're talking about direct, primary, AERMOD run concentrations, but now we're trying to get a realistic look at the world with the photochemical

model. Does it make sense to put all of your facilities in there at maximum allowable?

Horizontal grid resolution. You know, everything until recent for most of the states was running 12 kilometers. A few states were doing four kilometers. Is that appropriate? Is that sufficient? And then that brings up the idea about the sub-grid treatment.

I've mentioned earlier absolute versus relative. How do we tackle that? We've got the two worlds of the SIP attainment planning world and the NSR PSD world colliding. Earlier, there was a mention about SO2. That's an area where the two worlds are just exploding together. Additional work is needed to fully understand all of the implications with these various different approaches.

We also -- it was mentioned that there's an ozone PM2.5 and regional haze modeling guidance. In addition to this guidance that's being developed here and they are being developed independent, well, to a certain degree, Brian does work in our group and we talk but, nonetheless, they're independent documents right now. We've got issues with defining just in the model the size and chemical speciation of PM2.5.

And then the whole world of performance

evaluation. With the SIP world, we spend quite a bit of time on MET performance evaluation and looking at the emissions and recirculating through the photochemical modeling and performance evaluation there and, you know, what's going to be expected and representative for this community? And we're still -- probably more questions than answers.

As I mentioned, we have this existing guidance document for photochemicals. It may and probably is not totally appropriate in this context. We want to make sure that we don't develop something in this guidance document that goes in difference to that document and we also have aspects of this document that we don't want to do -- that's going to be difference to the qualitative hot spot analysis that OTAQ has formed that we've used many pieces of in this document.

There's also these other applications -- NEPA, DOJ, and what not that are ongoing. We want to make sure that everything is consistent there as well. So, I put a couple of links up here. That's obviously not -- I didn't actually have the link for the qualitative hot spot analysis, but that is linked through the 10th Modeling Conference website because, like I said, at least with respect to calculating your impact with respect to the NAAQS -- the design value,

that's more thoroughly explained, I think, in Section 9 of that guidance.

And so that ends my talk and I think I'm pretty close to the time that we had set. I should also mention that we, on the schedule, had set aside some time for a panel discussion. Presentations by a state and local representative, a regional office representative, and an industrial representative. All three of the people had agreed to do so with the understanding that we were going to release the PM2.5 guidance document so they could make a presentation on it. To that end, two of the speakers respectfully said, well, no guidance document, no presentation. But, they have also graciously offered, at least in the case of Jim Boyle, that he can answer specific questions to the NACAA workgroup recommendations. He was spearheading that. And Randy Robinson was going to be our regional and local -- our regional office representative and he also can answer some questions specific to things that they've dealt with in Region 5.

But before we do so, and respect to Ryan Gesser, he had prepared a presentation and it was sort of indifferent whether I got his document or not. So, my appreciation to you Ryan for agreeing to talk. We'll definitely have time for questions and answers

after Ryan. So, it's all you.

**MR. GESSER:** As you're about to see and hear, I can barely get through my opening monologue in ten minutes, so I'm happy to take up a little more time that's been yielded from the other panelists and, hopefully, wrap this up still in about 20 minutes to where we can get back to the schedule and the time for those questions and answers.

My name is Ryan Gesser and I'm speaking on behalf of the American Forest and Paper Association which is the AF&PA, the National Trade Association of the Forest Products Industry and the manufacturing operations for those products. And I'm happy to speak on behalf of that group. As a group we're very grateful to be acknowledged and recognized as an important stakeholder in this process in recognition of all that we have at stake and on the line through this process as well as the size of our footprint, not just in terms of the importance of our industry to the American economy, but also in terms of our footprint in being located and operating nationwide and virtually all 50 states and ten EPA regions. This means we're getting a lot of experience very quickly and identifying a number of concerns which is what I'm wanting to share in this forum.

As George mentioned, our comments or our participation, I don't remember if we were invited or volunteered, but whichever the case, we did hope to be sharing comments based on what we've seen of the draft guidance and obviously I just saw what everybody just saw for the first time. So, the flavor of my presentation is really based on our experience and what we hope will be addressed and obviously working through a lot of details in that guidance. And so my presentation is very much a user's perspective and so I think it'll be something of a change of pace from a lot of the discussions that we had yesterday which were very technically and detail oriented. This certainly comes from a higher level and reflects real world applications where we have to deal with the constraints that are in the real world which is making the best out of meteorological data, background data, all those things that are available and have issues to work through and constraints to overcome.

So, naturally, this talk and the things I'll discuss are a mix of both technical issues, but also policy issues and I know that that can quickly get across the lines of sometimes the narrow scope of what this group and Appendix W is meant to capture. But I think it's important to address those policy issues to

the extent that they reflect challenges in the technical formulation of the models and the way we apply them. So, I am going to address some policy issues and I hope that that's okay and everyone appreciates that perspective.

As I shared the content of my presentation today with peers and colleagues and counterparts from the member companies, more than a couple of people said, you're just really going there to complain, aren't you? I said no, no, no. I'm not complaining. I'm articulating our specific concerns. I think that's a very subtle distinction, but one that I hope you appreciate, especially in light of the discussions that we've heard about limitations on budget and needing to set priorities. So, if nothing else, consider this talk my vote for where the priorities go and, you know, where I hope those priorities are set and we can move forward on a relatively quick time frame. So, I'm offering those comments in that context and with that spirit. I hope you accept them that way and we look forward to continuing the dialogue there.

To frame up our industry perspective, we're focusing on integrated pulp and paper mills here which are generally a major source in the regulatory

sense of the word and that we're on the PSD list of 28 major sources. We are title five major sources in virtually all cases. But as far as an industrial operation goes, we're actually pretty well controlled. We're already subject to a number of regulatory programs that end up having standards to control PM not limited to just the new source performance standards that would apply to a number of our industrial boilers and eventually we'll be subject to Boiler MACT.

But also our chemical recovery operations which are sort of the heart of a pulping operation. They're already subject to an existing MACT standard which happens to regulate and set standards for PM as the surrogate for a number of HAPs and beyond that, at the front end and back end of our operations we have wood yards, haul roads, finishing converting operations, all of which can be sources of fugitive dust that are generally subject, at a minimum, to state level requirements for fugitive dust management of some type or another. So, I think we are a relatively well controlled source of PM, albeit being a major source category.

And then more generally, beyond PM, you know, we've been a heavily regulated sector. We're still a heavily regulated sector. And we're going to

continue to be a heavily regulated sector. In addition to the NSPS and MACT standards that apply, not just to our combustion sources, but our process sources and, again, we're going to be subject to Boiler MACT. We are a targeted source category under the various regional haze rules, whether it's BART and/or reasonable progress. A number of facilities will be having reductions come about because of that.

And also, we're in the process of going through the residual risk and technology review process, so we see a lot of reasons or a lot of things that are going to be coming down the pipeline in the next six years or so that are going to be reducing emissions from our operations even further.

So, especially with that in mind of all the changes that are going to be happening in the next six years, you know, we feel like we have a lot of stake, a big stake, in this process.

Like many industrial sectors, our industry finds it difficult to demonstrate compliance with the applicable NAAQS following the current EPA guidance. And I'm going to qualify, any time I say demonstrated compliance, I'm going to qualify it with that same statement. If I wanted to be hyperbolic, I would say it's impossible. You just can't demonstrate

compliance under this guidance, but we are on the Record, so I want to be precise and say, let it suffice to say that it's challenging to demonstrate compliance following the guidance. This, obviously, has a number of consequences.

New projects can't move forward until these modeling issues are resolved. George alluded to the significant emission rates that trigger this PSD permitting process and the significant emission levels that we test and, you know, the numbers, you know, you should that they're very small, so it's virtually any project ends up in the cumulative analysis process and even -- we find that there are sources that you have to resolve the modeling issues before you can even worry about what the project is.

But that's not the only case. There's plenty of situations. A number of states have requirements that require modeling even if you're not worried about PSD. This can come about from minor source permit modifications or operating permit renewals. So, just under the status quo we have a number of facilities in our industry that end up having to do modeling reveals issues that have to be addressed. And the result is ending up with better than BACT or better than MACT levels of control that

might be necessary to resolve a modeling issue and, in some cases, that would require a significant capital investment in new or upgraded controls, but at a minimum, it's probably going to require at least some on paper reductions to permit limits because remember, in this context, we're always modeling maximum allowable emission rates from all the sources and that often corresponds to a reduction in operating flexibility, whether it's just in terms of the operating scenario, alternative fuels, those sorts of considerations. So, there's a lot of this going on.

And I had to come up with a way to explain all this with the magnitude of the challenge to our non-modeling staff that have environmental concerns, but don't do modeling or just in operational and management staff that don't, obviously, do modeling. And I found it most convenient to boil it down this way, just what I call the order magnitude axiom where, you know, under the old PM10 24-hour standard of 150 generally was a controlling standard for types of sources like ours that are -- have a lot of PM emissions. And under that standard of 150, it wouldn't be unusual to find a background level of about 50, meaning we had 100 micrograms to work with and fit our operations into the model. Now, of course, back in

the PM10 surrogacy days, we were only considering filterable PM in that analysis and that's what the permits most often reflected.

So, we know that the PM2.5 24-hour standard is substantially more stringent at the level of 35 and we find typical background concentrations more like 25. So, now we only have ten micrograms of margin to fit our operations into the model. The additional complication, of course, is the PM10 surrogacy policy is gone and we are now including condensable PM in our analyses.

And so we find ourselves modeling emission rates that are about equal. We rely heavily on emission factor data. Filterable PM2.5 might be, on average, range varies from different sources, maybe about 70 percent of the filterable PM10, but then we add back in the condensable PM which varies wider. Sometimes, it's as little as 25 percent, but could even be 100 percent or more of the filterable PM rate. So, at the end of the day, boiling it all down, when I have to explain why we're having such a hard time to our management, we're modeling an equal or greater emission rate. But what was previously defined in terms as stack layouts, stack heights, et cetera -- what was previously defined to fit within 100 micrograms, now

has to fit within ten. I don't think any of us need a model to actually tell us that this is going to be a challenge and we're left with this question of saying can these emissions fit in this new standard?

If we look at the monitoring data, I think we'd be encouraged and we'd say, yeah, I think we could be okay. This -- I have a couple of examples here, no doubt there's other cases around the country that you might be familiar with or have thought of or be aware of, but there's one case we know of with a member company where there's a federal reference to PM2.5 monitor that's located less than two kilometers away from a large integrated tissue mill and wood products operation. And if you look at the state's summary report, what they quantified as the design value, three year average with the 98th percentile, it's 20.8 compared to the standard of 35. That's a good result.

There's another case where a state has deployed a special purpose, high concentration PM2.5 monitor within 25 kilometers of not just a big tissue mill, but also a refinery, chemical plant, power plant, coal and coke handling operation. You look at that data, you get a design value 98th percentile of 20.7. A -- it's not too different. B -- it's pretty far

under the standard. That case is interesting because you go 120 kilometers upwind to what that state calls a regional scale general background monitor and the result is 18.3. It's not that different. It's a little bit lower. And it's comfortably below the standards.

So, we consistently see these monitor design values in a range of about 18 to 26, which is just, roughly, 50 to 75 percent of the NAAQS standard and so we think, great. That's an ample margin relative to the standard. But, it leaves very little room if we take that design value and add it to a conservative model result to show compliance under the standard. So, can the emissions fit? We go back to the modeling and we see the answer is usually no following the current guidance. So, the examples I want to talk about are some PM2.5 modeling analyses following the current guidance, limited to the characteristic sources at an integrated pulp and paper mills which are utility boilers making power and steam and the chemical recovery units which are, again, the sort of unique characteristic source of our industry and a very important part of the process.

So, we can look at modeling results for a different -- for the different scenarios of emissions

control. So, we're going to look at some results where a source has already implemented the existing source recovery MACT standards that apply for our industry and assume that they are going to meet or they have their plan to meet the existing Boiler MACT standards that would apply.

And then we can also look at a case where -- take the controls a step further and assume that that facility could just put on or achieve the new source MACT levels of control which are about 40 to 85 percent lower than the existing and compare the results in those cases. And when we formulate this, you know, we're doing this by applying a typical PM2.5 size distribution, adding back in the condensable PM to make sure all those are accounted for.

I would note I've left out the fugitive sources here, which isn't to say they're not important. Obviously, they are and we definitely appreciate the work that we heard about yesterday in getting best practices because that is important, but I'm leaving them out so as not to confound the analyses or distract from, sort of, the fundamental issues here. I'm not including the regional sources. We'll get to them once we've solved our own problems. And secondary impacts aren't included. Under the current guidance they

haven't been yet. We'll obviously come back and think about what that is.

So, we'll do this and we'll show the results under the current guidance using the maximum model result added to a 98th percentile background and then go into the other extreme where we hope some guidance might lead us is the more -- the more -- approach what we call the paired sums or tier three where we simulate an hourly background concentration in the model. Use that capability in the model to give us the overall 98th percentile or highest eighth high, matching the form of the standard.

So, there's a ton of information on this slide. This was a slide designed for a ten-minute presentation. I don't have much more time than that so I'm going to hit some highlights here. What I'll do is focus on what we've just termed Mill A here. We've got three different mills here. I'll just focus on the one because the results are consistent as you look at the different scenarios. What you find in Mill A this is -- this would have emissions of about 100 pounds per hour total from the chemical recovery process. Maybe another 50 pounds an hour of boiler emissions in there once you've got Boiler MACT installed. And this would be the existing technology case and if we use the

existing guidance for the maximum model result plus the 98th percentile, the result we would get is 82.6. Obviously, that's over the standard. So, if we go to the other extreme and look at the highest eighth high and a paired sums approach, the result we get would be 48.5.

So, I'll stop right there and address a couple things with that difference. We see substantial differences in the magnitude of impacts at the highest first high and the highest eighth high levels. Of course, accounting for the background and using these different approaches. We suspect and we can, in our comments, look into greater detail that we'll submit. We suspect that what we're seeing is some sensitivity of these very maximum results to the model performance of low wind speeds and that we heard about yesterday and that those low wind speed conditions are more frequent when you have AERMINUTE that is generating these data at a very low wind speed threshold. So, we suspect that's what's going on.

The other thing I would note is that, you know, that's a big difference in the high first high and the highest eighth high however you look at it. Just by the magnitude of that case, it's 34 micrograms. Another way to look at it would be to say

the highest eighth high is 59 percent of the highest first high. Maybe I should have inverted that to say that the highest first high is 70 percent higher than the highest eighth high. But whichever way you look at that difference, when I look at that number, what we would wonder is is that what EPA intended or expected when saying this is a screening technique. We're concerned we're not accounting for the secondary transformation so, you know, that's the difference we see and that's how much that guidance means in a real world application. And again, I'm focusing on the one, but I think we find those results pretty consistently in the different scenarios we look at.

So, if that guidance or if that recommendation comes about because of concerns about secondary -- accounting for the secondary formation impact, then we're left with questions like is it reasonable because we don't necessarily expect those impacts to be at the same time and the same place?

George had alluded to the NACAA study which looked at different ways of characterizing that secondary formation impact and someone will correct me, I'm sure in the Q and A if I'm wrong in how I interpreted it, but I saw it in a number of places. So, I felt like I read it right where it said that in

all cases where they looked at secondary formation using photochemical models or various offset approaches, in all cases secondary formation accounts for less than or equal to one microgram of PM2.5. So, if we're trying to protect ourselves for one microgram, you know, here's cases where we're doing it by adding 34 micrograms or a substantial amount higher than that amount. So, that's a concern.

The second part of this slide that I would highlight is on the bottom where we move to the second scenario of assuming a better level of control and running that through the model and, if we follow the current guidance, we get an impact of 46.8. If we run it through the paired sums and take the eighth high approach, we would get a value of about 31.

So, we're doing better, but here we get to the conclusion that's very alarming which is that no scenario we looked at, even with state of the art control, would show attainment following the current guidance. And that is a very significant philosophical milestone to say that at state-of-the-art levels of control, we're following the guidance and, frankly, we're not that close to complying with the standard.

So, in the light of all this, we'd ask, you know, is there room to then back -- add back in the

fugitive source impacts? We look forward to applying those best practice techniques and, you know, I think we can. Can we fit in regional source impacts if we're located in a dense industrial area? I don't know. Is there room for secondary formation in these impacts once we add them? You know, maybe, maybe not. Obviously, we're going to exercise this new draft guidance and see if it could work using the qualitative or semi-qualitative approaches.

I would note that sort of working off of the NACAA recommendations and the offset approaches, what we saw was that we typically expect to add somewhere between, I think it's about 4 to 15 percent to a modeled direct PM2.5 impact based on the characteristic emission rates of PM2.5 and the precursors from these kind of operations. So, at the level of the standard, 15 percent is more than three micrograms. You know, that's a substantial amount if you only have ten to work with, so it's hard to say. And, of course, behind all this is looming a revised PM2.5 standard. So, if it were to happen to go down as 30 which is perhaps the lowest we've heard might be the proposal, you know, we see some cases where we would be encouraged that, yeah, it might work. But once you add everything back in, it's still going to be a challenge.

How am I doing on time? Can I take another minute or two?

I have this slide, it's under the heading of AERMINUTE, but it probably speaks more generally to the concept of model stability. And this example, I'll try to go through it very quickly.

In those states that I mentioned where modeling is done frequently for minor permit applications or operating permit renewals creates a really rich history of model results in a regulatory context and all of the data, of course, that went into those models. So, we looked at an experiment where we took a source, assumed it was static, no changes, no growth, which obviously is not what we hope. We want to find opportunities for growth, but let's keep all the model inputs equal and look at the model results over the past 11 years when we've had various versions of the model and certainly various versions of the inputs that go into it in terms of the MET data and the processors that go into it.

So, this is a little bit concerning and again, this is obviously just one case. Maybe other people have looked at this sort of thing, maybe not. But a couple of alarming things jumped out here and I'm not saying the answers or this is completely right or

wrong, but I think it is important. The key findings are summarized here looking at the various model tests over time. Starting back in 2001, if we were using the tools available at that time, you get a model result. We normalize that to 100 and did the various exercises with model versions and meteorological data over the years up and to the current day version of AERMOD 12060 with AERMET 11059 and AERMINUTE 11325. And the couple of things that stand out here in the results column is there can be significant run to run variability. And obviously, we don't have to go through why that it is, but it really seems to be due to the meteorological data. When you change the meteorological data sets, you're picking up periods of time or episodes that lead to high concentrations in the model and, you know, these pop out in the results.

Focus really on just the recent history, we found that introducing AERMINUTE to the results did cause a pretty significant jump of about 25 percent to this 24-hour average model result. And, just in summary, we're at a point now where we've run this model, inputs being equal except for the model version, meteorology, et cetera, and we find our model result is 38 percent higher than it was 11 years ago around the vintage of the 7th Modeling Conference and the tools we

had available at that time.

Is this right? Is this wrong? I don't know. There's no monitoring data to go along with this. We wouldn't expect a correlation if there was because of the way we run the models in a regulatory context. But again, you know, I think it's important and it speaks to the necessity of having good notice and comment and opportunities to consider consequence analyses of changing, not just model versions and guidance, but even the inputs to the models themselves. So, with that, it's obviously premature to call anything here today a conclusion. But in terms of observations and comments, let that be my summary that AF&PA is concerned about the current EPA guidance being overly conservative in situations where it just can't practically be implemented.

So, we appreciate the efforts that we've heard about to develop best practices for fugitive source modeling. That's very important to us. And also identify and correct systematic deficiencies in model performance for things like the low wind speeds that we heard about yesterday. Obviously, we eagerly anticipate getting the draft guidance and working through it. We appreciate the opportunity to comment on it and certainly plan to take advantage of that.

We're going to be focused on how the guidance can be applied to develop sound, unbiased estimates of our impacts, including background and secondary formation, emphasizing the consistency on temporal and spatial scales of the background concentrations and secondary impacts. And obviously, we're in favor of reasonable and practical implementation of new standards in modeling guidance. We think it's very important as we've heard many times to critically apply this guidance in practice and promote stability of the modeling, especially during these times of regulatory implementation.

So, where we're looking forward over the next decade of having to implement a number of control programs, the prospect of this extrapolating out in our model results continue to change -- is very challenging. It makes our decision making process very difficult. And we suspect that there will be opportunities or we'll need to look at opportunities to revisit some of the traditional approaches that are reflected in the guidance such as the way ambient air is modeled as well as variable emissions which we'll hear about more.

So, I'll stop my remarks there. Again, thanks for the time and turn it back over to George for

Q and A.

**MR. BRIDGERS:** I just checked with Jim and he can be a resource if there's some questions specific to the NACAA comments, but -- and Randy, I imagine you're the same, wherever Randy is. He can be a resource for questions, but probably not wanting to say any other remarks. So, at this time, we'll have a real quick Q and A session, unless there's a lot of comments and we can kind of structure the schedule. So, it's open mic time. And in case there aren't any questions or while people are thinking about questions, I want to apologize for the slides on the NAAQS comparison that had it with the NAAQS versus the SIL. We'll have that corrected and uploaded to the web probably later this afternoon.

**AUDIENCE MEMBER:** Bob Paine, AECOM. Just wanted to understand when I heard about adding model monitors to the background. As I recall for NO2 and SO2, it's adding the 99th models to the 99th or 98th monitor and is it not -- would it be consistent to do that for PM2.5 as well? And why not go to the tier -- three tier approach?

**MR. BRIDGERS:** In all fairness, Bob, I'm going to absolutely defer this to the expert that is not sitting at the table which is Roger Brode. I could

try to answer it, but I know that Roger would have to clean up my mess.

**MR. BRODE:** Well, I think one point is that what's been provided in the guidance is sort of a starting point. This is something that we feel is appropriate for these reasons. There's always the opportunity to propose alternative approaches with adequate justification that can be considered in specific cases.

I think it's a legitimate question and I think part of it is the complication introduced for PM2.5 of the secondary component. So, I think as we gain a better understanding of that, it's certainly possible that our guidance may evolve and examples like this will help inform us as to what the issues are and, you know, how to move forward if we can to improve the guidance.

I did have a couple of questions specifically about this and I appreciate you articulating your specific concerns for us and actually, I don't mind complaining. It's the whining, maybe, that's more of a problem.

**MR. GESSER:** I hope I didn't do that.

**MR. BRODE:** I'm wondering if you could explain in more detail, in terms of the paired sums,

how you actually implemented that for the daily standard? You know, it's a monitor -- a continuous monitor or?

**MR. GESSER:** Yeah. In those cases, I believe in every case, they were continuous monitors. We're mindful and obviously didn't go into details about the difference in the quality of the monitors and we recognize that that's part of a protocol process that would fully evaluate the appropriateness of the models, the quality of the data filling, et cetera. In those cases, what we found in the real world and, of course, it's not every case, is there's usually some continuous monitors upwind and downwind within about 100 or so kilometers and that's the data that we have to work with so, we worked with it.

We would look at the information complied by states in terms of their network assessments and monitoring plans. Those usually have indicators of quality, purpose of the monitors, settings, and if we can make a comparison, for example, of the county level emissions of where our source is and is the monitor exposed in a similar way. We would make those types of considerations.

Looking at those kind of summaries that the states provide, they often show correlations

between the different areas and we try to take advantage of that as well. So, it was continuous monitors implemented in the model with the hourly background files that we knew about.

**MR. BRODE:** I was actually more asking about just mechanically. So, you did generate an hourly background file input to the model that had a constant value for a given day and then --

**MR. GESSER:** Hour. Continuous hourly values.

**MR. BRODE:** They were continuous hourly values. Okay. I just wanted to clarify that. I guess the other question I was going to have is if you had actually looked at how the results you showed there would compare if you had combined the 98th percentile monitored with 98th percentile monitored results.

**MR. GESSER:** Yeah, so, I obviously didn't show that. I guess that would fall in between, sort of, the two extremes I showed.

**MR. BRODE:** Well, that's kind of in response to Bob's question as well.

**MR. GESSER:** I think maybe the way I would answer it is to say when we've looked at the seasonal variability, for example, you know, of course with seasonal variability you can sometimes end up with

seasonal 98th percentiles that are higher than your overall 98th percentile. And wouldn't you know it, the model result always ends up there. So, it ends up being higher or, in many cases, it does. So, I think it seems like going to the extreme of the paired sums, hourly, whether that's implemented with hourly continuous data or the one and three type data which, I think, we've yet to see some details about how to do that. I see us heading toward that approach.

**MR. BRODE:** Did you ever -- did you look at just adding the 98th percentile monitored to a single 98th percentile modeled?

**MR. GESSER:** Yeah. It would still be levels that comparable to what I showed since that 20 -- that 98th percentile background is still usually about 20 to 25. So, adding that to a model result that may be still in the 20s even is going to be above the standard.

**MR. BRODE:** So, that was somewhere between the two?

**MR. GESSER:** It would be somewhere between.

**MR. BRODE:** Okay. Thank you.

**AUDIENCE MEMBER:** I guess I just wanted to add, there we go. Usually I don't have a problem

with talking loud. Meg Patulski, OTAQ. I just wanted to add since so much of your guidance relies on what we worked with you on for a hot spot guidance that we've gotten similar questions about why did you pick 98th percentile? Why not in the 99th percentile? And things like that. And obviously, without -- I guess I'll just say what's unsaid is we had to be consistent with the NAAQS and so a lot of this was something that we, you know, had to develop these tools to be consistent with how the NAAQS itself was created. So, that's why it's the 98th percentile and there are all these hoops that we're walking through. If the NAAQS was different, we would have different calculations. And I was also interested in what the tier two results would have been, so.

**MR. BRODE:** I appreciate you clarifying for the Record that it's OTAQ's fault.

**MS. PATULSKI:** Well, just, you know, it was a several month effort to create Section 9 of our guidance and also the tool that we've created in our training to calculate the tier two -- NAAQS tier two results. So, it was definitely an undertaking, so.

**MR. BRODE:** Well, on a more serious note, I think it's good to acknowledge that interaction that we had with OTAQ was very helpful for us and

timely for us to prepare for what we're dealing with now.

**MR. FOX:** That experience let us know earlier rather than later how complicated this is and how difficult it is, but the bottom line is that we want to have a realistic and representative characterization as if there were a monitor there in terms of these receptors and the like. Not double count, but appropriately account for the contributions of the project source. That project source is the primary emissions. If necessary, that project source is contributions to secondary formation through its precursor emissions as well as the representative background which may be composed of, you know, PM2.5, but also there's an urban increment, so each area is very unique and different, you know, adding up different parts of the distribution can be quite complicated and so we're trying to work through and is evidenced by Roger's questions and we try to do some different simulations.

The more information we can have in terms of looking at real situations and seeing how well the different approaches mimic reality I think would help us in terms of coming up with an approach that we feel and you all feel comfortable with.

**AUDIENCE MEMBER:** Mike Kiss, Virginia DEQ. This question is for George. I saw no reference in the guidance with respect to increment consumption and expansion. And what we're seeing in some of our analyses already is that the class one increment, in particular, has already been consumed by direct PM2.5 emissions. And we're expecting to see a lot of increment expansion through the transport rule and secondary or precursor reductions sulfates and nitrates. And I think there's going to be some difficulty in class one modeling in pairing the secondary reductions in time and space with the direct PM emissions. For example, from a combined cycle plan. Is there any thought to adding that to your guidance document?

**MR. BRIDGERS:** Since the question was specifically addressed to me, is Ryan Corrales still in the room?

In all seriousness, at this time the guidance does not include anything with respect to modeling for increment. Obviously, moving forward, it's a draft document. It's a living document. Even when it goes final, it's a living document just like the other what we affectionately have called for years Brian's guidance, but the photochemical regional haze

PM2.5 modeling guidance. And consideration for increment could very well be added to it. I don't know that there's any plans at the immediate time. Right now, the immediate plan is to get what we've got out, but it would be taken under advisement.

**AUDIENCE MEMBER:** Okay. Thank you.

**MR. FOX:** I think we're, right now, focusing on the NAAQS compliance demonstration and getting that in a firm foundation there, but you're point is well taken in that we need to address increment. There's some nuances there and you mentioned them in terms of increment expansion and other types of things that get into other types of issues in terms of how best to account for that and I think, as George indicated, we're going to have to work with our policy division and both from a policy, legal, and technical standpoint, be able to do things that allow for an appropriate analysis. In that context, it's not something that we're ignoring. It's kind of first things first and we would welcome comments along those lines in terms of specific experiences with details about problems that you're encountering or issues that you foresee. That would allow us then, you know, as we work throughout this year to address that and maybe bring that in - hopefully, bring that in

later on in the year. Once we've got a firmer foundation of how we're doing it -- how we're addressing issues from a NAAQS compliance standpoint because that will give us a foundation to then view increment.

**AUDIENCE MEMBER:** Yeah. I think we expect to see increment expansion with these -- the transport rule, for example. I think the key is going to be trying to pair those reductions in time and space on the secondary side with the direct consumption from natural gas type facilities. Thank you.

**MR. BRIDGERS:** And Mike, your comments, thank you. Actually, Dan Deroeck just walked into the room, so it's good that there are other people within our policy division that are hearing these requests.

**AUDIENCE MEMBER:** Jim Boylan, Georgia EPD. I wanted to follow up on the NACAA recommendations for PM secondary formation. I know George joked with Roger about putting chemistry into AERMOD, but my question is does EPA have any plans to put chemistry into AERMOD?

**MR. BRIDGERS:** Fair question.

**MR. FOX:** No. No, we're, at this point, and you'll see more this afternoon in terms of the emerging models and techniques, there are existing

models that are out there that address secondary formation and are more suitable and have advanced in terms of the science and the like. At this point, we would like to look at and evaluate the existing capabilities and capacity for those models to meet those needs. And hoping, expecting that we'll be able to find in those existing models or modifications to those existing models that capacity or capability so that we're not thinking of adding PM2.5 chemistry or ozone chemistry to AERMOD. And that seems like the best approach for now.

**AUDIENCE MEMBER:** I guess, is it fair to say that it's not completely out of the realm of possibility, but it's unlikely?

**MR. FOX:** Well I -- it just changed. It would be EPA engaging in developing its own new model to account for chemistry that would go through the promulgation and it would probably be more challenging and difficult to take the current construct in AERMOD and add chemistry than to just embrace those more complete, full, elegant characterizations of that in existing models.

Now, as George noted, and I think we all understand and appreciate, those types of models then have resource constraints in terms of their ability to

be used in this context. And so, as indicated in the McCarthy petition grant and George reiterated, the pursuit of updating Appendix W to account for ozone impacts and secondary PM2.5 will not necessarily lead us to a model and a model would be used in every and all cases.

We, in some ways, see the use of a full scale model as an exception rather than rule in that the vast majority of cases where we're dealing with precursor emissions and the secondary impacts from the project source could be handled through other techniques if they could be developed with a credible basis. Either using photochemical models, or as was eluded to, both in the NACAA guidance and what we'll put forth shortly, credible techniques to base on an area-by-area basis representation of the impacts through offset ratios, you know, sensitivities through, you know, techniques in photochemical models like DDM, and those are things that we're looking at. And as referenced in George's presentation, this afternoon there'll be a presentation on how a similar situation was dealt with in Sydney, Australia for ozone and if the techniques can get there for secondary PM, maybe that's a viable option to be able to pursue to be able to come up with those types of characterization in a

screening tool standpoint, short of full scale modeling. But I think all those things are on the table and we welcome everybody's thoughts and comments on existing capabilities that you're aware of or experiences in dealing with those models. And from an operations standpoint, thinking in the context of their suitability for operation and use in a permit environment as well as any scientific or technical aspects of those models.

**AUDIENCE MEMBER:** George Schewe of Trinity. I've got a question for Ryan. Ryan, the last table there AERMET and AERMOD and all that good stuff. Did you see any differences in any of your results between the different mills or anything? Flat versus complex terrain?

**MR. GESSER:** That's a good point. I probably, if I had more time, would have done a better job of setting the stage of those examples. These really were, I think, exclusively flat terrain situations. It was a pretty simple analysis insofar as just point sources that are unambiguous. Everybody knows how to model them in a flat terrain without any complex situation.

**MR. FOX:** And I would just add that those types of examples are very useful to look at the

sensitivity. As you noted, the meteorological data seemed to be driving quite a bit. Actually, if you look at the results, in some ways the models showed some, you know, some stability in terms of use over time which was good. It seemed like some of the inputs and the like were driving those things and so the more that we could, you know, engage and investigate those types of sensitivities would be useful. And, hopefully, as an illustration of that for NO2 and SO2, James Thurman and Erik Snyder will be providing the AIWG results that tried to get at those types of issues and perhaps we should consider something similar for PM2.5 down the road.

**AUDIENCE MEMBER:** John Gill, EnviroMet. As a follow-up to George's question with regard to the same table. As a fellow modeler, you mentioned that you kept the MET data the same. Did that include how you were more concerned about your sectoring, especially with the implementation changes to the sector guidance a few years ago and were you using on-site data for that as well?

**MR. GESSER:** No. If I gave that impression, I should clarify. The MET data was not the same. It was -- the sources were constant throughout that and this was a situation where the state would

provide data or suggest the data to be used, arguably without thoroughly reviewing the consequences or what might be lurking in that data or just legitimately explain big differences like that you see in the model results. And I mean, they could have been legitimate. There just wasn't any consideration of that.

**MR. BRIDGERS:** Maybe one last question and then we're going to have to break.

**AUDIENCE MEMBER:** Hello. This is Julie Mitchell with URS Corporation and I have a question. San Joaquin Valley APCD has put out a guidance document on how to model PM2.5. It's got a couple tiers, but the tier two approach does talk about secondary formation and including that in your AERMOD analyses. Have you reviewed through that and what is your findings or feelings on the appropriateness of that analysis?

**MR. BRIDGERS:** If I'm not mistaken, Leland had shared that with us at some point in the past. I'm going to say that it's something that's not been a focal point as of now, but it's something that he, at least, extended our way, but in the myriad of everything that we -- I haven't specifically reviewed the document. No.

**MR. FOX:** If I recall correctly, I think

they were using the same type of approach that the NACAA recommendations had with offset ratios and the like and so, again, as was indicated, those types of things would have to be approved on a case-by-case basis in terms of their appropriateness for use in that area representing the type of chemical regime and the like that exists. So, you know, at this point in time, we haven't reviewed it in detail, but I believe it's following something similar to what was recommended in the NACAA. Run AERMOD for the primary and then have some offset ratio account for the secondary contribution from the project source.

**MS. MITCHELL:** Right. And he was advising to run AERMOD for the SO2 and NO2 and then do a post-processing with taking your -- an offset ratio that was appropriate so that you could determine what the impacts of the secondary SO2 and NO2 could be and then combine that with the PM2.5. Thanks.

**MR. BRIDGERS:** Thanks. With that, I think we need to move on to our break. Since we're a little bit over, let's go ahead and take till 10:40 -- to 10:40. And as everybody is leaving, I also want to thank again the volunteer effort by Jim Boylan and Randy Robinson and also Ryan Gesser for the panel this morning.

(WHEREUPON, a brief recess was taken.)

**MR. BRIDGERS:** Okay, if we could have everybody start taking their seats it would be greatly appreciated. Thank you.

I feel like I'm standing in front of the television camera and they're doing this, you know, extend it out and it's that cross talk across the banker desk that is just gibberish. So, hopefully, Tyler will be back in here soon because he had some points he wanted to make here as we kick off the 1-hour NO2 and SO2 and speaking of the devil, here's the Michigan man right here.

**MR. FOX:** And for the Record, this is water, not coffee so when you see me gulping it I'm not drinking coffee. I already had my dose. All right. So, I'm going to go through what we've been doing to try and address some of the challenges that we're all recognizing in modeling compliance under the new 1-hour NO2 and SO2 NAAQS. I'll also try and get us back on schedule and try and provide more time for James and Erik because I think those type of results and the like are more important and provide more real information for everybody to chew on.

So, we all are very familiar with the new 1-hour NO2 standard of 100 ppb based on a 98th

percentile annual distribution of daily maximum 1-hour values and that was effective April 12th, 2010. That was followed shortly afterwards with a new 1-hour SO2 standard of 75 ppb based on the 99th percentile of that same distribution and that was effective in August.

As part of those rules, as of the effective date, PSD requirements came into effect and so we followed up with clarification memos for the NO2 standard and the SO2 standard and I'll go through those briefly. But I'll try and focus most of the discussion on the March 1st guidance memo and aspects there that I think or at least we thought were very critical to get out there to the community and allow for flexibility to address issues that we have become aware of. And I have to stress the fact that we understand and feel your pain. We are dealing with these issues as best we can. That March guidance was issued as fast as we could to address and to provide much needed flexibility under these standards and I recognize that some folks would have liked review of those, but that would have just dragged that process out. So, we're facing the trade-off between going through longer review processes versus getting things out that we think are valuable and flexible and allow for more appropriate analyses for compliance.

But that said, these are guidance, so review and follow-up and additional guidance can always come afterwards to address certain issues and I think it's very valuable to have real examples as James and Erik and others, Ryan, provided earlier to inform the development of that future guidance and that AIWG effort was intended to evaluate the existing guidance including flexibilities in the March guidance as well as give us some understanding of what new guidance may be necessary. And this just provides the two clarification notes in terms of the applicability of Appendix W under those two new 1-hour standards as well as the additional clarification that we provided in March of last year.

So, as I said, the NAAQS for NO2 was revised in 2010. The monitoring guidance, design values, were based on three year averages, but it's important to note that that does not preempt or alter the Appendix W requirement for use of one year of site specific data which is preferred or the use of five years National Weather Service data.

For NO2, the guidance was more exhaustive, I guess, or detailed than for SO2. We established that under Appendix W, AERMOD is the preferred model for estimating NO2 impacts in the near-

field and we saw the existing three-tiered screening approach in Appendix W as applicable to this new 1-hour standard with some different considerations. And I'll go through the details in terms of those three tiers. Thankfully, we had that tiered approach and screening techniques available as part of the third tier for use here.

So, in terms of the applicability of these three tiers, tier one is obviously a conservative test of full conversion and it can be used without any justification just as it was before. And under the annual standard, we understand and acknowledge that most applicants were able to use either the first tier or the second tier in demonstrating compliance. And that we have not had as much experience with the third tier and these detailed screening methods, but under the 1-hour standard, we certainly have and will and need to.

The second tier is applicable in many cases, but needs to have additional consideration given the nature of how that ambient ratio method applies and given the default ratio of .75, at least at the time of this clarification memo, as being representative of area wide quasi equilibrium conditions. So, in there that was designed more specifically to the annual

standards so we thought it was applicable but, again, additional considerations need to be there and we'll hear later this afternoon about an approach to potentially modify that -- that has been brought to us.

Tier three represents more formal modeling using the existing techniques of ozone limiting method or the plume volume molar ratio method within AERMOD. And it can be used on a case-by-case basis. These are not refined methods. As I mentioned at the beginning of the day, perhaps one aspect of updating Appendix W is doing what we need to to allow these to be seen and used as refined methods. But with these techniques come greater requirements to inform the model appropriately and that gets at the representativeness of the background ozone data as well as the in-stack ratios. And those are obviously much more important as you analyze and assess the 1-hour NAAQS.

So, in the memo we went into detail about the tier three approaches, recognizing that given our lack of direct application and use of these techniques, there wasn't as much understanding across the full community. We did the best we could to try and provide information that we thought was very relevant and will continue to do so.

OLM is specifically referenced in Appendix W and PVMRM is also considered in that category until more robust evaluations can be done. Both of them are available as non-regulatory default options in AERMOD which requires a justification and approval from the regional office on a case-by-case basis.

One note here is that as part of the March guidance, we did provide a number of evaluations of these tools and I believe Roger may go through some of those in the next discussion. And we did feel as if some of the work that we did could be used as the justification to try and reduce the burden or hurdle of demonstrating their applicability on a case-by-case basis. That's not to mean that you don't have to work with the regional authority or the regional office to get approval, but you can reference the information that we put together and do maybe a little bit more work to provide sufficient justification. But we did do, we think, a good job of providing some information that can be leveraged in that case.

We also noted that applications of OLM in AERMOD should routinely use the OLMGROUP ALL for combining plumes. There had been some confusion there and now that the treatment is within AERMOD rather than

outside of AERMOD, some of the past issues have been resolved and we feel more comfortable using that OLMGROUP ALL in those circumstances.

We have several documents listed there. As I noted, Roger did some additional evaluations which we believe showed encouraging results, but we do recognize that there is a sparsity of information and data. It's very limited in order to move forward in the next step but, hopefully, we can work towards that in terms of considering PVMRM as a refined method in the future. And in regard to those evaluations, we extended those and updated them for predicting hourly NO2 concentrations.

For the SO2 NAAQS, again, the same holds even though we have a three-year averaging time for the NAAQS design values that does not preempt or alter Appendix W's requirement for use of one year on site MET data and the five years of National Weather Service data.

In terms of the clarification, we did put forth the fact that we believe that the current guidance in Appendix W that was done in the context of the previous twenty-four, annual, and the three-hour secondary SO2 NAAQS were generally applicable to the 1-hour standard and that AERMOD, just as in the case of

NO2, was the preferred model for estimating these impacts in the near-field.

So, then we had heard from a number of folks through both the state and local tribal agencies, the regional offices in terms of specific permits and experiences, and also directly from you all, the stakeholder community. Either directly through memos or other information provided to either me or Chet Wayland or our upper management, there were a number of concerns. I think it was mentioned earlier, the impossibility of demonstrating compliance here and particular issues rose to the top of the list in terms of priorities for us.

So, in March we issued after probably three or four months of work on these issues, a number of things that we felt were necessary to assist you all in demonstrating compliance here. I should say that even though the memo itself was referenced specifically for NO2, the options and treatment here that are put forth are relevant for SO2 except, of course, the recommendations related to the tiered approach for NO2.

So, we clarified the procedures for analyzing results given the new form of the NAAQS. We modified the recommendations in terms of the tier two ambient ratio and the in-stack ratio defaults for the

tier three options. Again, we modified those national defaults that can be used without any further justification. We always prefer and would look for more appropriate source area specific information for use in that situation and so those defaults in no way, shape, or form preempt folks from informing the model in a more appropriate way and we encourage you to do so.

Thirdly, we addressed the treatment of intermittent emissions. A prime example would be emergency generators. As we were finding that in a number of situations, more and more the previous way of treating these sources under the annual NO2 NAAQS and even under the pre-existing SO2 NAAQS, they were not the controlling scenarios and now, under the hourly NAAQS, they were becoming the controlling scenarios in terms of determining whether or not one would get a permit or not.

Given the nature of the standard and the nature of those sources, we put forth some flexibility there in terms of treating those sources under these two NAAQS. Then we talked about recommendations regarding combining nearby background sources and the modeling there with the monitored contributions in the case of cumulative analyses. So, in terms of the form

of the standard, we suggest that or recommend that for comparison of the SIL that the impact from your source be based on a multi-year average of the highest 1-hour concentrations at each receptor. And that's consistent with the maximum contribution that a source could make at that receptor. And then, in terms of the cumulative impact analysis in determining whether or not you're causing or contributing to a violation, we would ask that you examine whether the project contributes significantly to model violations paired in time and space, including all cases where the cumulative impact exceeds the NAAQS, at or below the 98th percentile for NO2 or the 99th percentile for SO2.

And to support that, I believe within a month or so of release of this guidance, after a lot of work by Roger and James, we modified AERMOD to be able to post-process that information and support those analyses. We recognized there was a lot of angst about the post-processing requirements and the needs in order to come up with the correct values for comparison to the SIL and the NAAQS here. And we worked diligently to try and incorporate those. We considered having a separate post-processor and air post program, if you will. In the end, we decided to put these types of post-processing routines in AERMOD to support these

analyses and so we hope that that was helpful to you all because we know that there was a burden imposed initially there.

We also addressed treatment of intermittent emissions. This has come up in a number of contexts. Given the form of the standard, we highlight the fact that there is a concern that assuming continuous operations for these types of sources would effectively impose an additional level of stringency beyond what was intended in the level of the standard itself. And as a result, we recommended that the compliance demonstrations be based on emissions scenarios that can be logically assumed to be relatively continuous or which occur frequently enough to contribute significantly to the annual distribution of daily maximum 1-hour concentrations.

And I know Roger said that yesterday and we didn't expand on that too much. I think one aspect here is that we need to know the specifics of situations and cases. We're trying to address situations where emergency generators or other types of emissions scenarios that are either -- can't be projected or can't be controlled, like an emergency generator coming online and that occur fairly infrequently in terms of that situation. Those are the

types of situations that we intended to address here.

If there are planned downtimes or other types of situations where emissions spike, if those are planned, that would be an aspect where they may or may not fall in here. That's something that you would have to work with the regional office to work with. And, as noted there, routine testing and operations may be one of those things that would not be considered an intermittent source.

We've been getting questions, as an aside, related to this at both right before the conference and then yesterday as part of the conference. We -- I wrote a letter of concurrence to Region 2 based on a request from Region 2 in the context of a situation involving hydro-fracking in Region 2. And the specific request was from New York DC and Region 2 was asking for our concurrence in their review of that. And what we said was that given the information that was provided to us, that the sources in question could not be treated as an intermittent source and, therefore, could not be eliminated from the compliance demonstration or not accounted for.

There are ways in which you can account for those types of sources. If they're moving within an area, I believe that in the work that Region 10 did

in the OSC permits, their situation where rigs are moving and they actually try to account for that spatial dynamic of moving over the year, but that's a planned activity and it's known. So, that did not seem to comply with the spirit of what we were talking about here in terms of intermittent sources. So, work with the regional office and others in terms of how best to treat those types of sources in that situation. But that letter was just a concurrence and was part of the public record to submit as a public comment in that context that we concurred with their assessment in terms of saying that those sources were not -- could not be treated under this guidance as intermittent sources and, therefore, not included at all. So, hopefully that helps clarify a little bit of that and if there are questions, we can deal with those separately.

In terms of determining background concentrations, cumulative analyses will be required, as you know, if the emissions exceed the interim SIL. Those were established as part of the memos that went out soon after the standards were set. What we try to do is address the components of the cumulative impact analysis including identification of the nearby sources to include in the modeling inventory. So, you're

explicitly modeling them and then how to combine those results with the monitored background. And what we stressed here, as well as in the previous memos in regards to the NO2 and SO2 standards, is that we advised and cautioned against the literal and uncritical application of very prescriptive procedures, particularly those that one will find in the 1990 draft NSR workshop manual. In some of those cases, following that manual will result in overly conservative types of assessments. And the challenge here is to find a proper balance between balancing those factors that are appropriate to account for versus those that, under the context of this new 1-hour standard, would be appropriate.

So, for example, the straightforward application of 50 kilometers and then going out another 50 kilometers in terms of your area of influence and the sources that you would include in your modeling analysis, frankly, is not something that we see as necessary and should be reconsidered. Again, everything should be viewed in the case or the context of the situation at hand. But blindly following the workshop manual in that regard, I believe as Roger said yesterday and we have said elsewhere, if one wants to go down that road and do an obviously overly

conservative analysis, we won't stop you. But these guidance -- this guidance was intended to provide flexibility and, again, caution not to do that.

And so, I'll give an example at the end that we've seen recently that illustrates some of the pitfalls of that, but we urge you and the community to take advantage of the flexibilities that we're providing here. Work with the regional office to understand the particulars of how to work in that. But we've got to move away from the very prescriptive nature of things that were in the workshop that were done under the previous NAAQS.

I guess I shouldn't say this for the public Record, but I'll go ahead and say it anyway. At some point, perhaps in the future, we'll have a bonfire. A well-controlled one. We will have all of our permits in place before doing that.

So, but joking aside, really, we do need you all in the community to work effectively with us, with this guidance to take advantage of the flexibility. If you, you know, hold onto these old techniques of 20, 30 years ago, we acknowledge and admit you're going to have problems and you're going to have issues. So, we hope that we can work together. As an illustration of that, in Appendix W we have the

concept of a significant concentration gradient. And we identify a significant concentration gradient in the vicinity of the source as the sole criterion for identifying which nearby sources to model.

Now, there's aspects of that that can be somewhat complicated, but it's not impossible to define that. It may be that, as we move forward in updating Appendix W, we can work towards having a more concrete understanding and example of how we define that significant concentration gradient, what it means, and how best to put in practice an approach to identifying in a more, I guess, prescriptive way what nearby sources to model. But right now, we need to work with what we've got.

So, we did not comprehensively define the term given the uniqueness of each modeling situation, but if we can get an understanding of these situations in the context of these standards, hopefully, we can provide more information and refine the guidance and ultimately perhaps update Appendix W.

So, these gradients in the vicinity of the source imply that nearby sources' potential interaction with the proposed source impacts will not be represented well by monitored concentrations at a specific location. So, there's a feedback mechanism

between the monitored background that you're going to use and the nearby sources and that requires some best professional judgment and assessment in terms of making sure you're not double counting and making sure that you're properly accounting for the concentrations gradients in and about the project source.

We hear about the nominal 50 kilometer distance and other things. I know in the workshop last year there were questions that, well now EPA, you're suggesting that the focus on nearby sources is within about 10 kilometers. We're trying to provide information that's helpful to you all, but there is no bright line. Obviously, with a 1-hour standard, we want to make sure that you're focusing on sources in closer proximity to the project source or those that are, you know, going to be important to account for in terms of potential violations and contribution of the new project source to those violations. And it does suggest that you need to look in a tighter domain. What specifically that domain is, you need to work with your regional office and the like to design that on a case-by-case basis and as we do more modeling and understand better the nature of NO2 and SO2 in that context, we can provide more concrete examples. And then, perhaps, refine that type of information.

And then, one of the more popular topics is how we combined the monitored to modeled concentrations. In the interest of time, I'm going to move down to the next slide where in the March 1st memo -- well, in June, we identified the overall highest 1-hour monitored background as a first tier. So, in a tiered approach, that's a conservative approach. In the March memo, we suggested a new first tier and that being the monitored design value and expressed that that should be acceptable as a less conservative first tier. And then given the form of the standards both for NO2 and SO2, we suggested then looking at the diurnal and seasonal patterns of those concentrations to then look and see whether or not more refined combination of those monitored concentrations is appropriate.

And so, again, I don't want to read this stuff but, basically, based on the guidance and Appendix W, again, all the things that we're putting forth are rooted in Appendix W. That we suggested that the use of the multi-year averages of the 98th percentile of the available background concentrations by season and hour of day is an appropriate methodology for the 1-hour standard.

So, we've provided three tiers here. A

first tier, admittedly conservative. A second tier that is fairly easy to implement, but still may have some nature of conservatism to it. But then a third tier, here, that allows one to take advantage in an appropriate way the combination of those data on a seasonal basis.

And here for Salt Lake City, you can see, I believe this was the same example provided in the guidance. You've got the 1-hour design value there. You've got the standard level up here. And each of the different colors are the 98th percentile for winter, spring, summer, and fall. And by hour of day. So, that information can be provided by the background monitor and we've suggested a more refined approach to combining those that we feel both provides more appropriateness and reality to a particular situation and it's firmly rooted in the data that are available. And appropriate for combining in a way that, at least at this point in time, we feel comfortable with people using.

So, that really covers the guidance that we put out in March. Again, we've gotten comments in terms of the sufficiency of that guidance and additional issues. Maybe not going far enough in certain areas and the like and we welcome those types

of comments to push us to better address your issues.

I guess two things that I want to mention before I get to this slide and finish up. When we first started this process and these NAAQS came out, the first call was, you know, hey it's impossible. It can't be done. Not, you know, Dana Carvey, not going to do it. It's just -- it can't be done. Well, at the end of last year, our policy division polled the regional offices and I will get the firm numbers and we'll submit it to the docket for everybody to see, but based on that polling of the regions, we had, I believe, 27 final permits that had demonstrated compliance with the NO2 standard. Ten of those permits had used either the tier one or tier two approach to demonstrate compliance. Seventeen used the tier three approach -- either OLM or PVMRM. From what we understand, out of those 17, three used OLM and 14 used PVMRM. So, we understand and hear what you're saying in terms of the difficulty and challenges here. But we do see -- and in certain situations and this was across the entire regions. You know, there were some regions that may have had more, but every region had a final permit that had successfully demonstrated compliance with the NO2 standard. And I didn't pull the SO2 results. That doesn't seem to be as much of an issue,

but it was, and I believe the number was closer to the -- in the handful.

But I did want to stress, and again, we'll compile this information and put it in the docket for you to see, that people are applying the guidance. People are successfully completing their compliance demonstration. And we appreciate and kudos to those who are moving forward and using these tools and techniques and guidance successfully.

With that said, as you all know through the Clearinghouse process and other venues or avenues, we get pulled into situations where there are issues in terms of demonstrating compliance. So, I don't want to discount or diminish the fact that there are serious challenges here. But I want to provide an example and I don't want to say any names and I'll try to be very generic in characterizing this which, I think from our standpoint, puts a burden and obligation on you all in terms of applying the guidance appropriately and taking advantage of the things that we're providing and working hard to provide to you.

So, we had a situation -- a source -- a combustion source was demonstrating compliance with the SO2 NAAQS under PSD. The timing of it was right around the issuance of the March guidance and then the

subsequent release of AERMOD. So, when we first heard of this application, our understanding is that they were having difficulty demonstrating compliance. In fact, they had modeled the fact that they exceeded the SIL, that they were going through the cumulative impact analysis and that they were significantly contributing to violations. And so, what was put forth was a novel approach, acknowledging that there were violations, but they were small in number. A probabilistic approach, so to speak.

Appreciate the creativity there and the like, but under the current guidance, those types of approaches you've demonstrated that there is a violation. You've got to address those types of things. We engaged through the regional office and others and early on in that process, pointed to the fact that we had March guidance. We had a new version of AERMOD. Perhaps one could run that through. They had previously post-process without the benefit of the tools that we had provided in AERMOD and so, we didn't hear anything for a little bit. Then, we -- probably in late summer or so, start hearing back about this issue and then get more details about what's going on. Still having an issue in terms of demonstrating compliance. We understood that the individuals

involved pushed back on re-running through AERMOD which we understand would have taken hours, days perhaps to do and to take advantage of the new capabilities in AERMOD. We also then got more details on the modeling and found out that they were going out 90 kilometers away and modeling an SO2 source -- 90 kilometers away and they were following the puzzle book. They were going out and modeling all of these sources.

We looked to our guidance and sent the message back that, well, it looks like the domain is much more expansive than we would suggest. To make matters even worse, we then looked and saw that they're between the project source and the source 90 kilometers away. It was a large SO2 source. I'll give them that. But there was a monitor between those two sources. And so why one wouldn't use that monitor as --

**MR. BRODE:** They did use the monitor.

**MR. FOX:** Oh? They double counted.

**MR. BRODE:** That was the monitor they used.

**MR. FOX:** Oh. Thank you.

So, to make matters even worse, you know. So then, what we did or what Roger and James and others did, is that we took the -- we got the input files and ran it through the new version of AERMOD and

found that, yes, they were above the SIL, but when you looked at the violations that they were modeling at the nearby sources -- the explicit sources that they had modeled, yes there were violations above the NAAQS level, but this source was contributing nothing. Nothing to those sources. Way -- I mean, it wasn't even close to the SIL. It was, literally, nothing. So, long story short, you know, we provided the information. I hope that the information found its way to the right places and that ultimately gets resolved.

But, you know, we need you to work with us in this context. We need you to apply the guidance. We need you to apply the tools. We need you to engage with the regional offices. This is a totally avoidable situation. And before those types of situations get to a point where they're political or other types of things where that's not the most constructive way to work these things out, we really need to, you know, engage and work better together as a community.

I'm not saying that this is what we see most often. It isn't. It's an exception to the rule and it's becoming more of an exception to the rule. But I use it as an example just to say let's critically evaluate the way in which we did things in the past. Let's embrace the fact that we do have to demonstrate

compliance under these, you know, 1-hour standards and the PM standards and the like. And let's work together to find credible, technically credible, legally defensible ways to demonstrate compliance such that you can get your permits and that we can move forward with the types of environmental protection that we need to.

So, with that -- there's again these outreach efforts that we've been engaging in to try and provide information and answer questions. As a lead-in, we've got the AERMOD Implementation Workgroup that James and Erik will cover and provide more examples. And that was a key thing in dealing with the types of issues that were being brought to us, you know, I know I said yesterday that the devil is in the details and I'll repeat it. The devil is in the details in a lot of these situations. So, the more information we have in terms of the particulars of what you're challenged with or facing, the better we can help diagnose and understand and either provide a case-by-case solution to that problem and through that build better guidance or address a real issue in our guidance and provide that in timely fashion so that not only that project source but other sources facing that same type of challenge can benefit from that.

So, I'll end now and turn it over, I

believe, to Roger to go through.

**MR. BRODE:** So, I'll try to do this fast. I guess a number of concerns that have been raised about the ability of the model to predict impacts, ambient impacts, and its accuracy. You hear over and over again that the AERMOD is overly conservative. We acknowledge that in some respects how the model is applied has conservatism built into it in terms of modeling with maximum allowable emissions, for example. But the model itself is not designed to be conservative. It's designed to be unbiased. And even within OAQPS people are asking, well, can AERMOD even calculate 1-hour averages -- failing to understand that that's the basic time step in the model.

So, I'm just going to try to go through briefly a lot of this stuff you've probably seen before. AERMOD was very extensively evaluated before it was promulgated. A total of 17 databases for use which is far more than any other model had gone through before. It was in two phases. A developmental phase where you're actually changing the model as you evaluate it and then independent evaluations covered a range of scenarios. It looked at short term intensive field studies. Long term studies from operating plants and so on.

They're very different. The one on the left, Prairie Grass that's sort of intensive field Tracer study where you had a large number of receptors oriented in arcs like that. So, removing the uncertainty due to wind direction which can be important. Here's the Lovett power plant example. Operating a power plant and far fewer monitors, but located on a critical impact area - a hill nearby.

A number of methods were used in that process. We were comparing AERMOD performance to the then preferred models that AERMOD would replace. For example, for ISC, for non-downwash, non-complex drain cases CTDM Plus was a preferred model for complex drain. And ISC-PRIME came along sort of in the middle of that process for downwashes. So, overall, AERMOD, you know, did pretty well against the models that it was sort of competing against or replacing.

So, I'm just going to go through these. QQ Plots, a number of people raised concerns. Well, a QQ Plot, you know, there's a lot more to it, but at least it gives you some sort of a quick visual understanding of how well the model works and I think what we're interested in in the model, especially for these hourly standards, is how well is the model going to predict the peak of the concentration distribution

because that's what's going to be compared to the NAAQS. We don't necessarily care did it get this value right for that hour at that receptor. We'd like it to but, you know, the level of skill involved to do that is much greater than we're expecting.

So, this is complex drain -- Lovett. AERMOD is the red curve so that line that it's almost right on is the one-to-one line. So, the unpaired distribution observed in AERMOD agreed very well. And also, to put it in perspective with these new standards, the model that it was replacing was CTDM Plus which was about a factor of two higher in that case and it actually required very robust, site specific MET monitoring in order just to run CTDM Plus. We haven't imposed that high level of standard in terms of collecting site specific data to apply AERMOD even in complex terrain situations. So, we've made a significant step forward, I think, in terms of the ability of the modeling to handle these challenges. ISC was about a factor of ten higher so, yes, it was clearly conservative for that, but it was also not a refined model for complex terrain.

This is another complex terrain. Again, AERMOD did quite well against the other models. This is a downwash case in Alaska, where a prime downwash

was involved so we had AERMOD versus ISC-PRIME and then ISC before PRIME. And even with PRIME in there, AERMOD actually did better than ISC-PRIME and performed very well.

Another downwash case and, let's see, there's an urban case, tall stack in Indianapolis, comparing AERMOD versus ISC. ISC has some biased over-predict it looks like, but AERMOD is pretty much unbiased.

One thing to point out is that there's some caveats here. These performance evaluations typically involved some fairly robust site specific MET data that was collected as part of the field study. You typically have hourly actual emissions or at least pretty good estimates of the emissions in order to remove as much uncertainty or bias associated with those key model inputs as possible.

So, you know, we're not -- this doesn't necessarily translate into the, sort of, practical world where I'm modeling my maximum allowable emissions. Maybe I'm using the nearest representative airport. It's representative enough, but it's certainly not necessarily going to achieve comparable results as you see here for those various reasons.

And also, you know, even -- well, I've

seen this case yesterday where we had one monitor, but even in the long-term field studies, there was typically about eight or ten or maybe more monitors that were being compared to, not a single monitor, to remove some of the uncertainty associated with the wind. The direction's a little bit off and then you either miss the monitor or you don't and that can result in significant differences.

So, just to caution that comparing PSD permit modeling results to observed concentrations at a single monitor or some monitor nearby is subject to possible misinterpretation and not necessarily a good indicator of the performance of the model. I mean, it may be useful information. We're interested in seeing that. We shared an example yesterday for the Portland Plant where there was one monitor downwind of the source. It was actually reasonably well sited. In that case, it actually did match up reasonably well, but if we looked hour by hour, it certainly wouldn't have looked as good.

So, now I'm going to talk about NO2. So, that kind of was a general evaluation of AERMOD dispersion. No chemistry involved. NO2 adds a new dimension. I do think, you know, we need to acknowledge you said that not many people needed the

tier three options in the past. At least maybe we were thankful that at least Region 10 did. And the State of Alaska because I think we're very fortunate that they basically sponsored the implementation of the OLM and PVMRM options within AERMOD several years ago due to that. And I think we're in better shape now that they did that than if they hadn't. So, I just want to acknowledge, Herman Wong and the State of Alaska for those efforts.

So, a lot of this has been documented in the documents that are referenced on SCRAM and in, you know, one of the clarification memos. So, this was probably the evaluation that was done for ISC, PVMRM, and basically these are power plant plumes showing -- comparing the ratios, you know, with distance from the source and overall, it's not perfect, but it actually for a convective case, it picks up the fact that it converts to NO2 pretty quickly. In stable cases, it recognizes that there's not much conversion because there's a very narrow, small plume. Not much entrainment of ozone.

We only had two longer term field studies and I think that that's still the case that we've looked at for NO2 to evaluate these options. One in Hawaii -- Palau, Hawaii where there's one monitor.

And then New Mexico. Empire Abo, where there's two monitors. And this just sort of summarizes the ratio of the robust highest concentrations and the average ratio of predicted observed for AERMOD with PVMRM -- PVMRM is about 1.5. There's some bias to over-predict it looks like, mostly for that case. OLM with OLMGROUP ALL actually does much better in terms of the average bias. OLM without OLMGROUP ALL certainly shows over-prediction as well as full conversion. We expect that.

And that is, just again, to make the point that we have not stated anywhere that PVMRM is a better algorithm or approach than OLM in any given situation. We don't know. I mean it may depend on the circumstances. There are aspects of PVMRM that are more refined. That might make it more appropriate in some cases than others but, you know, OLM certainly does much better in a few cases here. So, we hope to learn more about that and provide better guidance. We've provided some ideas. Some indicated some cases where PVMRM may not be as appropriate for low level sources, for example.

These are -- but that was, yeah, that was 1-hour. The previous evaluations that have been done for this were just focused on the annual standards, so this is what we updated more recently.

These are the QQ plots. That again shows the OLMGROUP ALL working pretty good. You know, it's not perfect, but it matches pretty well with observations. PVMRM at least, kind of near, but not quite as well, but certainly highlights the conservatism of the full conversion and OLM without OLMGROUP ALL.

That was the one monitor at New Mexico. This is another monitor where AERMOD does show with PVMRM more tendency to over-predict. That was highlighted on the table whereas OLMGROUP ALL was looking better. Palau is where AERMOD PVMRM actually does pretty well in matching. Again, these are the NO2 concentrations -- hourly NO2 concentrations. So, OLMGROUP ALL certainly much better than the other cases.

We had AERMOD -- a question came up about mobile sources and can the model -- how much confidence do we have in the model to predict concentrations for mobile sources. AERMOD was actually applied as part of the risk and exposure assessment for the Atlanta area as part of the most recent NO2 NAAQS review that resulted in the new hourly standard. And it was certainly focused on hourly impacts. A majority of the impacts were attributable to mobile sources and there's a longer story there which I'll kind of skip

over. But when it came to us, that's the kind of monitor to monitor comparisons we were seeing that AERMOD was, you know, grossly over-predicting the monitors. There were a number of factors there -- the MET data. They were using OLM. The roadways as area sources with OLM but without OLMGROUP ALL, so that was where we actually looked at that and realized that OLMGROUP ALL may actually be a pretty good idea.

But there are other issues, so that was kind of the before slide. This is actually a time series of NO2 concentration model versus monitored at a particular monitor for, I think, about a month. This is sort of after. So, much better agreement. It's not perfect, but in this case there's actually quite a bit of under-prediction of the observed. Well, that's because the wind is calm, so there actually are high concentrations under light wind conditions. In this case, the model grossly under-predicted those concentrations because it thought the wind was calm and so missing.

But given the level of detail and uncertainties in the emissions for the mobile sources, I think that's pretty good agreement. This is another monitor -- sort of a very similar picture there. So, that's -- and this is just overall looking at the

ranked distribution. One of the things we realized is that the emissions input to the model were defined by season and hour of day, but did not account for any kind of day of week component. And that showed up clearly here. If you compare during the weekday, the model actually agreed much better with predictions than on the weekends. So, some of that over-prediction actually was due -- occurred on the weekends. But more recently, we've had some interaction I want to sort of preempt later talks, but maybe feed into later talks on the NOx options and NO2 options in AERMOD, but sort of revisited this and had a range of conversation.

At New Mexico, there were two monitors. Kind of one north and one south that had NO2, NOx, and so forth, and ozone. And the original modeling evaluation had been done before we got involved. Just paired the north monitor -- ozone monitor and evaluated the north NO2 monitor which may be not the best idea. So, we looked at reversing them. So, we used the south monitor for ozone to predict impacts at the north monitor and actually did maybe improve the results a little bit in some cases.

But we also -- some concerns were raised. You'll hear more about it later. And wish we could have coordinated the timing on this a little bit

better, but about -- one aspect of PVMRM in AERMOD in terms of using relative dispersion coefficients may tend to over-estimate the volume of the plume under stable conditions because the relative dispersion coefficients really aren't applicable. The way they were formulated weren't necessarily applicable to stable conditions.

So, I actually started looking at what if we implemented PVMRM in AERMOD like it was for ISC originally by Pat Hanrahan using just total dispersion, but using a smaller portion of the plume predicted by total dispersion. And so I've done some tests with this which actually are kind of encouraging. And, yeah, so this is the new PVMRM results for the south monitor. Again, there was some sensitivity on whether you used the same monitor for the ozone or the other monitor. But it actually performs much better than with the current implementation. And this is the other monitor, and I can't even see what I'm looking at, of the same sort of thing. I don't have time to go through details because I want to leave time.

And then the other comment had been made is there's some, you know, disagreement on whether the way it's implemented now should actually use four sigmas to define the volume of the plume versus what

Pat Hanrahan has used with total dispersion which was 1.28. And this is just showing the difference. If I run the model as it's currently designed for one of the monitors, that's what PVMRM gives. And this is what it would give if you used the 1.282 to define the plume volume. It actually agrees much better in that case. But if you go to the other monitor, again, that's not perfect, but at least you're in the ballpark for the peak values. Now, you're seeing -- introducing some under-prediction by reducing the volume of the plume.

And for Palau where we actually did pretty good with the current implementation. If we just change the sigma -- the number of sigma Zs from what's in the model now, four, which gives you pretty good agreement, to the 1.282, clearly a biased under-predict, which is kind of what we would expect. And this is total -- using total dispersion, again, as a something to investigate for Palau and it worked pretty well before that and it still works pretty good. So, at least it didn't compromise the performance there.

Just to put it in perspective, finally, I thought I would throw in for Palau, where AERMOD with PVMRM does pretty well. ISC PVMRM actually gave you that. So, you know, same chemistry, same approach, except for the total. I think this is actually both

using total dispersion and the same number of sigmas, so the only difference is the dispersion model. And it's interesting to see how much improvement we get using AERMOD versus ISC with the same chemistry option. So, that was kind of an interesting point.

And that's where I'll stop.

**MR. BRIDGERS:** After a very on schedule day yesterday, we have slipped. And so, in the interest of time, I think what we'll do is we'll go ahead now and let's break for lunch and then when we come back from lunch, we'll pick back up on the schedule that we were going to keep. But to be fair to Erik and James, I don't want them to try to launch into a 45-minute presentation that a lot of people have come to see in less than 20 minutes. So, let's break for lunch and that means be back at 12:40 and we'll kick it back off.

So, thank you and see you after lunch.

(WHEREUPON, a lunch break was taken.)

**MR. BRIDGERS:** Okay, if we could take our seats. I see a lot already have. They're watching the clock better than I am. We're already four minutes over the schedule I had set.

So, as we start the afternoon session, obviously we've still got leftovers from the morning

session. Just a little bit of logistics and making sure that we can stay somewhat on time with the afternoon session and still get our question and answer sessions in because that's an important aspect of this conference is the dialogue between all of us.

What we're going to do, as I had mentioned before lunch, is we're going to have the AIWG, the AERMOD Implementation Workgroup talk by Erik Snyder and James Thurman now, but the talk that was originally schedule for one to one-fifteen that Roger Brode was going to deliver, we're going to scratch that from the agenda.

But that being said, a lot of the information that is included in that presentation can be found on the SCRAM website at the 10th Modeling Conference under the TSD for the New Jersey 126 and Roger and I are in conversations that we most likely will still go ahead and post the presentation under the conference presentations on the SCRAM website. I don't know that we would actually formally submit it to the docket, but nonetheless, it will be in there and there is additional information in the TSD.

Just in the interests of time, we want to hear from all of our invited speakers, so without further ado, let me turn the agenda or the podium over

to, I guess, James Thurman and Erik Snyder.

**MR. SNYDER:** Hopefully, everybody's not too full and I'll try to go through the 200 slides pretty quickly. I think we've got 50 or thereabouts.

I'm at Region 6 in Dallas and we formed the AIWG group to focus on NO2 and SO2 modeling. Anyway, we formed this last February, I think it was, or so. And we did some work last year and some follow-up work this year. Doing some cumulative.

Mainly, we formed the workgroup initially with these new standards and at the Region 6 we had a fair amount of experience already with some of the new standards and issues with modeling with some permits and so really, it's just focused on some one hour NO2 and SO2 as the workgroup and analyzing and trying out different things and trying to build up some community knowledge level in modeling.

It's comprised, I think, there's about 27 or 28 states that have members. I think we have six or seven regional offices and OAQPS involvement, so it's a good crowd of people.

The stage one of it was basically to -- we worked with our members that we had and we came up with specific facilities and then we kind of made them generic so that we wouldn't be modeling a specific

source so to speak. But we based these generic facilities on real facilities and just modified stack parameters and locations somewhat, but well within the tolerances of what we would see.

We completed this with all the single facility modeling. It was a non-cumulative last June at the regional state local workshop in Atlanta. And then we continued the AIWG process and we did some source grouping analysis, individual source culpability, and some cumulative analysis as well. We still have some additional work we'll be working on on these issues and then other issues as far as adding background and some of the other things as we work on the NO2 modeling and the future SO2 as well.

I'll kind of give an overview. We started with 12 typical industrial facilities that required the modeling and then we kind of expanded based on the workgroup. We had four more that we added to it and so overall, I mean, we worked to review it and, like I said, because of the concerns, especially with SO2 modeling that nobody wanted to have SO2 modeling at their own facility. We figured ahead of time before the actual modeling for the maintenance SIPS or whatever.

So, we did take these generic scenarios

to try to give that and it also had some generic property boundaries in lot of cases. We created base scenarios and then we also did a combination of stack heights and emission control combinations to see, when we modeled exceedances, what in the base level if we could fix it with additional, feasible things that you could do.

The way we kind of divided up the work in the regions and the states was, basically, the people that worked with that industry a lot did the work and the modeling so they were most familiar with it as well. And, of course, we did the normal five years met data and building downwash when we had the downwash data.

The caveats on the initial modeling we did and even on this modeling -- it's not in cumulative, I mean, even for the cumulative runs that we've done, we only put some sources in there. It's not the full level of what it might within the modeling domain of a 10, 20, 30 kilometers whatever you were looking at for your area of concern.

We didn't include the background monitoring values because that's going to vary a lot over -- so we know that there's also background monitoring to add in on these things.

As again, this was done not necessarily for one specific task other than to really get some experience doing this in the states and initially these standards were really being talked about in really difficult to attain and stuff and so we thought we'd form this group to really get some basic knowledge of is it plausible to work it out and show attainment or not.

I guess I'll turn it over to James for this part here.

**MR. THURMAN:** This is just a summary table of NO2 results. I'm not going to go very more in detail. This is also in the draft summary that we put out on SCRAM last week and that's going to be updated in the next couple of weeks as we finish adding maps.

Basically, what we have here is each facility we modeled base emissions or uncontrolled emissions the sales in yellow are where we had violations and it gives the maximum design value in micrograms and ppb. And also a percent of the receptors in the grid that violate. The ones in green are those where we passed -- didn't have any problems with the NAAQS.

So, these are all the NO2 facilities like the steel mill, for example, in the base case

there were violations of OLM and PVMRM, but when you go to a controlled scenario, the OLM case passes, but the PVMRM still had exceedances with one receptor. So, I mean that shows some sensitivity OLM or PVMRM.

Again, these are just summaries of NO2 and continuing with the summaries of NO2 on the refineries are actually very borderline. It could be depending upon how you convert from micrograms ppb.

So, you can look at this on your own in the report or the presentation was posted on SCRAM.

SO2, same thing. We had some similar facilities. One thing I'd like to point out about the ethanol plant is initially you see a violation of 296 and then control strategy is still 296. It wasn't the stack that you would -- the main-stack wasn't the problem, but actually what the modelers also did it. This is with a 50 meter fence line. They actually modeled with a 300 meter fence line and that receptor was inside the facility property so then you didn't have to have an exceedance. So this was a good case of showing how the distance to ambient air is important.

And then we have some more scenarios down more in the table and a few more. And like I said, more details can be found in this draft summary. We've got all the inputs in the summary right now. Bar

charts for all the facilities and maps for all the facilities that had exceedances and next week, I'll fill in the rest of the facilities. I thought we'd put in maps even for those that didn't have exceedances just so you could see the spatial profiles.

So, we're just going to go through a couple single source examples. If you were at the workshop last year, you'll remember we did all of them, but we don't have that much time today so we're just going to do a couple.

And I also want to acknowledge we have a few people in here that are a part of AIWG. I just want to thank them for all their efforts. It's been a lot of work. They had, you know, busy schedules, but they still took time out to help us with this so we really appreciate it and they did all the work here.

The first example for NO2 is the ethanol plant, actually. This was Dawn Froning from Missouri and Jen Krzak from Iowa. They were the modelers. So, we actually had states working together.

They evaluated four scenarios. These four scenarios with four different meteorological data states. All I'm going to cover are results for one today. But the four scenarios were our base case, you know, starting out with 1100 tons. Then, scenarios two

and three changed stack heights to 65 meters for this one point C004 and also for scenario three we added -- they added controls to get the emissions down to 381 tons. And in scenario four they added more controls to get down to 172 and, just to let you know, volume source remained unchanged in all of these.

They also had descriptions of what these sources are. C01 and two are flares. The third one is an emergency fire pump. And the fourth one is the source you would think of in an ethanol plant, the regenerative thermal oxidizer. I don't what that does, but it sounds important. That's probably a fancy word for a moonshine still, I don't know.

So, these are the emissions in grams per second down here. We highlighted the one source that we changed in yellow. So, we start out with a 32 grams per second, 43 meters stack height. Then, we changed stack height up to 65 meters and reduced emissions down to nine grams per second and then three grams per second.

They also modeled the, like I said, distance to ambient air. They modeled a 300 meter versus 50 meter fence line and then they also modeled stack ratios. First, they did .1 for all sources and then one -- .05 for the main source. They were looking

at C4 and then .1 for all others. And then a .25 for all sources.

And this is just an outline of the facility, how the 50 meter versus 300 meter fence lines and the spatial relationship of all the sources. Here's that main source that will be controlled. The red line is 300 meters. The blue line is 50 meters fence line.

These are bar charts of the maximum design value for each scenario in micrograms per meter cubed. Solid black line is the 188, 189 microgram level which is the NAAQS. These are all scenarios based on increasing the stack height and then with controls and more controls. And then different color bars are those NO2 stack ratio sensitivities and fence line.

Now, one thing you'll see is you don't see a lot of difference between the different scenarios when you control that one stack. And actually you don't really see any difference in when you change the NO2 stack ratio for that one source, the red and blue bars and the green and -- for the 300 meter you don't see a lot of difference. For the 50 meter fence line, you do see when you change that ratio for that one source, you get a difference because it's outside the

facility so its impacts are probably, you can see them better.

So, there's some spatial plots. This is where we're going to show the base case. The first one is for the .1 ratio at 300 meter fence line and the star represents the maximum design value. It's right on the fence line. You see the exceedances are basically kind of the orangy colors.

Or is this SO2? Okay. This is NO2, but I think I used a different -- it should have been stopped at 188. It should have had a break there, but anyway, the kind of orangy colors are the violations. 50 meter fence line, you get a max of 810. Still on the property line. And then changing the stack ratios for 300 meter fence line, you don't see any difference for max values. For 50 meters it goes up to 930 micrograms, but the overall spatial doesn't change a whole lot.

At .25, obviously you're changing that ratio a lot. It goes up to 1,000. And then .5, the default that's talked about in the NO2 guidance gives you a lot higher.

So, we had some findings initially. Our results are sensitive to distance and ambient air, and the stack ratios. The maximum design values did not

change based on changing that stack height or applying controls to that stack of interest. And we actually went back and did a few reruns to get a source contribution and it looks like the maximum design values were driven by the emergency fire pump. Now, we modeled that at continuous emissions so there may be -- in the permit you may want to take permit limits or something to help with that.

This shows is that problematic emissions may not always due to the stacks thought to be causing the problem. You know, you think I'm going to control that big stack and we find, well, not necessarily.

The next one we're going to talk about is for SO2. This is a coal-fired EGU. I did some runs myself and Erik Milligan from Oklahoma did some runs. I modeled with Charleston, South Carolina met data and he modeled with Springfield, Missouri met data.

We modeled six scenarios of a baseline, increasing stack height and controls. A combination of stack height increasing controls and you can see how the emissions change. More stack height increases and then another stack height increase with controls and a higher exit velocity and the actual inputs we'll show in a second.

And also, I did a run for two of the

stacks where we modeled a 65 meter stack height. Their original stack heights are 150 meters. So we thought what if you had the model at 65 for some reason. Also, I modeled with uncontrolled emissions at 10,000 tons. I believe this was based on a proposed EGU with controls already in place, so it looked like it was already fairly well controlled maybe.

These are the parameters. C1 and C2 are the two emission points we'll be adding stack heights and controls to. C1 is a 780 megawatt boiler. C2 is an auxiliary boiler. C3 is a diesel generator and four and five are fire pumps.

The uncontrolled emissions I modeled with for C1 are 290 grams per second. Controlled, I modeled 57 grams per second. Oklahoma modeled 112 grams per second.

Now, these are the bar charts broken out. Springfield -- so these are the Oklahoma runs here. These are the runs that I did here. As you can see, this red bar and this blue bar -- this white blue bar are the uncontrolled emissions and the base emissions using a 65 meter stack height. When their initial stack heights are 150 and 100, so I modeled them at 65 each. So you can see that makes a huge difference in the concentrations. We did model with

downwash. But except for those two cases, there's no NAAQS exceedances. So, if you were to model as-is, they would have passed.

Just some spatial plots for the uncontrolled case with the base parameters, but at 65 meter stack height, we get a 905 microgram. Right off the facility property, less than 500 meters. If we model with the base emissions and a 65 meter stack height, we go down to 445 micrograms and you still have a maximum design value of just off the property. But then once you raise the stack height to, you know, original stack height, it goes down to 65 micrograms and the max is actually farther out, but you have no exceedances. And eventually -- this is with uncontrolled emissions and this is with the base emissions and base stack parameters. Nice green background. So 33 micrograms.

So, our findings from the EGU are we didn't have any NAAQS exceedances unless we changed stack heights to 65 meters. For the base case and the controlled cases where we didn't change stack height, the maximum design value was driven by the diesel generator.

For the uncontrolled cases and for all the 65 meter stack height cases base or uncontrolled,

the maximum design value was driven by that big boiler which makes sense.

The Springfield case that Oklahoma ran seemed to be more sensitive to changes in-stack height than the Charleston case and that could be a combination of the meteorology and terrain.

These results may not be indicative of all EGUs, especially older ones that may not be as well controlled or have different stack combinations and terrain combinations.

So, those are our single source. We have three cumulative scenarios that I'll talk about. I'll talk about the first two for NO2 and SO2 and then Erik will come in with the natural gas compressors.

The first one is an ethanol plant, fuel or asphalt plant based on AIWG facilities we ran. So, we put them together. It didn't have any NAAQS exceedances.

The second one was an NO2. This was from a PSD scenario in Minnesota. They told me it was okay to use it. One receptor exceeded the NAAQS for NO2, but was located on facility property. And the design value was driven by that facility, so -- and it was not the facility of interest. And then, like I said, Erik will talk about natural gas compressors.

The first one, the ethanol plant, the turbine and asphalt plant, the receptor grid is centered over the ethanol plant. Here's the fuel turbine and the asphalt plant is out here. I can't remember the exact distances we put these at, so this is probably about -- this should be about 5 kilometers. I think this might be 15 from here to here.

Here are the results. No violations. The maximum is somewhere around the ethanol plant. What's interesting here is that here's a fuel turbine. When we ran this in single source mode for the initial AIWG stuff, Hadar from Tennessee modeled it and he modeled in complex terrain and he had violations. You can see those in the summary report, but they were less -- they were maybe about 500 to a kilometer away and you could tell it was driving by terrain. This is relatively flat terrain and the fuel turbine doesn't really cause any issues here. It's kind of an interesting thing to see that the terrain for the fuel turbine did have an effect, but here there's really no problem.

This is the one from Minnesota. This is the facility they were modeling for. Their facility of interest. You can see, you know, the dense network of receptors. You know, the fence line receptors around

the different facilities. These arcs down here are part of a polar grid, but then you have a Cartesian grid.

Now, the maximum design value is 189 and it was somewhere in here, but it was driven by this facility. So, technically, you wouldn't consider this facility's effect on itself, so I don't think there was a problem there.

I'm going to let Erik take over now and talk about natural gas compressors.

**MR. SNYDER:** Okay. This is, we presented this material last summer, but it's one of the first cumulative runs and I think it's kind of pertinent because there's a lot of oil, gas, shale clay stuff going on. That was one of the reasons we looked into this originally.

We did a scenario that's basically four compressor stations, assimilating in the DFW area. In the non-attainment area, we've got some rules on NOx, but Texas has put them in most of East Texas for their engines -- put these rules in place to control NOx. And so even outside the non-attainment area to help with the ozone levels.

But we've had a large shale clay. One of the first ones in the country was in the Dallas-Fort

Worth area west of Fort Worth and into Fort Worth and Arlington area. So, we're pretty familiar with this type of set-up.

We had facilities -- we had four facilities. We had scenario one that was a baseline. We had a small compressor at 28 tons per year. Another facility was 230 tons per year. 165 tons per year. And 135 tons per year. And these were in -- I'll show a map in a minute.

In the second scenario we raised, on the engines, we raised the height up to about 17 meters which, from the research we had done and the experience, that was kind of the upper limit of maybe stacks without too much back pressure on the engines.

In the third scenario was going to, would incorporate a 35 meters stack height on the engines from a standpoint of you'd actually have to do some induced fan probably to do that.

In scenario four was to look at, okay, what if you had one of these facilities that was maybe built in the 80s or something. There are a number of facilities close together like this that was built in the 80s and maybe they had an air field ratio controls was all they really had on the unit. And so you might have a 6 gram per horsepower hour type emission rate.

We modeled all four facilities. We looked at 100 percent, 80 percent conversion. So, tier one, tier two, looked at OLM and PVMRM and looked at different in-stack ratios of .1, .25, .5 and used a .9 equilibrium ratio.

We also did model a lesser controlled facility just using the same thing other than we didn't do the OLM on it, I guess. And short stacks on that unit. So, it's basically any existing facility you might come across initially. This was done with a couple other modelers; Chu Phong and Ashley Moore at Region 6.

This is a bar graph and, again, this scenario one is the -- this first scenario here and I do note the scale and nobody's jaw dropped, at least not too far anyway. But this is in micrograms per meter cubed, so this is definitely showing a problem here, especially this last scenario, this SC5, is for maybe one of the existing 80s type facilities that you've run into. Or 70s or something like that.

Okay, looking at this from the standpoint of existing facilities that you might run into and if they have to do a project or another facility comes in nearby and constructs and they show we got this other facility that's doing this type or a

combination of facilities are generating this.

Can we solve this problem is one of those open questions. So, that's kind of the construct of this scenario set-up.

So, we have the baseline scenario and we're still up over 1,000 micrograms with a lot of these scenarios and, just left to right, the blue is 100 percent so that's full conversion tier one. Tier two, 80 percent ARM is red. And the yellowish, dirty yellow, is using the .1 PVMRM. .25 is next. .5 and then OLM .1, .25, and .5.

And so, but you can see that as we go, even SC2 gets us, I mean, this black line is the NAAQS down here. It gets us below the NAAQS even just increasing the stack heights without doing anything else on controls if you have a well-controlled facility already.

As far as if you back off on the controls, it does cause a problem, even if you have the increased stack height. This is scenario four.

This is just to bring a scale now. I pulled SC5 out so the historical non-controlled facility or not very well-controlled facility, low stacks, is out of the picture. So, this gives a little bit more context. Okay, with baseline, 100 percent,

you know, you run the AERMOD, get the outputs and don't do any slicing of it at all. 1,800 plus micrograms. Using 80 percent, we drop it some. Then looking at PVMRMs, we had three cases, and it's still up over 1,000 for the baseline. OLM in this case was quite a bit lower.

Roger, you spoke earlier about the differences between OLM and PVMRM and the way they react with low level facilities and so, in some cases, PVMRM might not be the best scenario to go at. It should be -- consider looking at both. We haven't really developed an opinion on which one is the best under each situation. So, we're open to looking at both of them.

**MR. BRODE:** The best one is the one that gives you the best answer.

**MR. SNYDER:** Yes, whatever the best answer is. That may be user-defined.

But, again, the increased stack height, the only one that -- 100 percent was a little bit above here, but looking at this, even going to any of the three tier threes we had here you show, and this is just PVMRM runs on the stack height one with 17 meters increases, all these are showing fine as far as you can add the background monitoring and still be in

compliance or most likely. And again, if you add, it shows you can get it done further on the impact levels.

Here's some spatial plots. Okay, this is the 100 percent conversion so straight out of the box for scenario one baseline. No stack height increases and the 1838. And, as you would expect, the concentrations are really targeted right around the facilities. And this is -- kind of give you the spatial plot of the facilities. We had one here. We had another one right here on the edge. That was the small one. Another here and then another one down here. And these distances are a couple of kilometers -- zero to two kilometers, so the spacing on these facilities is a little over two kilometers here in direction. So, fairly close knit package of sources.

So, we see a fair amount of sources like this with the shale clay where you get a lot of these units fairly close potentially. So, it may be conservative compared to what you run into in a lot of cases, but it may be realistic in some cases. I mean, we know it's realistic in some cases from the stuff we've seen in Texas.

This is the cumulative using the PVMRM, so again and I didn't point out on this, but even the scale over here on the right, really, once in the

yellows, you have to get up in the mid-yellows to be above the exceedance level, almost into the oranges. Really, it drops off really quick around these facilities. If you had a lot larger footprint on the facility than what we modeled, again, the distance to ambient air, you may not have a problem at all.

And this is doing a stage three PVMRM .1. In the analysis and the data sets that we've collected, I mean, .1 may be on the low side for natural gas units. We've seen them lower than .1 and we've seen them higher. .2 levels. .25 on some of the data that I've gotten through the region and some protocols and stuff. So, but again, I mean getting in-stack ratios, doing that test, to me, is not difficult if you've got existing facilities to do that and then use that data to help drive the modeling. Again, the max is still over 1,000, but your non-attainment zone where your actual modeling exceedance is really small.

This is the stage two or scenario two, excuse me. So this is with increasing the stack height to 17 meters and 100 percent conversion. So this is, you know, you almost get there with doing 100 percent conversion, not even doing a tier two, and I don't have that plot, but you would get there depending upon what your background is. But if your background NOx levels

are high enough, you might have to go to tier three, but it looks doable on these short stack, fairly, I mean, these facilities -- if they're fairly well-controlled, it seems they can pass in a lot of cases.

This is, again, this was increases in-stacks. 17, less controls and, of course, the spatial plot grows a little bit, but still you have everything is in attainment with the 17 meters and using PVMRM, even with the less controlled level than the range of emissions rates we had. I think we backed that one off, though. I think it was in the two to three grams per horsepower hour emissions rate. I'd have to double-check on that exact number.

And then this is the cumulative 100 percent ratio with the higher stacks, so you can, with dispersion and raising the stacks on these units, it's not a huge tonnage of emissions so it drops it off real quick.

And this is the scenario four. This is the stacks with less controls and 35 meters stacks. So, raising stacks along may not solve the situation. From the analysis on this type of industry anyway, it looked like controls were the first thing, but if you had a lot of sources around, but you just have to model and see what works best for the facility.

And just for grins, this is what the facility looks like without, you know, a 1980s vintage facility and it does show a huge area of non-attainment, but the non-attainment levels are basically the dark yellows and you can see the contour line here of the standard and a distance of six kilometers, six and eight kilometers on the bottom.

So, you know, we know there's going to be situations where some of these might model this way with PVMRM, but the question is can they be solved or is it not a doable situation? In general, the NOx controls for this industry are fairly reasonable cost compared to some of the other point source controls.

Lessons learned, James, jump it at any time. Again, you need to evaluate both controls and stack heights within GEP. Evaluate for low stacks. Small property footprints are still the main problem and so it's not surprising. And I think out of this, again, the stacks that drive it aren't the starch dryers or the ethanol plant or the boilers. It was the emergency unit, but that's fixable with permit restrictions and stuff.

Another thing is this is one of those things that it really helps to spend some time getting the facility information together as far as the

emissions rates and any information on in-stack ratios, downwash, and getting your property lines. Make sure you've got good property definition for the facility as far as what's ambient air or the start point is.

We know a number of states have started working on collecting this as part of the AIWG process as well. And also for the SO2. People are pulling information together out of permits and stuff. Again, emergency units are solvable. And again, NAAQS exceedance is not just tied to emissions levels, but that is an important thing.

This last bullet, from the perspective of Region 6 and some of the feedback, I haven't heard from the region other than one facility that they were facing new controls to go forward with and they decided to pull the plug on a project. Other than that, I haven't heard of any SO2 projects being pulled. We have had successful demonstrations in the region and the state. It is a tight standard. We all realize that, but it seems that in our case it's workable. I guess we would support any information and if there's examples and work through those things. Work with the regions.

With NO2, again, not necessarily tied to the emissions levels or the predominant sources, but

again, in-stack ratios, getting it for other facilities may be problematic. I've run into that on one project already, but it's important to work with the other facilities and try to get that information and also do the research necessary.

The NO2 modeling is sensitive to the in-stack ratios and, of course, there's differences between the two tier three options and the background data has an impact as well.

I just emphasize the importance of this as we move forward as a group, a community. We really need to work together on pulling as much information on NO2 and in-stack ratio data for all types of facilities. Parameters and controls.

As far as AIWG, we're continuing the process and we'll be looking more on the cumulative and looking at some other scenarios and stuff as we move forward. Of course, the workgroup is also involved in local permitting, so it depends on their workloads as well. We look to loop in additional experiences in the RSL workshop coming up in another month, month and a half, but that's not that far away. So I'm not sure how much extra it will -- we may have some extra anyway.

I think we've got the draft report

created, but it's kind of rough right now, but I think we'll be working on that to really populate it further than what we've done so far in document what we've done so far and I guess that's kind of how I'll wrap it up.

I appreciate any input from people on scenarios when they have problems demonstrating. Work with the regions, work with us, and it will get to AIWG as well and we're interested in trying to help tackle those things and work forward.

**MR. BRIDGERS:** What I will say is, you know, James said that there would be some updates made as more maps and more charts are made for the AIWG report. I'm the one who actually uploads the stuff to SCRAM, so as that goes up, we'll make sure that under the recent addition, there will be some note that the report had been updated. Otherwise, it would not.

So, somewhat getting this train back on the right schedule, if there is a right schedule, we're going to shift gears a little bit and head into a couple of invited or a slew of invited talks and the first up is something that a lot of us have heard quite a bit about over the last year.

So Bob, if I can find your presentation here, we can --

**MR. PAINE:** Hopefully, you have it.

**MR. BRIDGERS:** -- yeah, it should be on here.

All yours.

**MR. PAINE:** Okay, this was first introduced last June at the regional workshop and we've made a lot of progress. Unfortunately, the version I have has fellow authors and I would like to acknowledge the contributions of AECOM authors and staff members Dave Heidle and Rich Hamel and EPRI members in the back of the room there. Eladio Nipping and Naresh Kumar. And, of course, they'll answer all your questions.

I'm going to talk about the guideline procedures that effect what emission rates you have to use in modeling and how we would deal with variable emission distributions. And a description of a procedure for a Monte Carlo type of approach, which we're calling EMVAP which stands for the Emissions Variability Processor.

We've got the code working and have evaluated three of the AERMOD evaluation databases with this procedure and I'm going to report those results. I'll talk a little bit about how the results are sensitive to the number of simulated years going from 50 to maybe 5,000 simulated years and conclusions.

This is a famous table in Appendix W which says for short term averages you have to model your maximum emission rate, design capacity, and assume continuous operation. And obviously, some sources don't operate at their maximum emissions limit continuously and that's definitely a problem.

Some sources may be able to accommodate more than one emission rate which they could assign a probability to and this procedure is designed to try to give a source credit for being able to do that. Some intermittent sources would be, for example, emergency backup engines, but sometimes bypass stacks which operate infrequently but have much higher emission rates during those operations. They present modeling challenges and so assuming a fixed peak one hour emission rater continuously will certainly result in unrealistic model results when compared to a monitor.

So, this approach would be to assume a prescribed distribution of emission rates and so this processor which I'm going to describe uses this information to develop alternative ways to come up with a compliance rather than just using one emission value.

Here's an example of a time surge over 8,760 hours and this is emission rate on the y-axis. You can see that the peak hourly emission rate is about

133 grams per second, but the average is more like about a third of that.

So, how do we use this information? Say this was a typical operation for a source. One way to do this is to put this into a cumulative frequency distribution and you can see that about a quarter of the time the source is off. And maybe two percent of the time it's pretty high and to model this, if a source wanted to permit this type of emission rate, they would have to say well, I'm going to model it at maybe 140 grams per second to be safe. Well, that's clearly going to overstate the emission rate for most of the time.

One way to do this in our emission variability processor is to come up with a few cases and to put boxes around those cases. As long as we can envelope this cumulative distribution with these sets of boxes and this is just an example. In this case we say well, we'll never operate more than 140 grams per second emission rate, but 98 percent of the time we'll be no more than 100 grams per second and 89 percent of the time no more than 65 grams per second and so on.

The average emission rate, the weight average, would be a third of the peak emission rate. In some cases I'm going to describe later, we can

divide this probability distribution in maybe five, ten, or even 20 divisions. Or another way is you can divide this range of emissions by emission rate into five, ten, or 20 divisions. Obviously, the more divisions you have, the closer you come to fitting this curve and therefore, the more closely you will come to actual emissions. And we're going to show how the procedure is sensitive to that type of set-up.

So, this approach would be to create an emissions frequency distribution I just showed you an example of. And then model the source with unit emissions for each case. In this past slide I had six different cases, but you could have up to 20 cases in the way the procedure is set up now.

Each case is modeled with unit emissions with its own exhaust parameters and even if you add a bypass stack that we even at a different location, you can model that because that's a separate AERMOD run.

And then you would create hundreds, thousands of simulated annual realizations of the concentrations distribution by basically rolling the dice, taking the probability, and then applying the emission rate that corresponds to the percentage that comes up.

Then we, basically, as a post-processor,

there is no change in AERMOD. We compile many, many simulated years of concentrations and then post-process these with a look alike to the AERMOD post-processor. We have basically replicated the AERMOD software to create the right design concentrations.

So, how do we do this random selection? Well, in one case -- in some cases, that is, peak emissions might occur in groups of hours, but since the form of the standards of the one hour NO2 and SO2 involve only the highest concentrations an hour in any given day, a group of hours in a day only count as one, you know, basically one day's maximum concentrations. So, it's conservative to basically spread these high concentrations out among as many days as possible. That makes the process simpler, but somewhat conservative, so that's what we do.

But we also have a procedure where if two different sources operate together, we can use the same sequence of random numbers to make high emissions occur in the same time and if they're not in tandem, we give them different initial random numbers.

So, the purpose and definition of this system is to do a probabilistic post-processor for a range of emission rates. We have three different modules in addition to, of course, AERMOD itself.

EMDIST is used to look at hourly emissions and decide how to set-up your cases for running AERMOD. Then you run AERMOD with unit emissions. Then you use the EMVAP probabilistic emissions simulator to take the probabilities you've set up, select an emission rate for each hour, apply it to your output concentration for each receptor for each hour of the year up to five years of normal AERMOD runs, and come up with output files which are then fed into the EMPOST post-processor which gives you the output design concentration predictions.

I'm not going to dwell much on EMDIST except that it can take into its -- input the, let's say, several years of hourly emissions data, come up with very useful statistics. You also want to do for each case a realistic hourly stack exit velocity emission and exhaust velocity.

Let's go to EMVAP. EMVAP is an interesting part of this whole procedure because it takes the number and lists of the years included in the analysis and the number of Monte Carlo simulated years to perform. Obviously, each run would use the same receptors. Each of the cases would be run with AERMOD with identical receptors, but the stacks can actually move. If you're using a bypass stack in some cases.

You then use a random, sort of a non-random random number file, but you can start with different starting points in that file to get sources that are uncorrelated. But sources that are linked can use the common sequences of random numbers for up to ten source data sets, one of which can be -- by the way, you can run AERMOD for a group of sources the traditional way. Combine them with running AERMOD with sources that have variable emissions and have a hybrid approach and even have a concentration file of just background. So, you can add in sources run the old fashioned way and sources run the EMVAP way.

Okay, EMPOST will then take the results of EMVAP and give you the required output. You'll want to know how many years you're using, file names, et cetera. The number of modeling iterations that were performed and it will give you the statistics to report.

Now, let's go into the evaluation of EMVAP. We developed a working code. We decided to, besides trying it out on prototype examples to see if our own staff could figure out how to run it, we actually tried it on three AERMOD databases that were previously evaluated with actual emissions.

But we ran AERMOD with both actual and

peak emissions for those data sets just to see how the evaluation might change if you were required to use what Appendix W says you have to do to compare a model to a monitor. And then we ran EMVAP to see if we got a more realistic result from running the peak emissions. And we ran it over 1,000 simulated years and we would expect that the EMVAP result would be more conservative that the actual emissions because we have this buffer. We're covering the cumulative distribution with a set of cases, but certainly less conservative than using peak allowable emissions.

We used the Lovett Generating Station. We've seen that mentioned before. Clifty Creek and Kincaid. So, these are all electric generating stations, all of which had hourly emissions very well documented, and different terrain settings from complex to rolling to flat.

Here's Lovett showing the stack here and the hill with the monitors. And this is the frequency distribution of those emissions. Pretty steep. And you would expect that EMVAP might do something about this because obviously modeling this emission rate all year has a lot of conservatism.

If you chose, for example, six cases. We chose five, ten, and 20 in different slices, but you

can see that the exit velocity as a function of emission rate will, as you might expect, go up as you get from minimum load to full load and that is reflected in the inputs to various discreet cases.

Now, I'm going to explain what this is all about. These are the results of the -- this is the design concentration and these are concentration bar charts here. These are the observed at the controlling monitor. This is the AERMOD with the actual emissions, so this is slightly under-predicting actually. AERMOD with the maximum emissions we're probably over-predicting by a factor of two. As you can imagine with this emission distribution, the average emission is about half the peak, so no surprise that the peak emission rate run through AERMOD would be something on the order of twice what you got for the actual emissions.

Now, here's EMVAP with five, ten, and 20 cases with the vertical slices that go along the probability x-axis and then five, ten, and 20 slices doing the emission rates, cutting the emission rates into various sections. As you can imagine, as you go into more and more cases, you get more toward the actual emission condition. In all cases, we are more conservative than the observed and more conservative

than modeling with actual emissions, but much less conservative than with maximum emissions. So, this procedure is giving us tremendous benefit in this case and performing as expected.

The next one, Clifty Creek, with six monitors in various directions between Kentucky and Ohio--

**AUDIENCE MEMBER:** Indiana.

**MR. PAINE:** Sorry. Thank you. My geography -- but it's close to Ohio.

Okay. We have here a fairly shallow drop off of emissions. So, you might expect in this case, EMVAP might be less helpful and will still see that it is, indeed, the case.

We again did five, ten, and 20 slices with vertical slices here and then five, ten, and 20 divisions of the emission rate from the peak emissions down here. And this is, by the way, there's three units so I'm just going to show you that they're all similar, sort of flat drop off of emission rates. And then we see, as expected, similar set up here observed design concentration over those six monitors.

AERMOD actually over-predicted somewhat with actual emissions and with peak emissions. Over-predicted some more and you can see that with EMVAP we

got the expected -- not a huge benefit because of the flat drop off, but with more and more slices we got a little bit lower over-prediction here and always higher than AERMOD with actual emissions. Lower with AERMOD with peak emissions.

The last case would be Kincaid. Flat terrain. 28 monitors. Lots to choose from here. In this case, sort of a peak, a few percentage pretty high up, so obviously if you modeled with this one you would maybe over-predict by a factor of two and, indeed, we are seeing that is the case. Actual versus maximum, a factor of two difference in just running AERMOD. Actual emissions actually under-predicted slightly.

We're seeing again a similar trend. EMVAP again always greater than AERMOD with actual emissions. So, we're getting expected and beneficial results with EMVAP.

Sensitivity analysis. You can get not only -- we're using the 50 percentile statistic out of EMVAP. That is, you take 1,000 simulations and you rank them. You take the 50th percentile, but you can take other percentiles and we decided to say okay, depending upon how many iterations I run EMVAP through, how fast does this solution converge? The 50th percentile convergence is pretty fast. After 500

iterations it's pretty flat. In fact, after 50 it's close to converging.

Obviously, as you go higher and higher you get more extremes and this keeps going up as you add more iterations because you can get more different selections of random numbers because each iteration starts with a different random number. Although, sources in tandem will start with the same random number. But you can see, even the 90th converges pretty fast and it turns out that for these evaluations we used the 50th and the 50th works out well.

Current limits in this code. There's no really effective limit to the number of receptors. We have tested it with 10,000 receptors and the computer hasn't exploded.

Source groups to be combined. You can combine ten different source groups currently. Some of those can be groups with constant emissions run the current way or background. Low cases per source group up to 20 with 5,000 simulated years up to five years of real modeling and the typical run time was surprisingly fast. You might think you have to have, well, it used to be a Cray but Crays are old fashioned. Let's say a Linux cluster, but you don't need a Linux cluster. You can do it on your laptop. A few minutes to an hour

maybe. Maybe the time it takes to run AERMOD or maybe five years of AERMOD. Not too bad.

And so, conclusions and status. Currently operational. EPRI is beta testing this.

Considering implementation approaches. We have found against field data that we like the results. As predicted, we are between actual emission results and especially for peaky types of emission distributions, we are much better using peak emissions. And you can imagine for sources that have very rare, but very high emissions, that could be a much bigger benefit, so if the source can see their way to accepting different emission limits for different probabilities, this may be a way to go. EPRI is still testing this procedure.

And I think that concludes my talk.

**MR. BRIDGERS:** Moving right along. That is much easier this time. I turn the floor over to Mark.

**MR. PODREZ:** Thank you.

Hello, I'm Mark Podrez from RTP Environmental Associates and I'd like to thank George and Tyler for making time today for this presentation which is on an update ambient ratio method or ARM for performing one hour NAAQS analyses.

In EPA's March 2011 guidance, they make the statement that given the stringency of the new one hour NO2 standard, many permit applicants may find it necessary to use the less conservative tier two or three approach for their analyses.

Usually, it's the PVMRM or OLM tier three methods that must be used because the current one hour ARM guidance is very conservative. I think as Tyler noted this morning that out of the 26 PSD permits that were issued that had to deal with perform analyses for the one hour NO2 NAAQS, 17 ended up having to use one of the tier three methods.

EPA has also stated that at this point there's no preference for any of these methodologies at this time. The test evaluations conducted to date have been somewhat limited and have shown that under different circumstances, one method or the other may indicate better performance or shall I say give the best answer.

Now, ARM was originally developed by Chu and Meyers in 1991 and in the report they noted that the plume mixing and near-field NOx chemistry processes can be accounted for through the empirical use of ambient monitoring data. They compile annual average NO2 concentration divided by total NOx concentration

ratios, the ambient ratios, from a large number of ambient monitors and they recommended looking at the 90th percentile value of 0.75 as a reasonable upper bound estimate for the annual ambient ratio. And in this ARM method, you simply take the modeled total NOx concentration and multiply it by the ambient ratio to determine the final NO2 concentration.

Now, in the more current one hour AMR guidance, the EPA has cited two more recent studies to support the current recommendation of a fixed ratio of .8 for one hour analyses. However, both of these studies as well as monitoring data evaluations demonstrate that the ratios are really variable as a function of time or distance from the emission source and the current fixed value method may be overly conservative, especially when your monitoring very near-field fence line concentrations.

One of the studies was the Wang Study of NOx near roadways for short-term monitoring tests. You'll see that the background concentrations for NOx and ozone were very low. Even the maximum measured NO2 impacts were really quite low, less than the ambient ozone concentration. So in this case, there is no ozone limiting occurring. In effect, this study is based on such low measured impacts, it may not be

indicative of the processes that may be occurring from higher impacts form point sources where the entrainment of ambient ozone into coherent plumes may be more important as compared to well-mixed roadway emission sources and where ambient ozone concentrations may limit the conversion.

Some of the study results. Certainly, some of the ambient ratios measured were variable and EPA, you know, focused on using the highest measured ambient ratio as a conservative fixed ratio for their one hour ARM recommendation.

I'd like to present a couple plots that show the variation of the measured ambient ratios. This is first as a function of the inverse of distance. So these are closer in. These are farther data points. Note that the closer in points generally have the lower ratio. Again, this is consistent with the simple conceptual mechanism of ozone being entrained and then subsequently oxidizing to NO2 which really dominates the near-field plume chemistry and as there is more time for dispersion, more time for entrainment, there is more conversion occurring.

You may ask why the inverse of distance is plotted here. Here is another plot where the ratio is plotted as a function of NOx, total NOx

concentration. You can see that these have very similar looking graphs and this really kind of illustrates how the NOx concentration can somewhat be thought of as maybe a bit of a surrogate for the amount of time, distance, dilution, reaction that has been occurring.

So, a variable ratio ARM method, calling it ARM-2, could be less conservative than the current fixed ratio, more conservative than refined tier three methods, it could fill gap, and if it's based on a large enough set of one hour ambient monitoring data, again it implicitly or empirically addresses a wide range of the processes occurring in the near-field.

What would the benefits be? Well, these are really the same benefits that the original ARM technique afforded. It's a simplified screening approach that's easy to implement either in spreadsheet templates or it could be coded rather easily into AERMOD or a post-processor. It does not require detailed in-stack ratio data. It does not require representative ozone data which also avoids having to look further and make decisions about issues of potential ozone scavenging in the data sets. It doesn't really introduce complex offsetting errors between complicated modules in the model. And, you

know, ultimately it would reduce both applicant time for preparing these analyses and agency for reviewing them.

So, we looked at a large data set of ambient one hour NOx data. We looked at the AQS database from the last decade of all NOx sites in the U.S. We looked at various subsets of that AQS database. We also looked at some of these data sets that have been used in tier three testing of PVMRM and OLM. The Empire Abo. The Wainwright data set, a new data set that Steve Hanna will talk about more in the next presentation. New Mexico Environmental Department had a big database. And then we got one from the Canadian Oil Sands monitoring network.

We have plotted all of these data sets by the observed ambient ratio as a function of NOx concentration. They all show very similar relationships. I'm going to go through them quickly in the interests of time.

Empire Abo and the Wainwright data sets, the New Mexico State data set, the Canadian Oil Sands data set, this is a network of six monitors. Numerous IC engines all around them. You will notice that the x-axis on here is larger. There are higher impacts being measured here. Anybody who is looking at this

little tail here, that tail is most likely an artifact of the NOx analyzer over-ranging on the NOx channel and therefore, that is not real world data. And then here are the plot for all rural and AQS monitoring stations.

So, in all these plots you see the same trend of lower ratios being observed at higher NOx concentrations. Certainly, there is a wide spread of ratios at the lower NOx concentrations. Well, that's because a low NOx concentration could indicate either a smaller nearby source that has less time for entrainment and conversion and therefore, a lower ratio. Or, a larger, more distant source that has more dilution and more time for entrainment and conversion.

So, we try to take some of these data plots and develop a variable ratio curve that could be used for ARM-2. Because of the large number of data points, we took the data and sorted them into bins. We tried to get a reasonable upper bound for each bin. We selected the 98th percentile not because it's related to the form or the standard, it's just a good indicator of upper level ratios in that bin. And this is what such a graph would look like. This is, again, that same kind of rural and suburban data. Each of the diamond points is the 98th percentile for that bin

fitted to a curve. You'll notice that the curve does start dropping off at the highest NOx levels. We did limit these curves then to .15, somewhat arbitrary, but a .15 ratio to kind of represent an in-stack ratio average and so that does agree well with what we end up seeing here at the tail end of the distribution.

These are some of the actual ratios then calculated both for the rural suburban data subset. Here's the urban city center subset. You'll see that they're really quite consistent. The urban city center are a little higher, but for example, at 300 ppb total NOx, the two ratios would be .21 and .23.

We also looked at various geographical subsets. Northeast, southeast, midwest, mountain states. Southwest. And with the exception of the mountain states which had a much lower number of data points, things are very consistent. I think for the mountain states, when you start getting to bins that have less than 100 or 50 data points, you're basically taking the highest observed ratio. But again, for example, 300 ppb total NOx, the ratios vary from about .2 to .23. This is all sites compiled together and that ratio would be .24.

So, we took this curve, we developed the curve for all data sites in the AQS database and we

wanted to do some performance testing in comparison to PVMRM and OLM using the available data sets. The Empire Abo North we looked at. Palaau and the New Wainwright data sets. Basically ran AERMOD to calculate the total NOx and applied the ARM to variable curve ratio. And we ran AERMOD with PVMRM and OLM.

One note used on our assumptions from Empire Abu North Site. As Roger was talking about today, we also wanted to address ozone scavenging at the North Site. It's not appropriate to use that ozone data because when you're having impacts, there's less ozone because it's being used to convert to NO2 and so that artificially lowers the ozone values. So, we used the higher of the north and south monitors. And we also used an in-stack ratio of 0.2 instead of the 0.1 used in EPA's original modeling.

You know, most of these sources at Empire Abo are IC engines and .2 is closer to the current typical guidance. You know, just those two little changes resulted in the highest PVMRM modeled NO2 concentrations being about 30 ppb higher. Almost 30 percent higher than the results originally presented by EPA in 2011. So, you know, again, obviously PVMRM is very sensitive to the ozone and in-stack ratio assumptions being used.

So, we plotted the performance in Q-Q plots for these different data sets. First Empire Abo, a monitoring station about 1.6 kilometers from the source. I averaged the ten highest monitored concentrations to give you a feel for the impacts. 375 for NOx. 91 for NO2. About half of the NAAQS.

This is the original reported performance by the EPA in their March 2011 memo and, again, you can see that both PVMRM and OLM Group are clustering pretty close to the one-one line at the high concentrations. OLM is a little low and PVMRM is a little high.

This is an updated one. And here, total NOx is blue. 100 percent conversion. The PVMRM is green. OLM Group ALL is yellow. And ARM two is red.

So, you can again see that with these updated assumptions that PVMRM and OLM now are about a factor of 1.5 higher, but you will notice that ARM-2 is performing roughly the same for this data set.

The Palaau data set. Here the monitoring site is only located 200 meters away from the source. You know, very little time for entrainment, mixing, and reaction to be taking place. This actually might be a better site for assessing in-stack ratios than for determining how well these

methods address the atmospheric conversion processes. We do see some very high monitored NOx impacts. But not very high NO2. You know, the average ratios for the high concentrations is only about .12.

Here's the Q-Q plot for this source. Again, in this case we see PVMRM, OLM, and ARM-2 all performing about the same. They're all over-predicting, whereas full conversion drastically over-predicts the highest concentrations.

The Wainwright data set is 500 meters from the source. Similar maximum concentrations as Palaau. Similar Q-Q plot.

So, I think the conclusions is that, well, a couple conclusions. First of all, the relative performance between PVMRM and OLM, you know, it can vary depending upon the data set.

Again, the tier three results can be very sensitive to ozone data and in-stack ratio and really any continued performance tests really should be using the same assumptions as the current guidance for permit modeling.

The ARM-2 method is more conservative at lower concentrations. It's assuming very high conversion because it's based on that upper bound of observed conversion at low concentrations. But at the

higher concentrations, it really performs comparable to the tier three methods.

Finally, we just wanted to do a little sensitivity testing to compare the methods. Again, we used the same data sets that were in the MACTEC PVMRM sensitivity analysis. We did not use the data sets that had downwash because we did not have building structures in the downwash parameters and, in addition, we did a couple of hypothetical source configurations.

Really, the conclusions are kind of similar depending upon the data set in one source or one method or the other might give you the best answer.

For the EPA original data sets, PVMRM generally predicts the lowest NO2 concentration, although them ARM-2 are similar, except for that last case. The gas turbine and complex terrain where here, ARM-2 is predicting the lowest concentration.

Here are some project examples. Again, there is variability which one of these methods ends up predicting the lowest NO2 concentration.

So, in summary, ARM-2 is a simple method that's easy to implement. It's straightforward to review. It is more conservative that PVMRM and OLM at low concentration ranges, but at the higher concentration ranges it performs comparably. It could

fill a gap in between the current, more conservative and not as useful one hour ARM and the refined tier three methods. And we believe that the ARM method for one hour modeling should be revised. I think, also, that this points out that additional data sets with higher NO2 impacts at or above the level of the NAAQS are still really needed along with concurrent source data, emission data, are needed to better evaluate these various conversion options.

So, I would just ask you to please provide your comments or suggestions as part of the record of these hearings.

Thank you very much.

**MR. BRIDGERS:** Outstanding job there, Mark.

Humming right along, I turn the floor over to Steve.

**MR. HANNA:** Okay, thank you.

The previous talk by Mark Podrez and this talk are both sponsored by the American Petroleum Institute and we've been working -- Mark and I have been working in tandem on this and we -- most of the work that I'll be describing here on this very specific evaluation of PVMRM and OLM was actually carried out by Liz Hendrick and Vin Tino of Epsilon and they were the

project managers actually. So, Bruce Egan and I were more of the science people and planning the evaluations and interpreting them. And I also wanted to note that we've been really pleased with the collaboration with the EPA OAQPS that has taken place over the course of this study. We've had, you know, several information exchanges. A couple of face-to-face meetings. And some of the technical suggestions we've made, Roger Brode tested with some of the runs that he reported earlier today. So, we think this is a good example of a collaborative exercise between industry and the agency.

Well, there's already been a lot of talk about PVMRM and OLM and Mark just talked about the ambient ratio method. This morning, Roger showed many examples of the evaluations and there's this tier one, tier two, and tier three and so on. And the PVMRM, you might say, is an intermediate it, I would consider it an intermediate approach. It's a very simplified chemistry approach that just looks at the amount of entrained ozone into the plume and then figures out how much NO2 is going to be produced as a result of that. And there are more detailed plume models available like SCICHEM and RPM that include a little more detailed chemistry. So, there are several categories that are

available.

And OLM, I guess you might say, is in between the ambient ratio method and PVMRM in terms of complexity. It does what it says it does. It limits the amount of ozone that can be mixed in. So, if there's not enough ozone to thoroughly react the available NO, then it recognizes that and doesn't proceed.

Well, what we were trying to do with this specific study is find a new data set. As we say from Mark's presentation and Roger's presentation, there is just a few databases. They only have one or two monitors. There's the power plant plume study in the Netherlands that's included.

So, we looked around first trying to identify a better data set and the API is more interested in low level sources and not so much in the power plant plume and the aircrafts flying through them. So, we ended up identifying a particular area in Alaska and it's, as you all in this room recognize, it's often hard to get people to give up their data because it's not too good in some cases to be identified here as a source and plastered across a screen. So, we were very fortunate to have these data.

It's what we call the Wainwright data

set, Alaska, and Mark already described most of this. There's a picture of the site. It's just a little town and the power plant is on the edge of town. One monitoring station 500 meters away and then there's a local ASOS met station. But the monitoring station has meteorological data that it's measuring also.

There's these five diesel fired generators and stacks. And the picture of the building is here, so you can count five stacks and there were logs, operating logs, of which unit was operating when. Then for the emissions, temperatures, and so on, we just used the manufacturers design criteria for those generators.

We tested a few versions of AERMOD and I should stress that OLM and PVMRM have one missions here; to calculate the ratio of NO2 to NOx. So, AERMOD is calculating the NOx concentration and then you just multiply by whatever OLM or PVMRM gets.

Roger reviewed this morning some of the scientific considerations that are being considered, like the relative dispersion parameters and whether they should be larger than the continuing dispersion parameters. The use of this N sub Z which is how wide you assume the plume to be when it entrains ozone. Some other things like downwash and whether the model,

the multiple plumes, how they're combined. If you have several plumes that combine, they are now, actually, have a smaller total size so there's less ozone entrained into the combine plumes than there would be if they stayed the same.

So, we did a few of these model runs. The downwash was done assuming, what you see here, there's the building itself on the upper with a peaked roof and then a couple of storage tanks and a shop building were included in the downwash considerations.

There's the local monitoring station that we used about a year's worth of data. Because of the land, air, surface data not being available, we just assumed it was desert shrub land in one direction and water in the other.

And in order to narrow things down a little more, we only considered hours in which the wind direction was in a 60 degree sector containing the monitor and we applied about boot software.

It was a little difficult to do an apples-to-apples comparison here and defining the thresholds and how the background was going to be added and the minimum value that we would consider. So that was all carefully worked out.

So here's the first example, just of the

prediction of NO2 to NOx ratio. And we thought we'd see what would happen with stable versus unstable conditions and for different monitored concentrations and none of them seemed to make much difference. And the key thing here, you'll notice, is that most of the observed and predicted numbers are between 0.2 and 0.4. Incidentally, we assumed an initial in-stack 0.2 ratio. So, there's not really much happening here in Wainwright, Alaska. It's only a little bit of the NOx is converted to NO2 in that plume in this location.

OLM, on the other hand, because it converts more to the NO2, somewhat over-predicted the ratios. And just looking at the statistics, the highest NO2 value once you link it with AERMOD was over-predicted by about a factor of two, perhaps a little bit more, by OLM. PVMRM, the mean was better predicted by PVMRM than by OLM. And then we have the fractional mean bias and I included a little asterisk there about not significantly different from zero with a 95 percent confidence limit for PVMRM.

Now, just doing a couple of Q-Q plots, as I said, AERMOD calculates NOx concentration as if it was an inert substance, so here's what we got and there's just a few of the highest values that are predicted high, so even though this is, yeah, this is

just the straight NOx, so we're predicting maybe a factor or two high, but once you get down to the lower values, it's under-predicting.

Now, when you combine it with PVMRM and OLM, and now you're looking at NO2 Q-Q plot and Roger showed these for other places and Mark showed them also. You're a little bit closer with PVMRM than with OLM. The blue curve is full conversion, so that's obviously way, way over-predicting, but was we showed, there's really not that much conversion at this site.

These two, this slide is for the original AERMOD assumption of this N sub Z equal to four. And then we change it to 1.28 as Roger described in some of his tests and that didn't make all that much difference here because of the lack of much conversion.

So, the limitations, just to summarize now, is just that we don't really have the emissions, we're just sort of estimating them from operating logs and performance data.

There's only one monitoring location, so we don't really know what the upwind ozone is and we're selecting only the hours when the wind is blowing towards the monitor, so the ozone that we're using may have been affected. Although, we did do a test of that on one of the sensitivity runs and because it's out in

the middle of nowhere, the ambient NOx and NO2 concentrations are lo anyway. So, it's not like you're in a big industrialized area.

So, just at this site, PVMRM is doing well. OLM is over-predicting. Both of them, sort of, over-predict the high end concentration, but to be really fair, when you're doing a comparison or trying to evaluation a model, you should look at the entire set of data sets in different locations. Because as we've seen from Roger's presentation and this one, sometimes PVMRM is better. Sometimes OLM is better. It may be due to just AERMOD itself under-predicting, so this is just one piece of information.

And finally, what we've been saying all along is why don't we do a real field experiment and get to the bottom of this instead of just dealing with all these data sets with one monitor and listing caveats?

Thank you.

**MR. BRIDGERS:** Right on time. All right. Thank you, Steve.

We have one more talk before we launch into our Q and A. I will yield the podium to Dan.

**MR. DIX:** Thank you, Roger.

We've heard a lot of information today

about how important the OLM method is becoming now that we have a new one hour NO2 NAAQS in place.

I'm going to go through a case study that kind of shows how easy it might be to collect this NO2 NOx ratio information.

You know, we're collecting this NO2 NOx information for PVMRM and OLM for the one hour NOx modeling and many facilities out there are already collecting NOx data using continuous emission monitoring systems for a variety of either state of Federal programs. These monitors out there, a lot of them are the same and they're collecting the NOx data through the chemiluminescence process. And that's also measuring NO and NO2 information.

So, kind of a brief agenda. What I'm going to go over is I'm going to talk a little bit about the project that I brought this up. I'll talk about the current set-up of what the facility had there. Then, I'll also talk about the equipment in place, the equipment that I used to collect this information. Then, some conclusions and additional considerations that came out of the project.

You know, we were conducting some exploratory one hour NO2 and SO2 NAAQS modeling for a cement facility. It wasn't for any PSD project. This

particular client was being proactive and wanted to assess their status with the new hour NO2 and SO2 NAAQS, you know, for future planning. So, they wanted to see where they fell out.

When we did the NO2 NAAQS modeling, we went straight to the tier three and we used the OLM method and we used the default .5 ratio for the NO2 NOx in-stack ratio. You know, this was a cement kiln and what we knew from other studies out there was the ratio was probably more in the ten percent range. So, we decided that we would use their existing NOx monitors to collect some of this information so that we had some site specific NO2 NOx ratio information.

So, the current set-up of the facility is that they had a Thermo Scientific 42y chemiluminescence NO NO2 NOx monitor, which is a fairly common monitor set-up to collect NOx readings. The system currently collected just NOx data on their data acquisition handling system and their project logic controllers.

Just a little brief background on the chemiluminescence process. You know, what's taking place is that NO is being introduced to O3 and when it's converted, it gives off a luminescence and the luminescence is linearly proportional to the NO

concentrations. So, that's kind of how the monitor is working.

A little bit more in detail of the monitor. This is a 42y, flow schematic and what we have is a sample coming in here and there's basically a solenoid here and it goes into two different modes. Basically, you have your NO mode here which is just a pass through. Essentially, all of the sample gas is coming through and going into the reaction chamber here. An O3 converter is introducing ozone into this reaction and then when the NO converts to NO2, that luminescence is then the NO concentration.

This solenoid then switches back and forth, usually every ten seconds, and it goes into this other mode known as the NOx mode and this is where the NO2 is converted to NO and now that is sent to the reaction chamber and the ozone is introduced. And now when that NO is converted to NO2 and luminescence is given off, that measured amount is now my NOx amount. And when I take my delta between the NOx and the NO, now I'm getting our NO2 reading. So, this particular type of monitor is measuring NO, NO2, and NOx.

So, what we decided was it was definitely easier and more cost effective to install a temporary data logger than it was to set-up NO2

readings in their current data system for a couple of reasons. They were collecting this NOx data, you know, as a permit requirement so they didn't really want us tinkering with that monitor at all. Also, if they were to set-up NO2 readings on our PLC system, it would have required the purchase of new cards as well.

So, we decided it would be simple to install our Campbell Scientific SEAR-1000 data logger and we hooked it up to the unused ports on the 42y and we programmed the SEAR-1000 to take one minute average readings of the NO, the NO2, and NOx.

This is just some pictures of the set-up. This is SEAR-1000 here. In this particular picture there's a lot of wires because I set-up to a met tower. I had only three wires coming out of the SEAR-1000 and we hooked them up to these empty slots here on the back of the 42y and the wire that is existing here goes to their data system, so we didn't have to mess with any of these wires or anything. We simply were able to hook up to some existing ports here. One was reading NO, NOx, and NO2 and we were lucky in that the analyzer -- usually these analyzers come set-up to export to these particular ports here, but it's easy to set-up. You can see how many empty slots are on the back of these analyzers.

So, here are the results. This is basically a snapshot of the one minute averages. We have the NO NOx readings on top here and then the NO2 and I've scaled this logarithmically so we could see the NO2 NOx ratio. Basically, we found what we thought we would see. We weren't above 50 which was good. We were in that six to ten percent range for the in-stack NO2 NOx ratio. So, we felt confident that we could go forward with using ten percent in the OLM method.

When we did ten percent and we modeled it for this facility, that was enough to get us below the NO2 one hour NAAQS.

So, just some conclusions and considerations that came out of the project. You know, we really found this to be a, you know, cost effective and simple way to collect this information, you know, for use in NO2 modeling for AERMOD's OLM and PVMRM options.

You know, a lot facilities operate these exact type of NOXCEMS under a variety of different programs. There's a lot of them out there that could be potentially collecting this info.

You know, it was easy for us to set up a data logger. We set it up. We left it there for a three month period and while it was there, we had the

facility run the kiln through all the different operating conditions so we could get a good, robust asset.

That being said, there's no reason why they couldn't have hooked up their data system to collect this data as well, but for this particular case, it was just easier to come in and put in an external data logger.

What we're recommending to our clients for facilities that may not have any NOXCEMS in place is a lot of facilities are also required to do NOx testing every year. So, we're recommending that they talk to their stack testing company and request that they also do NO2 testing during that. I mean, you're only going to get a three hour data set there, so if it's a fairly continuous type of operation, it might be useful. If you're doing RATA testing, you know, it could be a 12 hour period, but you're not going to get as much as if you had NOXCEMS in place.

Like I said before, this was a purely exploratory type modeling we were doing. It wasn't for a PSD, but our thinkings are that, you know, if this were to go to a PSD modeling project, that we would include the set-up of all this in a protocol, outline exactly what we did, how we collected the data, what we

used in the protocol so it's out there in front of the stage to see how we collected it.

You know, I just took one minute average concentration values from the data logger. I'd be curious if anybody had any other opinions on other different statistical approaches to take for that in-stack ratio information.

As far as QA QC considerations on this, you know, the NOx monitor is being calibrated per requirements. There's a couple of other things you could do. You could send a known amount of NO2 cylinder gas to the analyzer to see how efficiently the NO2 converter is working.

So, that's pretty much it. You know, this was just a very cost effective, easy way to collect this information and I feel like a lot of facilities could potentially benefit from this so we could collect this information and get it in some type of database so that we have a place to go to to get the in-stack NO2 information.

With that, I'll turn it back over to George.

**MR. BRIDGERS:** That's a great talk, Dan, absolutely.

Actually, I should have told you don't

run away because we're going to do our Q and A and this will actually span, let's see, Erik and James and Bob and Steve and Mark, so everybody who's talked since our last Q and A, the hot seats are up here in front.

The floor is open.

**MR. PAINE:** I have a question of Erik, actually.

You did a lot of stack ratio. Now, did you use the version of AERMOD where it extends the downwash above the formula height?

**MR. SNYDER:** We used the standard version, so most of the work -- I think some of the cumulative work, and James you could answer on them other runs you did, but a lot of the oil and gas work we did last summer which is when we did it.

**MR. PAINE:** So you have to raise the stack higher?

**MR. SNYDER:** Yes.

**MR. THURMAN:** Yeah, I think everybody's runs used version 11059.

**MR. PAINE:** You may have to update your conclusions.

**MR. BRODE:** They used the version that does extend it above that.

**MR. PAINE:** Oh, okay. They used the

current version. Okay. You're sure?

**MR. THURMAN:** We used the 11059 which introduced that.

**MR. PAINE:** Okay.

**MR. THURMAN:** That change was introduced in version 11059 and 11059 is the version that they used.

**MR. PAINE:** Okay.

**MR. THURMAN:** But it's not the most recent version because that was recently updated.

**MR. PAINE:** What would that do?

**MR. THURMAN:** That would have no effect on these results.

**MR. PAINE:** Good. Okay.

**AUDIENCE MEMBER:** This is Dana Wood with BP.

I just wanted to make one clarifying statement, particularly about the Empire Abo data set. It was collected by Doug Bluett with Amoco at the time. It's a BP data set. And during that data collection, there was no actual emissions data that was collected. What was used in the New Mexico evaluation was the potential to emit emissions and so, really, to add to the statement that Steve Hanna made, we really need to get out there and do a field study where we collect

actual, you know, actual emissions data at the same time we're collecting the monitoring data to truly evaluate these models. And I know that's going to be an expensive undertaking and I hope that the EPA would be willing to collaborate with industry and other groups in order to do that.

**MR. FOX:** I think we would agree and echo that same sentiment as follow-up to the conference as to how we might be able to set those things up. Again, as I mentioned earlier in terms of the budget constraints and the like, but I think working together in terms of developing the protocol for the field study and the like and if there are funding sources that could get it done, it would certainly serve a valuable -- it would be of great value to the community in getting that done.

**AUDIENCE MEMBER:** Mike Anderson, TRC.

This is a question for Roger.

Roger, did the model performance evaluation results that you presented this morning take into account the calculations of downwash from stacks higher that GEP formula height or the use of the AERMINUTE low wind speed data?

**MR. BRODE:** I think downwash was present. I don't know that there were any stacks that

were above formula height, but I'd have to think about the most recent version I ran for those, but both of those cases did have site specific data, so airport data were not relied upon.

I mean, that might be a good question to look at if there's a nearby -- near enough by airport that would be more or less adequately representative, but I guess, yeah. The one man data would not be available for those time periods.

**MR. ANDERSON:** Okay. So, the point I was making is that the issue of the effect of the calculation of downwash for stacks above the formula height wasn't addressed there and the low wind speed case hasn't specifically been addressed in those studies?

**MR. BRODE:** Those questions were not paramount in doing those evaluations. I don't know that either of those questions really played a very significant role, but it's something that maybe I'll take a look at.

**AUDIENCE MEMBER:** This question is for Bob Paine.

In your EMVAP paper, in a permitting environment, any permit must be enforceable. How would you enforce a distribution of emissions in a way that

the plant managers would be happy with?

**MR. PAINE:** Okay. I'm not going to speak for EPRI because EPRI is developing the tool and not the policy, but I'll put on my consultant's hat and I would say that this would be averaged over a five year period. Let's say. Because the modeling would be over a five year period and you would make sure that your distribution of emissions was within the envelope of what you modeled.

So, it could be -- if you had an exceptionally odd year, you could average it with the four other years you're using in your distribution. That's the best possible approach.

I don't know if EPA is going to think about that, but that -- okay.

**MR. BRODE:** That was actually my question to Bob the last time he presented it to us.

**MR. PAINE:** I'd like to give you an answer.

**MR. BRODE:** I mean, if you have an operating plant, I could see where you could base it on existing data, but you know, we're open to some ideas on how that might be done. I think that's one of the key questions in being able to move forward with implementing an approach like that.

One thing is, the form of the standard, the fact that it's a multi-year average is something that should be taken into consideration. It may give a little bit more flexibility such that if you determine what the, you know, you take a permit condition on a particular emission distribution, you can track it over time and if you tend to overshoot, at least you can maybe make plans to come back into compliance over the multi-year period. That's something that could be considered as part of that.

**MR. FOX:** This is Tyler. I had the same question and it is something for EPA, not the modeling folks, to consider because that would be an important aspect in how you actually put it in a permit limit and monitor it and manage it as such.

**AUDIENCE MEMBER:** Eladio Knipping, EPRI.

Actually, Tyler, you're sort of expressing the same type of comment that I was going to make that this is something that is going to require, first, an understanding of the emissions distributions of existing units and understanding how different units of different named clay capacities, capacity factors, control technologies, how they operate, and what are, you know, what types of emission distributions would be acceptable to include in such an exercise.

And this is really a discussion that EPA will really have to take the lead on, but we will be providing information and analysis of that type of information so that people can understand how different power plants and the different operating environments and under different operating conditions what their emissions distributions are and what their stack parameters relevant to those emissions distributions are as well.

**MR. FOX:** This is Tyler Fox, OAQPS.

That would be very helpful. I think, in addition, to understanding the operating conditions and, I guess, the value or usefulness of that approach in those different conditions. That would be useful for others in the community who have maybe an understanding absent the same type of continuous emissions monitoring information of the nature of the distribution of their emissions or operating cycles to then understand whether or not it would be a valuable approach for them.

I guess, along those lines, I'd have a question that -- two fronts. One is, how do you take an approach that is designed for a certain type of source and has data that may not be readily available to other sources and use it across the different

situations for permitting and how do you, you know, tie a new source's distribution that doesn't yet exist to an existing source or a set of existing sources' profiles?

I think those are questions that we would like and something to consider in comments from the community on as they review the report and understand this method. As Eladio said, and then try to translate it to what it would mean to them in terms of how they operate.

**MR. KNIPPING:** I just want to say that first the -- with respect to your first comment on how industries or other sources may implement this tool without the appropriate knowledge, well I can't really speak to that, but that should not preclude the use of the tool for this particular application of power plants where we do have that information.

Second, as to how an emissions distribution is assigned to a new source based upon the amount of data that we're gathering over many years for 1,000 plus units, that is a discussion that we're really going to have to have with the information available. Without that information available, we're really just speculating at this moment and speculating is dangerous.

So, let's first wait for the data to tell us what and inform us how we move forward.

**MR. FOX:** Thank you. I appreciate that.

I guess my first comment was really to the community at large, for them to think about this approach and relate it to their experience in terms of understanding their operations so that they can then have an understanding and express an opinion through the public comment period for this conference to EPA in terms of the value that they see in this approach.

**AUDIENCE MEMBER:** John Glass, South Carolina DHEC.

I think this question is probably either for Roger or Tyler. I wanted to go back to that March memo of last year. The Clarification Memo. We discussed the screening of background sources and I wanted to make sure that I was interpreting that correctly. Some others might be interpreting it the same was as I am and I want to clarify that.

You cautioned against the prescriptive application of the old 1990 guidance, you know, causing overly conservative results. Part of that prescriptive guidance would be, you know, you form a significant impact area. You would include all of the sources within that significant impact area. And then you

would screen from 50 kilometers there out.

I think the intent of your memo would be certainly that you could apply that significant concentration gradient on the screening area, 50 kilometers out from the significant impact area, but I think your memo also implies that you could apply that within the significant impact area.

Is that a correct interpretation?

**MR. BRODE:** I think that's consistent with my understanding of Appendix W and that's kind of the main point. It was to focus what Appendix W actually says regarding nearby sources should be considered for inclusion and the significant concentration gradient is the only criterion that it provides. And it goes on to say something like except in rare cases there will be relatively few sources. I forget the exact wording, but yeah, I don't -- I think it's for the whole domain and certainly the closer the source, a background source, is to your source, the more likely it would be causing significant concentration gradients.

But also it tried to make a point in that memo that that question, should that source be included or not, is not an isolated question. It has to be looked at from the perspective of what ambient

background concentrations I'm also going to be including and the cumulative impact analysis and make sure that all the different pieces fit together in a way that makes sense for that application.

So, that's something I hope people will keep in mind is look at each of those pieces, not independently, but as a whole. Make sure they make sense together.

**MR. GLASS:** Right. And I think that memo also talks about there are certain situations where you would not include any background sources and probably the only way you're going to get at that is using that significant concentration gradient. I think.

There may be another way to exclude those within the significant impact area, but I'm thinking that's the only way you're probably going to get at that.

**MR. BRODE:** Well, every case is different and I think that question then gets to, not just where the monitor is, but what metric you take from the monitor to represent background.

So, if you take the first highest value across three years or five years of monitoring, that might allow you to justify eliminating more sources

from the inventory than if you used something -- a metric that provided a lower background.

**MR. GLASS:** Right. Thank you.

**AUDIENCE MEMBER:** This is Steve Hanna. I hear the words significant concentration gradient. There's always concentration gradients. How do you know or define when they're significant?

**MR. BRODE:** Appendix W does not define significant in any more detail than that. So, I think that's a good question and that's part of it.

**MR. HANNA:** You'll know it when you see it.

**MR. BRODE:** Well, exactly. I mean, we actually -- one thing is people, we actually provided some examples in, I think, our webinar. I don't know if they've shown it here.

We took a source, actually a taller stack and a shorter stack and calculated the concentrations and actually calculated gradients. And one of the things that our March memo points out is that Appendix W just says significant concentration gradient. It doesn't say a gradient in which direction, so there's a longitudinal gradient. Along the path of the plume, there's a lateral gradient and, in my view, maybe the lateral gradient should be given

more weight, in fact, especially for an hourly standard. Because one of the issues is if there is a strong lateral gradient, it means that that plume's impacts may not be adequately captured by a monitor.

I think that's, I mean, Appendix W doesn't go on to say why that's the one criterion, but if you think about it, I think that makes sense. If there is significant concentration gradient, then an ambient monitor may not adequately capture that source's contribution.

But, you know, we actually did some plots and it was kind of interesting. We might try to do some more. We actually talked about maybe modifying AERSCREEN to output the concentration gradient versus distance or something.

There's lots of idea, but there's just too many of them and not enough time to do them, but I think it's a fair question. I don't think that there's a clear answer for it, but we're welcome to hear other feedback or comments or suggestions along those lines.

**MR. BRIDGERS:** And Roger I was just going to add the fact that in addition to SCRAM being the place for MCHISRS and the Clearinghouse actions and Clarification Memorandums and everything else. The two webinars are actually posted on SCRAM. They're linked

under the recent additions, so there's a webinar that was given back a couple of months after the March memo came out for NO2 and then one for SO2 that was given later in the summer.

So, if you didn't have a chance to participate, you can go review the slides by going to SCRAM under recent additions.

**MR. BRODE:** I think they actually have the audio with that, too.

**MR. BRIDGERS:** I don't have that link, but it's posted.

**MR. BRODE:** I think it was June 2011 NO2 webinar where we included those gradient plots.

**MR. BRIDGERS:** We do have time for more questions.

**AUDIENCE MEMBER:** Hi. My name is Sam Sampieri, Department of Energy and Environmental Protection in Connecticut and, of course, our old Commissioner is Gina McCarthy.

My question to you guys -- by the way, Bob, that was a great analysis you did. It would be great for one of our EGUs in Connecticut to use.

However, we have a problem with all that, with our SO2 SIP modeling. And the question I have basically for EPA is we have a year and a few

months to submit modeling for our SO2 SIP. We're not going to submit non-attainment. Well, of course, without any further ado to it, we could have some non-attainment issues.

My question is, you would have to change Appendix W to use your type of analysis to put in our emission rates for these big EGU coal plants. That could work to be in attainment, but you would have to change Appendix W and 8.1 or 8.2 table where you would have to use the maximum allowable emission rate and we're going to be looking at pound per hour maximum hourly emission rate for the hourly standard to be in compliance by 2017.

So, you would have lean this process to change Appendix W in time so all the SIP modeling can be submitted.

Am I off base?

Plus, are we going to open up a can of worms if we start letting EGUs doing that kind of analyses and, you know, we're going to start talking about we're getting away from the maximum allowables and we're going to start modeling with actuals or your Monte Carlo analysis.

**MR. FOX:** Tyler Fox, OAQPS.

I think, as we asked before, the

critical question is whether or not you could actually implement permit limits based on that approach. I think a lot of the questions that we asked in terms of that approach from a technical standpoint as well as actually how you would implement that would have to be resolved.

To the point of modifying Appendix W, right now I would say yes. In order to use that type of approach, it's conceivable that it would require a modification to Appendix W and yes, in terms of updating Appendix W in that time frame, that would be an extremely difficult task.

That being said, we issued guidance in March that provided some flexibility. So again, internally we would have to work with our management and also the folks in the policy division and the OGC to see whether or not there's existing flexibility to interpret things in a way that would allow that to be done without Appendix W.

But again, that would have to be something that we have both the information that Bob and Eladio referred to, have an understanding of how it would actually be implemented in a permit limit, and then work the chain within EPA to see whether or not that's a feasible approach.

We're understanding and trying to be as flexible as possible, you know, working through guidance rather than through rulemaking. You guys know very much, as you've indicated, the rulemaking process sometimes does not work with the existing time frames for other compliance purposes.

**MR. SAMPIERI:** It's one thing to talk about new source permitting, but it's another thing now with the SIP modeling that's going to come due fairly shortly and, you know, do we have the time to do all that and so, is that one of these things where we'd be doing a case-by-case basis? But that's going to be in a SIP, you know, and each state, of course, has its own issues.

Thank you.

**MR. FOX:** Thank you for that question.

**AUDIENCE MEMBER:** Dick Perry, Stinger.

I was interested to see Monte Carlo raising its head again because about , oh, eight or ten years ago, John Chadwick and I did a study of running of looking at heavy metal variability running through a kiln, a cement kiln processor and in that Monte Carlo analysis and something you may want to look at, we did one further thing after we had run this. I got the statistics. We looked at the likelihood of that

maximum potential to emit ever occurring and found that it was out at 33 standard deviations from the center line which, you do the math, and it's an unimaginable number of years.

So, the point there is that between 33 standard deviations and what you're likely to see, you've got a lot of room for maneuver to make things more reasonable and still give yourself a comfortable margin of conservatism on how you structure the emission variability.

**MR. FOX:** I have one more question for Bob. This is Tyler.

In terms of the distribution that you showed, do you understand or know what's causing that high end of the distribution? Are those peaking units or, you know, start-up shut-down emergency generator type of use that may actually be remedied through the treatment of intermittent sources or are those, you know, base units operating at their allowable levels just in an -- infrequently?

**MR. PAINE:** Well, for the AERMOD evaluation data sets and Roger, you may have as much understanding as I do, these were back intersection he 1970s and 80s. It could be just a slug of coal with high sulfur emissions. I don't think it was a bypass

stack operation or anything like that. It was just a delivery of coal or maybe -- because I don't think that they had any controls at these sources. So, it was the old fashioned sulfur variability.

**MR. FOX:** Okay.

**MR. BRIDGERS:** I know we're running just a few minutes long on the session, but we'll entertain a few more questions. We had a lot packed into this session and with the remainder of the conference, we only have one more Q and A time before we get to the public presentations tomorrow.

**MR. HANNA:** This is Steve Hanna. I have another question about this Monte Carlo.

If you have to model multiple sources in the domain, do you account for correlations between the emissions of the different sources?

**MR. PAINE:** Well, you can by having the same random number starting point for sources that would be correlated and if they're not, you can add different random numbers starting point. So, you can accommodate that.

And of course if sources are not to be run with variable emission distribution, you can run them normally, save those concentration files and just add them into the EMPOST processor. So, you can

accommodate variable sources and constant sources in the whole process.

**MR. FOX:** This is Tyler.

That actually is a very good question, especially in the context of a SIP situation where it may be a multi-source area. So, how to handle that would be a useful thing to maybe put some information out there on. Or thoughts.

**MR. BRIDGERS:** Okay, we'll do this like the auctioneer.

Questions going once? Ah, we'll just cut to the chase. Sold.

I appreciate everybody being patient through the session, if we could give our speakers another round of applause.

Let's go ahead and take until five minutes after three. That's just a few minutes under 15, but that will hopefully keep us on schedule to get out of here.

So, thank you.

(WHEREUPON, a brief recess was taken.)

**MR. BRIDGERS:** I guess I will call Tyler Fox to the microphone. Paging Tyler Fox.

**MR. FOX:** All right. The room doesn't seem as full. I guess everybody is still taking their

break.

We're going to go ahead and start introducing this last session for the day. Very much appreciate the presentations that we've had up to this point. They've been very valuable and very productive in terms of informing us and hopefully the community and we look forward to further engagement on those new methods, techniques, and the like.

Similarly, we're going to be introducing some of the work that EPA has done jointly with the FLMs and try and engage similarly with the community and get your thoughts and comments on this work and how we're approaching handling the issues that we see in terms of potentially updating Appendix W to meet ongoing emerging needs.

So, over the past years, at least three or four, we've been engaging with the FLMs and we've had various meetings in terms of the issues related to the long range transport applications and issues related to chemistry.

It's clear, over time and particularly now, that our interests and needs are overlapping across a number of multiple programs and regulatory responsibilities that we have.

Examples include the NEPA air quality

analyses for energy development on Federal lands. We went through a process with EPA, Department of Interior, and the Department of Agriculture to sign an MOU on going forward with consistent and credible analyses under NEPA and that took a huge step forward in terms of a number of issues related to long range transport and chemistry and it shows the commitment of the Federal partners to work together and try and make things both more technically credible, but also respect the resources that are required to conduct these types of analyses.

And most recently, I've mentioned in previous talks EPA has granted the Sierra Club petition on ozone and secondary PM2.5 models and we are considering in engaging in a process starting with this conference and potentially updated Appendix W to bring those types of models in.

I will say that we will be following the IWAQM process for conducting the necessary evaluations and reporting of those evaluations and engage with the community in the same manner in which we did previously and that resulted in the 2003 promulgation of CALPUFF.

In addition to the Federal partners viewing, you know, the needs and the like, we have comments from the most recent modeling conference from

the ALMA AB-3 committee that also provide information and support for moving in this direction.

The comment areas included the need for regional models, Eulerian models, best uses for Lagrangian and Eulerian models, and where does EPA go from here.

So, I just wanted to pull some of these comments. In terms of needs for regional models, they identified the fact that ozone and PM are pollutants involving precursors and trans-chemistry in transport and that our current Appendix W do not address these needs.

Just for everybody's clarification, we addressed the use of photochemical models in a separate guidance document. The SIP modeling guidance for ozone, PM, and regional haze SIPS and attainment demonstrations which does link to Appendix W and follows the same type of approach in terms of an alternative model and applies that and, as you all know, models such as CAMX and CMAQ are the workhorse models in the SIP context. And that EPA needs to provide procedures for modeling PM2.5 and I think that we recognize that. We're working on draft guidance in the permitting context that gets at single source issues and so we certainly would agree with those

comments.

In terms of the Lagrangian models, they are suitable for individual sources, but there could be a resource penalty in terms of running them. And the chemistry, at that point, was limited. Ozone is not modeled and PM2.5 can be modeled in certain degrees of sophistication.

In terms of Eulerian models, there's issues in terms of, you know, conserving mass and the like with respect to these models. I think a number of the analyses that we have done or are doing understand those points and are trying to address those points. And obviously we're dealing with situations where we have complex, non-linear chemical conversions and at sometimes long distances for regulatory purposes.

And in terms of approved use of these models, there have been evaluation studies, but EPA should have system to determine acceptable criteria for approving these models. And the models accuracy is good in some areas and poor in others. Continue to work on consistent evaluation approaches recommended, similar to short range models.

Obviously, looking at those comments and looking at the direction that we're moving. We believe that we're responding in line with those comments and

our needs, as I mentioned, we've been discussing with the Federal Land Managers and other Federal partners.

We -- this is a slide from a previous, earlier presentation. As I mentioned, we'll follow the same type of IWAQM effort. We've defined it as phase three and we're focusing in on that next generation of model to meet those needs such as single source ozone and secondary PM2.5 and AQRVs.

And our program needs and commitments, again, have made it clear that we need to address the long range transport and chemistry issues and have made that a high priority for us.

In terms of meeting with the FLMs, we initially talked about the needs and attributes for models as part of the process and previously under IWAQM, the group put out a models attribute study or report. We would feel as if we would follow suit and provide that type of document in the near future. These are some of the needs and attributes that were discussed as part of those interactions with the Federal Land Managers.

As we move forward, as has been pointed out throughout the conference thus far, evaluation is at the heart of our ability to move forward and have the confidence necessary to use and, in this case,

promulgate new models and techniques.

There are a number of historical efforts that looked at these things. EPA's 1986 eight model study based on the Savannah River and Oklahoma mesoscale experiments and then the use of those throughout these different efforts that ultimately led to the promulgation of CALPUFF were critical in that process.

However, there was some lessons learned in terms of these evaluation efforts. There is no EPA recommended methodology for evaluation of long range transport models and there's no consistent approach between the efforts in the 80s and 90s.

The evaluation methodology used all published AMS metrics and data organization strategies. This did not take into consideration the regulatory use of the long range transport models and the schemes for weighting those may not have been appropriate.

We observed high sensitivity of these models to meteorological inputs. And we see the need for more objective meteorological performance evaluation measures as part of moving in the future.

And there were no data sets available to evaluation chemical transformation mechanisms with the long range transport models.

So, in terms of working forward and, as mentioned previously, a fit for purpose type of paradigm, we feel compelled to define our performance objectives by starting with the regulatory use of the models. And our current use of these models include PSD class one NAAQS and increment analyses. The AQRV analyses. A visibility and deposition going on. And, in terms of potential future uses, the single source ozone and single source PM2.5 analyses.

So, in terms of an evaluation framework moving on, we believe that the evaluation of these long range transport models within their defined regulatory niche requires an evaluation of these three independent components of the system; the meteorological component, the advection and diffusion component, as well as the chemical transformation.

In terms of the inter-comparisons, the modeling comparisons, the long range transport models performance is going to inherently be linked to the suitability of the meteorological inputs, so you get out what you put in and so we need to make sure that we address those and that we use a common source of meteorological data across these modeling systems when we evaluate them to reduce potential contribution of the differences put into those analyses by the

meteorology. And that was one of the motivating factors for the beta release of MMIF, the Mesoscale Model and Interface tool. Program. Sorry.

At least I didn't say BARF, right Bret?

And, you know, the meteorological model performance is necessary and is integral to any part of an evaluation framework and, as we talked about, we put forth the MMIFSTAT tool to facilitate those types of evaluations and the visualization tools to facilitate the use of that information for visual inspection of these data in a number of different frameworks that exist in different parts of the community.

In terms of the single source chemistry evaluations, application of these models for chemistry usually only involved in individual or a small set of sources and traditionally, photochemical model evaluation techniques the chemistry evaluation are combined with the inert Tracer evaluation to evaluate the suitability of a model. The best performing chemistry model will only be as good as its ability to treat advection and diffusion appropriately.

So, we are taking these things into account and trying our best, as you'll see in the following presentations, to put forth an evaluation paradigm and put the models that are currently

available to the community through their paces consistent with the regulatory applications that we see these fulfilling under the guideline and then letting that information go to you in the community and getting your feedback on that.

So, with that, I'll turn it over to Kirk Baker.

**MR. BAKER:** Thanks, guys. Thanks, Tyler.

I'm here, basically, to provide an additional layer of overview to Tyler's overview and to fill in some of the gaps between Roger's talks.

So, I'm going to talk a little bit today about some of the work that we've been doing to get a better idea for what types of tools we have and how well they work and in terms of looking at the secondary impacts from single sources, so a lot of this is a work in progress. So, as we go through this talk I'm going to show and the talks after this, just keep in mind that this is a work in progress. All of this is evolving. So, if you see something that looks really cool, I wouldn't get too excited or I wouldn't get too upset about stuff you're seeing because it's likely to change as we go through this process of understanding these things better.

So, what I've got here that's probably kind of hard to see in the back, I just got a chart showing some of the different scales of modeling and types of modeling we do. We do single source modeling assessments at fence lines, urban scales. We do all sources, types of assessments for the purposes of projecting design values for model attainment demonstrations of the NAAQS.

And where I've got some of the yellow areas that we're trying to focus on learning more about single source impacts of secondary PM2.5 and ozone near the fence line for permit applications, urban scale types of impacts, and long range transport assessments for things like ozone.

So, I'm going to talk first a little bit about some of the work that we're doing for looking at long range transport of ozone, PM2.5, and deposition at class one areas and then talk a little bit more in-depth about some of the single source modeling we're doing looking at fence line and urban scale types of evaluations.

So, for the long range transport, this is important to do primarily for NEPA type of assessments and we're looking at a lot of existing and alternative types of modeling systems for PM and ozone

in combination. A big step for this was Brett having the foresight to develop the MMIF tool that he talked about yesterday, so now we've got a program that can translate prognostic meteorological model output into a common platform for a variety of dispersion, Lagrangian, and photochemical models. So, when we start comparing all these types of modeling systems, we're starting from as consistent a place as possible with the meteorology and that's very helpful.

So, with that tool in this beta form is currently available on SCRAM and we're actively supporting that internally in our group and with Environ.

So, a lot of the work that has been done is currently being done through contract with Environ and this is going to be talked in more detail in subsequent presentations.

So, one thing that Tyler touched on was how well do these modeling systems just transport mass around a large domain between source and receptor? So, one useful way of trying to evaluate that is comparing modeled -- the modeling system predictions for Tracer release experiments.

So, we have a big report where we've compared a variety of different Lagrangian and

photochemical modeling systems to several different Tracer release experiments like ETEX in Europe. The Great Plains Tracer experiment and CAPTEX in 1983 that people are pretty familiar with. And as part of that evaluation we looked at SCIPUFF, CAMX, HYSPLIT, FLEXPART and CALPUFF in different combinations for different field experiments.

In addition to that, there's a second report that Environ is working on which is more of a consequence analysis and Bret's going to talk more about that later.

Notwithstanding how they compare with Tracer experiments, it's just how consistently do these models predict air quality and air quality related values like deposition at class one areas?

So, what the plot is on the bottom is some results from that report that show CAMX as the two bars on the left for total nitrogen deposition at one particular -- at a few different class one areas and CALPUFF total deposition at a few areas on the right and we can see that they're different. We're not trying to say that one is better than the other, but we just want to understand what these differences are. And I think in the end, when we apply models as consistently as possible and against each other, we're

probably going to learn things about both of them and make both of these types of systems better and have a better comfort level when we try to apply them for these types of purposes.

So, switching gears back to the urban scale, we're looking at the single source impacts of PM2.5 and ozone for PSD NSR purposes. Tyler talked about the Sierra Club petition, so now we've made an agreement to come up with some modeling guidance to put some information out there to provide some steps for people on how to do these types of assessments.

So, we're going to build off things that are already out there like the NACAA recommendations for doing single source assessments for PM2.5 for the purposes of PSD NSR. Evaluate some of those approaches and see how well we think that's going to work for these purposes.

In addition to that, we think a lot of the work as we go through this process is going to inform and help improve technical basis for establishing inter-pollutant trading ratios for PM2.5.

So, NACAA recommended a multi-tier approach and, in general, the approach just kind of goes from a more simple approach to a more complex. So, they start off with just applying AERMOD and taking

the primary PM2.5 estimates and adding offset ratios to estimate secondary impacts at receptors near the source. One issue with that is it can be kind of complicated to come up with really good offset ratios that are location and site specific.

So, moving forward from that, there are more complicated approaches like using Lagrangian puff or particle models that have secondary chemistry or using a photochemical modeling system. They recommended all three of these things as a possibility.

And so when we get to something like a photochemical model simulation, there are a lot of different ways to track the single source contribution. We need to do a lot of work and we're just starting to do that work to try and understand which approach is going to be the best or are there a bunch of comparable approaches that are going to be equally appropriate for these types of purposes.

So, we're working with contractors, other Federal agencies to try and figure these things out.

One thing that we're thinking about is the feasibility and utility of a screening level tool that would provide quick, reasonable, and credible single source secondary impacts before you got to more

complicated modeling systems. And just thinking along those lines, where do you draw the line between using a screening level tool and more complicated tools like Lagrangian puff models and photochemical models?

And then when we get to these places like a photochemical model, like I said before, how do you apply it in a way that's going to be appropriate for these purposes?

Ralph's going to talk a little bit later about a possible screening level tool. At CMAS last October, Environ presented a reduced form single source screening model that estimates ozone impacts from VOC and or NOx emissions based on CAMX hired order DDM modeling that was done for the City of Sydney. So, they wanted to come up with a screening level approach for their permitting purposes before they required sources to do a full scale photochemical model assessment.

So, we think this approach may potentially fill a need for a technically sound screening tool to allow us to efficiently evaluate what sources would be required to do more rigorous types of assessments and hopefully this type of tool could also help improve or give us a better inter-pollutant trading ratios as we go forward.

For single source modeling, we've currently got three reports being compiled by Environ. I've touched on the top two already. We're hoping that, even though they're geared more towards long range transport assessments, but I think a lot of the lessons we've learned and the information there is going to be helpful on an urban scale, too.

The third bullet on the top, the evaluation of chemical dispersion models using atmospheric plume measurements from field experiments, that's the one I have not touched on.

So, for an urban scale in the near-field, how well do these models perform and one way to try to assess that is to compare the model estimates from a particular source to field measurements that are made.

There have been many different field campaigns over recent years where helicopters or airplanes have flown through the plume of a particular source, so we're hoping to use some of those experiments as an approach to try to get an idea of how well some of these models are performing for these purposes.

The report is focused on two different field campaigns. Tennessee Valley Authority

measurements in 1999 from Cumberland and measurements made as part of TEXAX 2006 around Oakley Union Plant. So, those are the two field experiments that are being evaluated at and the models that we're evaluating there are SCICHEM, CAMX, and CALPUFF. Both the regulatory version of CALPUFF and the newer version six series of CALPUFF.

So, we expect that report to be available in early April and that's going to be talked about in a little more detail later.

Jim Kelly is going to talk a little bit about some of the SCICHEM work we're doing in-house. We've evaluated against the 1999 TVA field experiment. We're learning a lot about the application of SCICHEM and we're hoping that work is going to lead us towards better understanding the model and finding the best approach to ramping it up to use it in a regulatory fashion where we would apply the model for multiple years.

The other thing that we're doing is we're comparing a variety of different photochemical modeling systems, single source approaches, to the 1999 TVA field experiment. And this is just a plot of SO2 on the top and NO2 at the bottom that Jim put together for the TVA case from SCICHEM.

The photochemical modeling that I just mentioned, there's a variety of different approaches that we're using in the photochemical models and a variety of photochemical models. We're starting off simple using CAMX and CMAQ and a brute force approach where we just zero out the facility.

And we've also got an additional sensitivity approach where we're using higher order DDM where available, DDM to track the emission sensitivity from that particular source to see what its contributions are. Just -- all of these techniques are just a way to isolate the contributions of a particular source and we're comparing these to the field campaign measurements.

In addition, we're looking at CAMX source apportionment. When it becomes available, we're going to be adding in CMAQ source apportionment.

When we've got issues with proximity, a lot of times the fence line and the source are a lot closer to each other than the grid resolution that we're using in the photochemical model.

So what do we do in those situations?

So, we want to look into using ultra-high resolution fine grid nests around the source looking at sub-grid and plume treatment and sub-grid

and plume sampling to see how well that's going to work for these types of purposes.

And, as we move forward, we're hoping to fold into that the new CMAQ sub-grid plume treatment, APT, that's being developed by EPRI.

The modeling set-up that we're using for this evaluation, it looked pretty good on my screen up in my office, but not so good here. So, on the right is the photochemical modeling domains. We've got a 36 kilometers continental U.S. domain and we're nesting down with a 12 kilometers domain which is about the size of the big box. And then it's kind of hard to see, but around this green dot we've got a smaller, four kilometers domain, and that's what I'll be showing results from the four kilometers domain today. Preliminary results.

So, for the TVA case in 1999, we're using 1999 CEM emissions for NOx and SO2 from Cumberland. In addition to that, we're using 1999 biogenics and the other anthropogenic emissions are based on the 2001 NEI.

WRF 3.3 was used to feed met inputs to CMAQ 4.7.1 and CAMX 5.40.

So, just to kind of show comparability in estimates in a photochemical model from a single

source, there's kind of a lot of things going on here, so I'll walk you through what each of these tiles are. What you're looking at, in each tile is the two week episode maximum of NOx from this particular source.

So, on the top row we've got CMAQ estimates and on the bottom row are CAMX estimates.

On the top left is the CMAQ brute force simulation, so just from zeroing out emissions from the facility, this is the two week maximum contribution that we're seeing.

In the top center is the contribution based on DDM where we did not do any brute force sensitivities. We just used DDM, the approach, to track the contribution from that source through the photochemical model and you can see that those two approaches come up with pretty consistent answers.

On the bottom we've got the same types of things with CAMX.

On the far left is the brute force zero out run.

In the center, using higher order DDM.

On the right is source apportionment.

Again, they're pretty consistent, but there are differences and as we move forward, we want to better understand these differences and figure out

what the reasons are, if they're important differences or not when we start using this modeling for permits.

On the top right, we're continuing to work with ORD to implement source apportionment into CMAQ and once we have that, we'll have the sixth tile on this plot.

Looking at the same thing, but this time this is the two week episode maximum impact from primary elemental carbon from this source. So, the tiles are the same thing here on the top. You see the CMAQ brute force zero out of elemental carbon compared to DDM and they're pretty comparable in their estimates.

On the bottom, you've got CAMXs results. We don't have -- the most recent version of CAMX does not have DDM for PM2.5 species, so we don't have any information on that, but the source apportionment on the right and the brute force on the far left are pretty comparable. The differences you see in some of the spatial extent of the plumes are due to differences in the horizontal and vertical advection schemes in these two models and it's also a function of the color scheme that we're using. That tail to the right of the plume is there in CMAQ, too. It's just not quite as pronounced to show up as different colors on this plot,

the lighter blue.

As I mentioned before, there's going to be times we've got a source receptor proximity issue and this just shows an example of that. If you're in the front couple of rows and can see it. The yellow boxes are one kilometer grid cells and the source in question is outlined in green. At the monitor -- the impacts are being assessed at is the letter A. The B monitor is a meteorological monitor. You can see they're all in pretty close proximity to each other and the one kilometer modeling cells do not really adequately represent this proximity relationship.

So, we want to look at a variety of different approaches and this is kind of the next step. We have been able -- we don't have a whole lot of results for you yet, but we're going to evaluate the sub-grid and plume treatment and looking at finer nesting to look at and see how we can resolve these relationships when we've come into these types of problems.

So, in the end, our goal is to have a really sound and useful modeling guidance for people to go out with when we start doing single source assessments for PM2.5 and ozone and so I think that all this work that you're going to be hearing about after

me is going to feed into that, including the things that I just showed you.

**MR. BRIDGERS:** All right, Ralph.

**MR. MORRIS:** Thanks, George. I'm Ralph Morris with Environ. I think I've heard my name several times here, so it's good to be up here defending myself.

Anyway, I'm going to talk about the long range transport evaluation using the Tracer test. This report has been up in SCRAM for over a month. It's only 260 pages, so I'm sure you've all read it by now. I did provide a summary. The 35 pages is actually the summary, so if you want to read that. Anyway, it's been up there for a while. It documents dispersion modeling that EPA and mainly Bret Anderson has been doing over the last three or four years. It's hard to say. Anyway, it's a lot of work. We got all the results and QAed them and documented them and did some interpretations, but we didn't do any of the runs. I usually don't like to do this, because it's hard enough to explain the stuff that I do and also I probably would have done things a little differently. But we did QA the results and document them in this tome.

So, in their Tracer tests, and with this crowd I probably don't need to do this, but you release

a known amount of inert material that has a zero background or as close to zero background as possible from the source and then you measure it downwind. So, we're measuring the transport and dispersion. There's no chemistry. There's no deposition. So, you're actually evaluating the long range transport model's ability to treat this transport dispersion.

In the early 60s and 70s, we had lots of great dispersion tests using radioactive materials and that's how we did all of our -- a lot of our dispersion curves. That's frowned upon these days. It was good while it lasted.

So, an example of the past Tracer experiments are as the 1986 eight model study. I think Tyler mentioned that and that's a study Argonne National Lab did. I was one of the reviewers of that study. I was only 12 at the time, but I was very into reviewing.

Rocky Mountain was one I worked on and then this 1988 EPA evaluation was the CALPUFF using two of the experiments and that actually fed into the IWAQM recommendation of CALPUFF as long range transport model.

1994 European Tracer experiment is the one that I'll be talking about. That was, they had a

real time experiment where they used Tracers and people got forecast deals and tried to simulate and then they had a retrospective ATMES two where they went back and did it again and that experiment was actually spawned by another experiment called Chernobyl which they weren't ready for.

So, CALMET using the 1988 experiment because part of the objective here was to see how CALPUFF had changed from the 1998 Tracer test evaluation. They used the Oklahoma Great Plains 1980 database that had ARCs at 100 kilometers and 600 kilometers and the Savannah River Lab SO6 database that had an ARC at 100 kilometers. So, these are ARCs or receptors. And at that time, the 1998 study and this study using the new version of CALPUFF used a fitted Gaussian plume evaluation technique where you take the observations along the ARC of receptors. They'd be like 20 observations and fit a Gaussian plume on there and you look at things like the maximum ops on the ARC or the maximum fitted plume center line because the receptor may not have picked up the maximum concentration. And you can look at plume widths and cross wind integrated. And then you can do the same thing with the model predictions where you may have 100 receptors. And you can also look at timing statistics

of how long is the Tracer on the ARC and when did it get there and when did it leave.

One of the findings that we had here was that for this long wind distance, like a 600 kilometers ARC, assuming that the Tracer has a Gaussian plume along the ARC may not be true because there's wind shear and stuff and so we found out, at least in the Savannah River Lab that that was a poor fit and it could be misleading.

So, I think you need to look at this. Look at how the fit and the observations occur and then see whether it's working because this is not -- it doesn't always work.

So, we revised the Tracer test evaluations that we documented. It was for the two historical Savannah River Lab, a GP 80 that had ARC receptors experiments, but also the 1983 Cross Appalachian Tracer experiment. They had five releases and I'll talk more and then the 1994 European Tracer experiment. That had more, lots of receptors out there.

And the idea, at least for the Savannah River Lab GP 80 was to compare the current version of CALPUFF, which was 5.8, with the older version, CALPUFF four and what's changed and whether some new techniques

like puff splitting has improved CALPUFF's performance.

And then for some experiments, maybe the CAPTEX and the ETEX compared multiple model configurations like I have on the right.

Originally, my talk was going to try to talk about everything, but people want to leave here before eight, so I'm just going to talk about CTEX3 and everything is in the report. So, it's been up there for a while.

So, the CAPTEX experiment had five releases during 1983 and they were released from either Dayton, Ohio and Sudbury. Actually, EPA modeled two of them. CTEX3, October 2nd, 1983 from Dayton, Ohio. And then CTEX5 from Sudbury. And Dayton, Ohio is where my Cal Bears play tonight in the play-in game for the NCAA, so that brings a little relevance.

So, in doing this we did some met or a met model was evaluated first like we talked about yesterday and so there are multiple CALMET configurations. We'll talk about that.

And one of the things, EPA's objective for doing multiple model evaluation is identify the best performing CALMET configurations that kind of fed into their EPA OFM recommendations. And then CTEX5 also had the MM5 evaluated in there. And then for

CAPTEX, we also evaluated multiple models and I'll talk about that at the end.

Okay. For CTEX3, there are 31 CALMET sensitivity tests and what was varied were things like the MM5 model that was put into CALMET where there was 80 kilometers. Actually, it was MM4 for CTEX3. 36 kilometers MM5 or 12 kilometers MM5. That was put in. And then how the MM5 was used by CALMET because you could use it different ways. As a first guess field which is the usual way of using it. As a step one wind fields which means you skip the diagnostic effects and feed into the CALMET step two procedures that does an objective analysis of the observations. CALMET was also ran with no MM5. CALMET was also run with different grid resolutions; 18, 12, and 4 kilometers.

And there was how did the observations get blended into the step one wind fields and that's controlled partly by this RMAX R1 procedures where there a three A, B, and C different values for RMAX R1 would be the 100 kilometers for the surface winds and 200 kilometers winds. That's the distances that are used to blend into the field being the EPA FLM recommended settings from their August 2009 Clarification Memo. And then the D is not to use any observations.

We also had three MMIF sensitivity tests. So, where the MM5 data at the 36, 12, and 4 kilometers are processed with a minor interpolation from the -- to get from C to D or B to C way. That has to be done just because of the way the models are formulated.

And then CALMET STAT was used to evaluation wind speed and direction and for the CALMET runs we just did it for winds.

And then down the bottom which you guys can't see are some benchmarks that we typically compare our mesoscale model performance with.

So, this is the wind speed direction bias. I'm just showing one. There's 31 tests -- CALMET tests. Three MMIF tests. So this is just showing the ones with the MM5 that was run with 12 kilometer because we didn't seem to see, if you get to finer MM5 fields, this is up to 12 kilometers, we got better wind performance which is a good thing because we're paying a lot of money for something.

And this one uses a CALMET resolution of 12 and four, so the experiments fours are 12 kilometers resolution as experiment six are four kilometers resolutions.

And then the one on the far right is the

MMIF 12 kilometers met which -- MMIF mostly doing a pass through, for the most part, of MM5 winds. It's kind of the MM5 performance.

And if you look at the top left at the wind speed bias, you can see that the ones that are shaded are the ones that are the B series. The RMAX1 RMAX2 which is what the EPA FLM recommended. And you can see for the wind speed bias there, they also under-prediction, but the under-prediction for the B series, quote B series, EPA FLM recommended has the lowest bias. Yay. Did something right.

And then on the wind directions on the right, the bias, again, the lowest bias, the highest bias is from the MMIF, i.e. the MM5 data coming in and then the use of CALMET actually reduced the bias in the wind direction with the lowest wind direction bias being the A and the B series.

And then on the bottom is the error. I know it's hard to see, I was sitting in the back. It's hard to see and you've got all these smart people sitting up front and they got big heads and it's hard to see through them.

You can't see the bottom, but the B series is performing the best and if you look at the wind direction bias, the MMIF or rather the MM5 model

actually exceeds the benchmark at 30 degrees. It's not a pass-fail grade, but it's, you know, the CALPUFF is doing it's intended thing of making the winds match at the observation sites better than what it got from MM5.

So, conclusions from the CTEX CALMET model performance, I skipped a lot of runs here, but the recommended settings that EPA Federal Land Managers came up with in the May 2009 Clarification Memo produced the best wind model performance. And then using a four kilometers grid resolution, CALMET tended to produce better wind performance than using the 12 or 18 kilometers. I can't say anything about less than four kilometers because we didn't run anything, a model, on that.

And then using MM5 data with higher resolution, we didn't see a huge difference between 12 and 36, but that is definitely better than the 80 kilometers MM4 data.

And then this is CTEX4. CTEX5 also found that the EPA recommended RMAX1 RMAX2 settings did produce the best wind performance.

But I do want to caveat in a way at the bottom which no one can see, but this is not an independent evaluation. The same wind data that goes in the evaluation database is also used as input to

CALPUFF. Excuse me, CALMET. So, it's not an independent evaluation.

Moving onto the CALPUFF CALMET sensitivity tests. In this case, CALPUFF was evaluated using 25 CALMET sensitivity tests and it was evaluated using these ATMES II Sysco performance metrics and the question is which CALMET -- CALPUFF CALMET configuration gave you the best Tracer performance?

CALPUFF was also run using three MMIF configurations corresponding to MM5 grid resolution of 36, 12, and four. These ATMES Sysco performance measures, there's 12 of them. They look at the spatial performance, temporal performance, the global performance and they conclude measures of bias and error, scatter, correlation, cumulative distribution and a couple of the ones down the bottom I just gave four of them that I'm going to show here.

There are four of those spatial ones that count as, like, forecast measures. Like if you configured a mirror up in space that looks at the overlap of the predicted cloud with the observed cloud divided by the union of those two clouds. So, what percent of the overlap -- things like, normalized meets squared error, you know, the error, people are familiar with that. Correlation coefficient, again, people are

familiar with that where a perfect correlation is one. A fractional bias, again, it's a bias. You want to get zero. And then Kolmogorov-Smirnov parameter which is looking at the cumulative frequency distributions of the two prediction's observations and how their maximum difference.

We also looked at these composite cisco ranking models. One is a parameter called rank that Roland Draxler came up with that combines four parameters of correlations, bias, the figure mirror, the spatial parameter, and then the cumulative distribution and he normalizes them so they each get ranked one, so a perfect model will get a four.

One of the things we found is that the fractional bias is probably not a very good metric to have in this composite because you can have a model that creates 100 or a minus 100 and have a zero bias, but it's not performing very well. You know, it's off. So it could have compensating errors and so I think we're looking to revise this and maybe put an error statistic in there that more -- and you don't have this compensating error. We used rank in the report.

The other thing that we did was we looked at all of the statistics and averaged across the 11 statistics as 12 -- it ranked as a 12 and see how

that changes because the models will perform differently for different statistics.

And this is the CALPUFF model performance for the statistics. I guess the same test I showed before using the 12 kilometers MM5 and CALMET. 12 and four kilometers CALMET resolution and those A, B, C, D version of RMAX1 RMAX2 and for the figure merit and space, higher metrics are better and it looks like the experiment four A pro forma MMIF is performing best.

If you look at the mean squared error on the right, you want to get lower numbers and experiment four C is the lowest, they're all pretty close, but I'm going to go to the fractional bias. There again, you want zero. And they're all fairly close. And you finally get to the rank metric where we want a perfect -- higher score is better and one of the things we noticed is that the B series is not performing as well it was given to be for the -- as we thought for the met model comparison.

So the evaluation conclusions and this includes -- one of the things I found was that the CALPUFF MMIF with CTEX3 was the best performance configuration, outperforming all the other CALPUFF CALMET configurations, but then you go to the CTEX5 and

it was the worst performance configuration. So, it depends on your application and, but consistently across the CTEX3 and CTEX5 we found out that the B series the EPA FLM recommendations appear to be the worst performing values for RMAX1 RMAX2 and that's in contrast to what we saw in the met model. And then the difference RMAX1 and RMAX2 configurations, the A series, seemed to perform best, followed by C and not using any ops performed worse. And then the, as I mentioned, CALPUFF using CALMET with a higher than five resolution performed better than generally CALPUFF. Using CALMET with a four kilometers grid performed better using the higher resolution.

So, one of the things we came up with here is that you can't just go by met model evaluation when you incorporate met observations into the met model.

Also for CAPTEX3 experiment, there were six long range transport models that were then compared and these were evaluated using, again, the ATMES II statistical metrics. There were two Lagrangian models. It would be CALPUFF with MMIF, not CALMET. Two Lagrangian models. CALMET and SCIPUFF. And then CALMET was run with MMIF. In this case it was the best performing CALPUFF configuration.

Two Lagrangian particle models, FLEXPART and HYSPLIT and then two Eulerian grid models, CAMX and CALGRID. They're photochemical grid models, but they run no photochemistry and no deposition.

These are performance statistics where the best performing model has the lowest value. And you look at false alarm rate and that's how many times the model said you had Tracer at a monitor that was none. So, how many false alarms, these are kind of forecast statistics. In that case, FLEXPART was performing best and maybe CAMX second best.

Go to fractional bias and again, FLEXPART is performing best and HYSPLIT is performing worse. Kolmogorov-Smirnov test, the frequency distribution, well HYSPLIT is performing best and FLEXPART and CALGRID are performing worst. It depends on which statistic you look at and normalized mean squared error, by far, HYSPLIT is performing the worst and FLEXPART is performing best. So, it depends on which stat you're looking at.

And then looking at performance statistics where the best performing model -- well, you want to get the highest. You want to 100 percent. For figure, mirror, and space, the spatial statistics, CAMX is performing best and then CALGRID is performing

worst. And then on the right, it looks like SCIPUFF and CAMX.

And then on the correlation coefficient, lower left, HYSPLIT actually is negative correlated with the Tracer concentrations and CAMX and SCIPUFF are the highest positive correlated.

And then for the composite rank statistics, it's clear that CAMX and SCIPUFF are the highest models, followed by it looks like CALPUFF and FLEXPART with HYSPLIT and CALGRID being the lowest.

So, in conclusions, try to get us back on track here -- I know it's late. The GP80 Tracer field experiments, we're using this different valid or reasonable CALMET configurations. The maximum CALPUFF concentrations vary by a factor of three, so you can get a factor of three variation just by varying your inputs to CALMET. And since you have less options in MMIF, meaning the pass through options, it varied by, I think it's plus or minus 20 to 30 percent.

One of the things that we found is that in order to reproduce the, quote, good performance of the 1998 EPA study on the 600 kilometers ARC of the GP80, we need to use the slug near-field option which they used in that 1998 study. Slug is a way to represent a continuous source near the plume and why it

was used for a 600 kilometers ARC which I guess may be near the source on a global model, but is unclear to me, but that's the way they ran it. We ran it without the slug and we couldn't reproduce the time of the Tracer on the 600 kilometers ARC, but when it was turned on, it was able to. So that was kind of a head scratcher.

As I mentioned earlier, the Savannah River test. 75 Tracer. The fitted Gaussian plume observations were very poor and thus we couldn't say anything about modeled evaluation because the model evaluation paradigm was broken for that experiment.

And the CAPTEX field -- brute force CTEX3 and CTEX5, we found that RMAX1 equal 100, 200 kilometers, i.e. the FLM recommendation from the 2009 Clarification Memo -- it produced the best CALMET wind speed wind direction performance but the worst CALPUFF Tracer performance.

So, keeping all of the CAPTEX experiment with MMIF as a driver to CALPUFF, it performed better than CALPUFF CALMET for the CTEX3, but was worse in CTEX5. You know, as usual, it's the atmosphere. You can't say anything with certainty.

And then for CTEX3, you look at the multi-model. You know, SCIPUFF and CAMX were the best

performing with CTEX3, followed by CALPUFF and FLEXPART with HYSPLIT and CALGRID the worst.

Then CTEX5 is CAMX and HYSPLIT were the best performing, followed by SCIPUFF FLEXPART and CALPUFF CALGRID the worst, so there wasn't always consistency.

The ETEX Tracer experiment which I think, from looking at these three years ago at this meeting, the CAMX, the HYSPLIT, and the SCIPUFF were performing the best and FLEXPART and CALPUFF were performing the worst. I think we'll hear more about the ETEX tomorrow.

So, with that, I'll open up for questions.

Or I don't. We've got questions at the end of the thing. Nevermind.

**MR. BRIDGERS:** Ralph gets the gold star award for getting us absolutely back on track.

While Bret is coming up to the podium, I'll make a request for all of the presenters on tomorrow's public session, at the present I have 50 percent of the presentations for tomorrow, so it would be very helpful if at least the ones that are in the morning first thing, if I could get those before sometime this evening.

Thank you.

**MR. ANDERSON:** Thank you. Bret Anderson. U.S.D.A. Forest Service.

The outline of my talk this afternoon will be another element of this project that Tyler and Kirk outlined and before we begin with that, I'd like to discuss the use of air quality models, you know, for both NAAQS and air quality values under NEPA which is a major driver in this project. Then we'll discuss the design elements of the EPA FLM single source model evaluation project as it relates to the consequence analysis that Kirk referred to. And then to examine some of the initial results of that evaluation. Also, a part of this is, as we move forward into the future is kind of an evaluation of the practical considerations of the use of some of these your learning models more routinely in the regulatory framework because part of this project is also to look at the comparison of the resource requirements associated with that to kind of inform the public.

So, as I had mentioned, the NEPA requirements and the FLMs, we have a very distinct motivation here in this. And because prior to about 2005, 2006 time frame, air quality modeling for NEPA projects was fairly well-defined. So, basically, you

had to do your standard near-field analysis and also your far-field analysis so you had to deal with local scale NAAQS and you had to deal with the air quality related values and you had analyze, you know, each option -- each resource management option relative to each of those. And then, thanks to our buddies in the State of Wyoming and their deployment of monitors we started to find some problems. And in particular, what we found was we found, you know, I'm sure you've seen many articles about these, but basically we found winter ozone in the Upper Green River Basin in Wyoming and the Uintah Basin in Utah. And now we have to deal with addressing ozone air quality modeling for pollutants that occurred in times of the year and in remote locations that were once considered to be urban scale and summertime problems.

So the paradigm in NEPA modeling shifted and so I speak somewhat on the behalf of the both the Bureau of Land Management and the U.S. Forest Service because we are unique among the land management agencies that we are multi-use agencies and that we're responsible for developing environmental impact statements for any resource management decisions that are made on Federal lands and so, typically, when you see the BLM doing an EIS for a resource development,

you'll see a companion document coming out from the Forest Service because many of our lands are adjacent to one another.

So for air quality, this means that we now have to analyze potential impacts to local -0- to both local and regional air quality for each resource management option that is considered. This was translated into running AERMOD, CALPUFF, and CAMX. And so the complexity and the cost associated with meeting these needs and air quality analyses under NEPA have grown considerably in the last five years.

In response to this, and this is what Tyler highlighted in his presentation is that the Department of Agriculture, the Department of Interior, and EPA entered into a memorandum of understanding outlining generally agreed upon principles for conducting air quality related analyses under NEPA for energy development projects.

And so the general principles are to establish an agreed upon procedure for conducting air quality analyses. Having a development of a formal stakeholder process for input and how the modeling analyses area done. And then also dispute resolution procedures.

And one of the major themes of this is

to reduce the cost to both the development agencies, the agencies that are responsible for the development of these NEPA documents as well as the project proponents through the promotion of modeling techniques which allow for the leveraging of existing analyses to the extent that they're practical.

This means that we're trying to reduce the burden in the modeling requirements and this occurs through two approaches.

The first approach is to establish what we refer to as a reusable modeling framework which are regional scale air quality analyses that can bracket, you know, development potential in a given air shed that can be leveraged to help describe, you know, the potential impacts from any individual development project and; two is to promote the use of a single modeling platform where practical to deal with issues of both ozone, PM2.5 and then the subsequent issues related to air quality values which are visibility and deposition for this NEPA.

And so you can see, we have quite a bit of a resource burden here in terms of the modeling requirements and so we have a very strong interest in the evaluation of these various platforms.

So, where do the EPA and the FLMs go

from here? So, the NEPA requirements in the Sierra Club petition necessitated that both the EPA and the FLMs reassess the suitability of the existing modeling paradigm in order to address these issues. In order to address these needs, the EPA and the FLMs undertook this project to compare the model predictions of existing models and emerging models to understand both the predicted impacts for resource management decisions and to better understand the resource requirements and challenges to implementing these.

So the EPA had a work order, a multi-task work order, with Environ and under task order six there was the single source LRT demonstration project. And so the first element would be to apply a long range transport chemical dispersion models, for example; test sources as one would for both a PSD far-field class one assessment which is essentially the same thing that you would have to do for NEPA in describing each resource management option impact. And so for this, they used a 2005 and 2006 annual simulation and this configuration, what you see here, is that CALPUFF and CALMET and CALPUFF and MMIF were examined and then also using CAMX for source apportionment capabilities.

The other thing was to compare the air quality and air quality related value metrics for class

one areas across all of these different ones. And so this is essentially -- this is not a performance analysis. This is simply a consequence analysis to look at what the predictions are looking like for each one of these types of things.

And so thank you, Ralph, for outing me so I'm outing him here. This presentation just documents the transport dispersion model simulations that were performed and done by Environ. So, turnabout is fair play. It's like in 2011.

So, basically, what you see is that Environ used two separate modeling domains here and so they used it -- for 2005 they used what we call the four corners air quality task force project and that had a 12, you can see it's a region 12 and a sub-region 4 kilometers domain that's focused over the four corners area in the southwestern United States.

And then you'll see here, you'll see the 12 kilometers domain and, I don't know, is this for Peyonce Basin or is this just a --Uintah Ralph?

**MR. MORRIS:** Uintah.

**MR. ANDERSON:** Uintah. So anyway, this is the 2006 domain. You know, this is purely just 12 kilometers domain here.

So, for this evaluation, for the 2005 four corners air quality task force modeling database that was used, they looked at five EGU sources which are identified in the top figure and nine oil and gas point sources -- or point and area sources that are identified in the bottom figure.

For the 2006 study, they identified -- they modeled 13 EGUs and then 11 oil and gas, you know, corresponding the same figures as in the previous slide. And this is going to be and I'm going to talk a little bit about this, but there is a lot more to explain in terms of, you know, going into the report and actually looking at this because there is just an awful lot here and these images are going to be pretty difficult in the back. But basically, what you see here is -- all this is is a head-to-head consequence analysis looking at the concentration predictions or the deposition value or the visibility analysis predictions of each modeling system compared to each other modeling system.

So, in the left figure for top and bottom which is annual NO2 at the top and sulphur dioxide on the bottom is that on the left you see CALPUFF and CALMET configured with CALMET according to the EPA FLM recommended settings and what you --

compared to CAMX. So, CAMX is on the left axis and CALPUFF CALMET is on the right axis and so you can see that CAMX has slightly higher predictions than CAMX for nitrogen dioxide for the annual standard.

And then when you compare in the center one where you have CALPUFF MMIF on the left axis and CALPUFF CALMET configuration on the right axis, you see that the CALMET is slightly predicting higher relative to CALPUFF MMIF and you see a similar behavior for both SO2 on the bottom, you know, in this and the fact that you see CAMX has slightly grading or slightly higher predictions for SO2 compared to CALPUFF CALMET.

And when you compare CALPUFF MMIF to CALPUFF CALMET in that, on the bottom here, what you see is there is a slight edge towards over, you know, CALMET predicting, you know, the CALPUFF CALMET configuration predicting higher.

And then likewise for, you know, here on the left when you're looking at CAMX on the left and CALPUFF MMIF. CAMX has that same tendency to predict higher than the CALPUFF MMIF configuration.

So, here, and I'm going to quit rambling through these here, but I just encourage you to go look at the report. Basically, what you see is, again, just a consequence analysis and it's informative to look at

each one. How these stack up against one another here.

So, I'm going to slide through this and, now, the one thing you see here is in the PM10, where you're looking at it, because we have different issues with species mapping from the secondaries that are coming out of CAMX compared to what is in CALPUFF, the values are going to be significantly different as a result of that and so that, you know, that's the important take-home message when you're examining the results of the PM10 analysis.

And then for annual SO2 in the 2006, you can see here -- you'll see some very high CALPUFF CALMET and MMIF outliers here that we'll get into examining that just a little bit closer. You can see, for the most part, at the lower concentrations they do -- there is fairly strong parity and -- but when you see them in the center image here, you'll see that there's a significant outlier here that was with CALPUFF and CALMET.

And then, again, you'll see this. You'll see something similar with this as far as the outliers go with the CALPUFF CALMET and the CALPUFF MMIF.

So, basically Environ did take the opportunity to go back and take a look at the outlier

in a little bit greater detail and so basically what they found was the maximum annual NO2 and SO2 by CALPUFF CALMET occurs only for the smallest of the 12 EGUs and in that 13 EGU scenario for the 2006 study. And this occurred within the Holy Cross Wilderness which is a class two area. So, it's likely that they were co-located with receptors.

The maximum CALMET, MMIF, and CAMX annual NO2 concentrations were 3.1, .6, and .02 respectively and so the -- we understand why CALMET was much higher than CAMX because CAMX was configured for LRT application with a 12 kilometers grid, but why are MMIF and CALMET so different?

And so they were looking at the wind fields here and this is just a snapshot using the CALDESK software to look at the MM5 winds at 12 kilometers overlaid with the CALMET vectors here and so you can see that with CALMET, which I believe is the blue, you can see that at the surface, both the surface and aloft, CALMET is showing that there is much more response to the terrain in the CALMET wind field as compared to what the MM5 shows which you would hope to expect running CALMET at a higher resolution there.

And then likewise, when you run it aloft and you look at aloft, you'll see that CALMET still

shows significantly more response to the terrain than the MM5 data does at that resolution. So, you can see that there are, especially in this area of the domain over here, you can see significant variation because you can start to see more of a transition to more of a zonal flow which is more indicative of synoptic scale flow and so it's not -- the course of resolution and prognostic data is not responding as much to the terrain as what the finer scale CALMET winds would do.

So, looking at that outlier again. CALMET was modifying and slowing the MM5 winds and it occurs both at the surface and aloft. And their conjecture -- they are unsure as to whether or not it's the diagnostic effects or the objective analysis procedure doing this. It occurs throughout the year and it results in very high concentrations for the CALPUFF CALMET configuration and, but as they indicate here, you get much better agreement between the CAMX and the CALPUFF CALMET configuration at all of these other sites.

So, then we get into visibility which is where we're very interested in here. And as you can see, the visibility has a very similar thing here where you see, basically, the CALPUFF and CALMET configuration and CALPUFF and MMIF are yielding higher

results than CAMX, you know, in terms of the visibility impacts.

Now, there is an explanation -- a partial explanation for this and it's because when Environ ran CALPUFF initially, they used the old IWAQM default of ten parts per billion background ammonia, so it's going to affect both the particulate nitrate estimates you get and then also the total nitrogen deposition that you get because one model will carry the species around more as nitric acid which is, you know, probably much more susceptible to wet deposition in that as opposed to particulate nitrate. So, again, but you can see that there are significant differences in the visibility configuration there -- the visibility predictions as a result of the various configurations.

And so one other thing that we had Environ look at was to look at the spatial variability across the receptors because one of the primary concerns that the Federal Land Managers have is that, you know, if you understand how visibility modeling is conducted, we deploy discreet receptors within the boundaries of the class one areas at a routine interval of about 1 kilometer. And the reason why we do that is we're interested in picking up the inhomogeneity of the wind field for lack of a better term. Basically, what

we're looking at is we're looking to see -- we're concerned about the spatial gradient of the concentrations and also the spatial gradient of the deposition and visibility and so one of the concerns that there is is how applicable is a grid model to visibility estimates or deposition estimates? Do we see a high degree of variability across the predictions in distant class one areas?

And so we asked Environ to go in and look at the distribution of the visibility impacts. And what you see is that four far-field receptors, so when you look at each one of these class one areas and I think this is the 2005 Four Corners, right? But basically what you see is is that there is very little, much tighter distribution across the receptors within the class one areas or these further ones except for the nearest class one area which is the Mesa Verde one.

And so this is a very important piece of information that you see here. The Eulerian models in close proximity to the class one areas account for much less spatial variability as compared to either the CALPUFF or the CALMET or the MMIF configurations for that and that's what the information that we were trying to drive at was is that for -- when you're doing LRT assessments and you're looking at source receptor

distances that, you know, are not 200, 300 kilometers beyond but are within 50 to 75 to 100 kilometers is that is there a spatial gradient in those concentrations and the answer from this is yes, there is very much a spatial gradient there. And that was information that we were very concerned about and has some considerations as we move forward is the suitability of these types of models for different source receptor distances.

So, looking a nitrogen deposition here and this is what I was talking about earlier was is that and this is what you see is that CAMX has higher, a factor of two higher, estimate of deposition than CALPUFF. And the CALPUFF MMIF nitrogen deposition is just slightly greater than that off CALPUFF CALMET. And then you'll see something similar, but to a lesser degree with sulphur deposition. Basically, what you see is CAMX predicts slightly higher sulphur deposition relative to CALPUFF and, again, MMIF has slightly higher sulphur deposition predictions relative to CALPUFF CALMET except for one point. And the results from this are similar to what they were from the 2006.

And so Environ went in to look and see why is CAMX estimating much higher deposition than CALPUFF? And one of the first reasons is that because

they used different species mapping with CAMX, so there's more nitrogen species and only ammonium from the source and CALPUFF included ammonia assuming both the full sulfate and the nitrate are neutralized.

So, they performed CAMX species mapping according to CALPUFF rules. And, Ralph, would you care to explain this a little bit further?

**MR. MORRIS:** I guess using the CALPUFF species mapping where you assume nitrate and sulfate would be completely neutralized by ammonium, it kind of went the wrong direction.

I mean, it gave you more nitrogen than CAMX and so maybe it will over-estimate a lot worse higher nitrogen deposition in CAMX greater. And so it didn't explain it. The species mappings and the extra species did not, in itself, explain it.

**MR. ANDERSON:** So they basically look at the fact that -- what they concluded was that CAMX was carrying much more nitric acid in the model as compared to CALPUFF. CALPUFF is carrying much more nitrogen in the form of particulate nitrate.

Again, bear in mind that this analysis will be redone because they initially did this evaluation looking at CALPUFF configured with the old IWAQM ten part per billion recommendation. So, they

were going to be redoing it with, I believe we said it was one part per billion. It was, I think that was what the FLMs recommended is one part per billion here and that's much more in line with what you see from monitoring studies in high terrain out in the west like at Dinosaur National Monument and stuff. So, there will be another revised set of CALPUFF runs with more realistic background ammonia values and so this will be part of it, but you'll also see additional CALPUFF runs with much more realistic ammonia background.

So, I did want to take a little bit of time to talk about some of the practical considerations associated with this and these are kind of, as you can tell, we are interested in looking at our Eulerian models or a higher order Lagrangian chemical model such as SCICHEM or if they're reactive particle models. If those are -- what type of challenges would they impose on the modeling community? Like if, for example, as Tyler was talking about from the FLM side, we try to -- we're relying on the EPA to provide a guideline model for us, even though applications of the guideline model for air quality related values technically don't fall under EPA's regulations. We try to stay in concert with EPA as much as possibly can and so we're very sensitive to shifts in modeling technology that would

be adopted by EPA because we try to adopt a more uniform approach to things with EPA.

And so there are some practical barriers to implementation when you look at the use of these higher order models. And one of these things is that the dispersion modeling community that is represented here, most of your work is done on Windows platforms. And especially for state agencies. And, unfortunately, for most of the agencies, that means when you have to do these large, annual simulations for photochemical models, the Windows systems are typically designed in such a way that you can only do serial applications of runs which would me if you're doing the standard way for doing an annual PM2.5 run for photochemical models is typically to break it up by quarter. And run them across multiple machines.

Well, on a Windows environment, that's not possible. So, as you can see is the disconnect is both meteorological and photochemical models are largely Unix and Linux based platforms. And also we take a look at the time that's involved with running photochemical grid model simulations compared to the current models that are used under the AQRV analyses.

So the computational considerations here is is that if the community moves to these higher order

models is that -- is it necessary to adapt these platforms to adapt to a Windows based environment which may sound for the SIP modelers and the meteorological modelers, that may sound like a very strange concept, but bear in mind the fact that the vast majority of the community that uses these models -- will be using these models in the permitting arena are Windows based people.

So that creates two problems. One is that the level of fluency for Unix and Linux operating systems in the dispersion modeling community is typically much less and they typically aren't as fluent in programming skills as they are in the Linux and the Unix environment with the SIP model because we have to do much more data manipulation and a lot more custom programming that way.

And then the other issue that we run into is the fact that the IT authorities within the states and local permitting agencies often lack the familiarity with and the resources to dedicate to systems administration for Unix and Linux based systems. And thus, they actively present -- in my case, when I was a state modeler, they absolutely refused to allow us to acquire any Linux equipment. And if you were given the privilege of buying Linux

equipment, it was a stand-alone machine. It would not sit on the network. They absolutely forbade it to be on the network.

So, from a barrier to implementation from a regulatory perspective, basically what you have now is this is the clash of the worlds that George talked about here. And this is where the clash really begins. It's that for permit modeling, the operational construct for permit modeling is a highly rigid set and it's based upon a series of regulations and guidelines that generally restrict operational flexibility in order to promote more general consistency in the application of models.

The operational construct for meteorological and photochemical modeling is vastly different. Because those are based upon a more loosely binding set of EPA recommendations which typically encourage the adaptation of both the science and the modeling techniques to produce the most scientifically feasible answer given the constraints of the state of the science.

So, from the regulatory considerations, these two differences in the operational paradigms between the two communities will require both the EPA and the FLMs to develop a more rigid set of operational

procedures similar to both the current permit modeling paradigm in order to ensure that a scientifically sound and consistent set of procedures prevents an anything goes process which would likely develop without such procedures.

Additionally, like what you see in the NEPA context, we typically only do one year's worth of evaluations, but when you see the AQRV requirements under PSD or with the near-field analysis, it's three and five.

So, if you go down this route of doing this, the length of meteorological record for the photochemical models will likely have to be expanded to be consistent with the requirements with the guideline on air quality models.

And then finally the -- and this is EPA policy here so I won't get into this, but you know the old issue of significance thresholds for single sources which is a hornet's nest that I'm not going to touch here.

And so in conclusion, basically, what we have found from this study is that photochemical grid models are capable of assessing single source impacts for both AQRVs and ozone requirements if there would be any for PSD. The source apportionment techniques that

Kirk talked about earlier eliminate the need for multiple zero out runs or the brute force techniques. However, there are, as the community thinks about this and moving forward, is there are significant barriers that remain to implementation of these and these come in terms of both training requirements for staff, computational requirements, and then also the creation of a suitable regulatory framework that can accommodate the requirements of permitting but respect the fact that you're dealing with a much more scientifically robust system.

**MR. BRIDGERS:** Jim Kelly.

**MR. KELLY:** Okay. So some of the previous speakers have pointed out that a need exists for a single source models that can accurately simulate secondary PM2.5 and ozone formation.

The SCICHEM reactive plume model is one that could potentially be used in single source applications where consideration of secondary pollutants is required. However, before the model can really be applied widely in regulatory applications, it's important that we can thoroughly test the model under conditions relevant to those regulatory applications. It's important that we can thoroughly test the model under conditions relative to those

regulatory applications.

So, today I'll talk about a very preliminary study which is our first implementation here at EPA of the SCICHEM modeling. Basically, we'll simulate the plume from the TVA Cumberland Power Plant for a day in July of 1999 and then compare predictions with simulations from CMAQ and some observations to kind of get ourselves familiar with the performance of this model.

So SCICHEM stands for the Second Order Closure Integrated Puff Model with Chemistry. The plume is represented by numerous puffs that are advected and dispersed independently according to the local meteorology. The name comes, in part, from its use of the second order closure parameterization for integrating the turbulent diffusion equation. So, the dispersion rate is related to turbulent concentration fluxes which are saw through that parameterization.

The model has some puff merging and splitting capabilities which can be important under inhomogeneous meteorological conditions. And what's important to us is that it holds the promise of potentially being able to give us comprehensive simulations of chemical process as well as gas aerosol as well as aqueous phases.

So, as I mentioned, we've just started to implement this model here and we started by simulating the Tennessee Valley Cumberland Power Plant for a day in July of 1999. Now, the reason that we picked this up to begin with is that it's a well-studied episode and so as we start to learn about the behavior of this model which is new to us, we wanted to benchmark our results against some simulations that had come prior to us.

Also, this is a pretty data rich period because there was a helicopter that flew transects through the plume downwind of this power plant and measured some suite of chemical species and so we can use those measurements to help us evaluate and understand the performance of the model.

This gives an overview of the model configuration that we used in the study. And basically, I'll talk about the results of two simulations with SCICHEM that we've named SCICHEM-WRF and SCICHEM DIAG.

In the SCICHEM-WRF case, the meteorological used to drive the SCICHEM model is based on WRF version 3.3 output that's been converted to MEDOC format using the MMIF tool that Bret talked about.

For this simulation, the background concentrations that we're using are time varying three dimensional hourly varying concentrations that we took from CMAQ photochemical grid model simulations which were converted into a format that SCICHEM could read using a tool developed in-house.

The SCICHEM DIAG simulation is based on meteorology that uses observations from four met stations and this met was provided to us as part of a test case that we received with a pre-released version of SCICHEM that EPRI was gracious to allow us to use. The background concentrations for this case are set to constant values and we're using, pretty much, the default values associated with this test case, although we adjusted ozone by a few ppb and SO2 as well to be more in line with the background measurements. We're using hourly emissions that were based on CEM data in this simulations.

We also conducted some CMAQ simulations for this period and so CMAQ is a three dimensional photochemical Eulerian grid model. And the reason we applied CMAQ is that this is something that we're very comfortable and familiar with in our group. And so, as we learn about the behavior of this SCICHEM model, which is new to us, it helps us to understand the

performance of that model if we can look at it through this lens of CMAQ which we know pretty well.

So, we did some simulations with CMAQ version 4.7.1. And these simulations were done at four kilometers resolution shown in this inner domain down here. Two points to mention is that the WRF meteorology that drove the CMAQ model was the same meteorology that was used in the SCICHEM-WRF case that we simulated. And the emissions from the TVA power plant for the SCICHEM simulation -- or for the CMAQ simulation was the same as that used in the SCICHEM simulations.

So, we'll get started by I'll show you some figures comparing absolute concentration predictions of CMAQ and the SCICHEM-WRF case. And basically what we'll do is we'll overlay SCICHEM-WRF predictions at receptor rings onto CMAQ concentration fields.

So, this shows a plot for SO2 concentration. Just to orient you here, what we have is this point in the center is the TVA power plant. And this is basically, these three rings that you see around the power plant correspond to our receptor rings that we placed in the SCICHEM simulation. So, the colors inside those rings correspond to SCICHEM

predictions and the colors outside those rings correspond to concentrations from the CMAQs. We're overlaying SCICHEM and these rings overtop of the CMAQ field.

And there's three plots here which are at layer one of the CMAQ model at hours of ten in the morning, one p.m., and six p.m. later in the afternoon. Now, if we look at this quickly and just qualitatively, we see that the plume direction for the SCICHEM simulation overlaps the plume for CMAQ pretty well here and later into the afternoon. And so, this is kind of reassuring because both models were driven by the same meteorology, so it's nice to see that they put the plume roughly in the same location. That wasn't guaranteed necessarily.

Another thing we see is that possibly near the source, SCICHEM is predicting high concentrations of SO2 than CMAQ. That's something that we'll look into in terms of dilution rates and, you know, puff versus grid based averaging. We need to look into some of those differences.

One of the things that's kind of puzzling us right now, as I mentioned, is this is our first implementation. Away from the plume, you see these grey colors in the SCICHEM simulation. And as I

mentioned earlier, in this simulation, we fed the SCICHEM model three D, time varying ambient background concentrations from the CMAQ simulation. So, we expected that, away from the plume, down here in these rings, we would get back the same values that we had in our CMAQ simulation, but we don't see that and so it's something we'll look into.

We had similar behavior for NOx. These are NOx concentration plots and we see kind of a similar thing where the direction of the plume is the same largely between SCICHEM and CMAQ and that transition from the morning to afternoon happens in the same way, but it looks like SCICHEM is predicting higher concentration of NOx near the source at layer one and, again, we'll look into this further.

Now, the second set of figures I'll show you will attempt to just compare plume concentrations for the different simulations. So, what we did was we did a set of simulations where we set the emission from the TVA plant to zero and then we got predictions from that simulation and subtracted them from the simulations where the TVA emissions were accounted for. And then sort of the difference between the zero out and the base case simulation should help us isolate the impact of the plume. And I'll show you some figures

for that now.

So this shows the SO2 concentration difference between the base case and our zero out run. And up top, these are the same scenarios that we've just seen. So this is comparing CMAQ predictions and the SCICHEM-WRF case that we simulated. Again, the plume directions are pretty similar.

Down on the bottom row, I'm not sure everyone can see it, but this is comparing the CMAQ and the SCICHEM DIAG case which, just to remind you, just used the observation based meteorology. So, especially in the morning hours, if we look at the direction of the plume when we use the observation based meteorology, it differs from the case with the WRF prognostic model meteorology.

As the afternoon wears on, then the directions kind of align a little bit better, but you still see here's some red down -- pointing that there is some southward trajectory of this plume in this case that we're not seeing up here with the WRF case. And so, not surprisingly, if you use different met fields, it has some consequence.

These are similar results for NOx. Because of the time, I won't through a lot of the details, but the same differences apply here.

And finally, this is a plot of ozone. And so, we mentioned at the beginning that we're interested in SCICHEM because we're interested in photochemistry and secondary pollutants and so this is the first example I've show you we're really starting to look at that.

And so, just to orient you, the blue colors in this figure represent cases where when you add TVA emissions into the simulation, it results in reductions of ozone concentrations. Those are the blue colors. Whereas the red colors show when you introduce the plume emissions to the simulation, it leads to a net production of ozone.

So, if you think over here these blue colors very close to the source, that represents a situation where the plume is very concentrated in NO and that NON will react with the ozone molecule and kind of destroy ozone. But as we move further downwind, some background ambient air mixes in and that contains radicals and VOCs and so, under those conditions, NOx is a net producer of ozone. And so we see, moving from the source where we have titration of ozone downwind, there's some production, we're capturing these qualitative features that we'd expect. Although we see for the CMAQ simulation, the reds are a

lot more prominent. I don't know how well you can see. Within the ring of the SCICHEM simulation, the ozone production downwind was a little bit less and we're going to look into that.

This shows just a surface concentration Tracer plot that compares on it -- the left, the SCICHEM-WRF simulation and on the right the SCICHEM DIAG simulation which used the observation based meteorology. And as I've already pointed out, in the morning hour, this is nine local standard time, there's a pretty big difference in the direction of the plume, but then later in the afternoon, the plume directions are a little bit more in line.

Okay, so as I mentioned, one of the purposes of simulating this episode is some helicopter observations were available for this time period. So this Google map shown here shows the TVA Cumberland Power Plant and then these black transects at distances downwind of the power plant is where that helicopter flew through and sampled the plume coming from the power plant. And the average altitude of these aircraft transects was 500 meters. And so now we'll compare some of our predictions with these helicopter observations.

So this shows the SO2 measurements for

the plume transects. So the plot on the left is the observed SO2. The plot in the middle is the CMAQ model predicted SO2 concentration. And on the right is the results of this SCICHEM-WRF simulation.

So, we see in the observations you have, near the source, these bright red colors which indicate that we have a fairly narrow plume of concentrated SO2. And if we move downwind, the concentrations are going down, presumably because of some dilution and this plume broadens a bit.

For the CMAQ case, we have similar qualitative behavior where you see the red colors for the plume near the source and then the concentrations just diminished a bit as we move downwind. We would want to look into this a little bit more quantitatively to give a full assessments, but those patterns are there.

And we see a similar trend with the SCICHEM-WRF case, but for the SCICHEM and CMAQ, it appears that maybe the plume is moving a little bit to the south compared to the observations in the beginning.

This shows some similar results of NOx which I don't want to dwell on because the patterns are pretty similar to the case I just showed you for SO2

and then finally we have ozone.

So you see in the aircraft observations that near the source there's some blues in here indicating that within the plume, we're having a net destruction of ozone. So ozone is falling below its background value in the plume, but then downwind, now we really start to see these deep reds where the NOx from this power plant is leading to some net ozone production.

Similar trends that you see here with CMAQ again. There's some blues near the source and then there's some transitions to these redder colors down here which might indicate some ozone production. But again, we want to look at this more quantitatively.

SCICHEM has similar behavior where you have some ozone titration near the source and downwind we saw less ozone production with SCICHEM, but we saw some.

Now the final set of plots I'll show has to do with comparing what I'll call centered profiles for the observations to the model and I'll explain that now.

So, what we did is we searched along these receptor ARCs that we used for the SCICHEM simulation and we find the maximum -- the location of

the maximum. And similarly, we search along the aircraft transects and find the maximum. And then we sort of look at the profiles and we center them all to zero at the maximum.

So, we're saying here, okay, if there is some angular displacement from the models that doesn't exactly track the plume, let's try to eliminate that from our evaluation by matching up the peaks and centering them at zero. That's what we've done here. And we have the radius of 11 kilometers, 31 kilometers, and 65 downwind of the power plant and this is all an elevation of 500 meters.

So, what we see with the black points is the observations and, you know, they're pretty narrow with a sharp spike near the source, but then as we move downwind, this peak comes down a little bit and the distribution broadens.

The curves that you see for the two SCICHEM simulations similarly follow this trend. They start out with a narrow plume with a large peak and then this diminishes a bit as we go downwind and there's some dilution of background air mixing in. But really the striking thing about the figure is that there's a pretty big difference in concentrations predictions between the SCICHEM WARM case and the

SCICHEM DIAG case. This is likely related to some differences in dilution and mixing or maybe plume placement and it's something that we plan to look into further. Right now we don't have a full explanation.

Similar behavior for the NOx concentration where we see similar trends. In this case, actually, the SCICHEM DIAG has actually over-predicted the observations of NOx at this far away distance which is one of the cases where that happens.

And then finally, ozone. So, if you look at ozone and for the black points and the observations, again, you see this titration near the source and you see this minimum in ozone. Then, as you move a little bit downwind to this radius of 31 kilometers, you still see this minimum here, but now you start to see ozone production near the edges. So, what's going on is that the edge of the plume, some background air is mixing in and that might have some proxy radicals and VOCs that allows the NOx in the plume to then become a net producer of ozone and you start to get some ozone production around the edges.

And then when you move all the way downwind, these two wings that they call them, ozone wings, start to approach each other from both sides and you still have a minimum here in the middle.

Now the model simulations, they capture the -- we can be critical of this, but this is just a preliminary application. But we capture this trend. We get ozone titration near the source and then as we move downwind, the models aren't getting these ozone wings, but I think there are certain configurations in SCICHEM where you can try to capture these features that we haven't really implemented here.

But then we move to the final distance. We kind of flat line with our ozone in both SCICHEM simulations. Part of that might be related to that we didn't resolve these ozone wings here, so we moved downwind, maybe we don't get that. It could also be related to our background ambient concentrations. Perhaps they're low in VOCs and radicals. We haven't really looked into this because a lot of the work has just gone into setting up the model and conducting the runs.

So, just to summarize, we developed some preliminary tools for using SCICHEM in terms of processing model inputs and outputs. And so much of the work that we've done today has focused really on getting the model inputs set-up, running the model, and extracting outputs so we can make the figures that we saw today. We haven't focused too much on the

chemistry and the details just yet.

So, as I mentioned, we simulated this TVA Cumberland Power Plant with SCICHEM and CMAQ version 4.7 and considering that we haven't spent much time on the model configuration or gotten into he details, we think we have some reasonable model behavior in the sense that within the plume, we have high SO2 and NOx concentrations. These profiles broaden and go down further from the source due to dilution. We see ozone titration in our plume for the SCICHEM simulations which is what we'd expect. And some qualitative similarities between SCICHEM and CMAQ predictions we found. But clearly there is some next steps.

The next thing we're going to look at is exploring the impact of different treatments of ambient background concentrations. They might play a role in this ozone production issue I just mentioned. And then once we get that issue addressed, we'd like to look at NOx oxidation products, NOz in particular. So, there's measurements of that which I haven't talked about. And also we'd like to look at vertical profiles. So, some of the difference between those models may have been related to the different models putting the plume in different vertical regions which I haven't really

accounted for in the comparisons. So, we'd like to look into that.

Another thing is we'd like to consider additional plume observation studies. In particular, we might look at a nighttime case where we have very stable meteorological conditions and see how well the models perform under these stable nighttime conditions compared to this daytime case here.

As Kirk mentioned earlier, we waiting for this CMAQ advanced plume treatment model that's planned to be delivered sometime in the future and when we get that model, it has a sub-grid scale reactive plume treatment and we're eager to see how that performs for these types of simulations.

And then finally, we want to move away once we can test and get some familiarity with the models, we'd like to simulate longer time periods in larger domains and start moving to more regulatory relevant applications and testing the models under those conditions.

And so we're just getting up to speed here, I'd like to acknowledge the people who have helped us out getting set-up and providing the SCICHEM model to us.

Thanks.

**MR. BRIDGERS:** Home stretch.

Ralph?

**MR. MORRIS:** I'm glad everybody is sticking around for the ozone presentation. There will be a test on the chemistry afterwards.

Anyway, this is something completely different. We talked about a screening methodology for single source ozone and it was developed for Sydney, Australia. I'm going to present it, but the work was done at Environ. It was headed up by Greg Yarwood, Edward Tai, Prakash Karamchandani and sponsored by the New South Wales Office of Environment and Heritage. We also had our folks at Environ Australia participate and since this was a screening model and you have to come up with some screening thresholds as to what's significant or not, the New South Wales Office of Environment and Heritage was also involved in that because we don't set significance thresholds.

I'm going to talk about the motivation. Why they wanted to do this. 3D modeling of new source ozone impacts. The development of the screen tool. That's technical work there. The framework for evaluation ozone impacts. So, that involves technical and policy considerations. What is a significant ozone threshold? I don't decide that. That's for EPA and

these folks and I'll summarize it.

Okay, so the city greater metropolitan area region, GMR, exceeds the applicable ozone standards which in Australia is one hour average of 100 ppb. Our old one hour was 120. And a four hour average of 80. Our current eight hour is 75. So, they do things a little differently down there.

They decide attainment, areas of attainment is less than 82 percent of the standard. Not 80. Not 84. 82 percent of the standard. So, if your one hour ozone above 82 ppb, then that's attainment. I don't know what the reasons are for it, but it's to protect the most sensitive kangaroo with a margin of safety.

Anyway, they define things differently, but they need a method to quantify ozone impacts from ozone sources. We talked about photochemical models for ozone is preferred approach across the world. But it's very resource intensive. Bret talked about that. They need a technically sound screening tool to evaluate which new sources require -- are likely too small to require a more comprehensive photochemical model application. So, that's where it started.

When the Australian office got the RFP and sent it to us, the guys in the bottle that do this

stuff that they don't understand, and they talked about well, we need to develop something like the Scheffe Tables. I think people in this group know what the Scheffe Tables are. They were developed by Dr. Scheffe in 1988 using your active plume model to come up with the VOC NOx screening for ozone increments. Six months later, he completely disavowed ever developing them and it was only years later that people came back to say, you know, Rich, they're using -- can you explain these Scheffe Tables. And he immediately started writing letters saying, don't use it -- don't use it -- don't use it. And so, they mentioned that and now we actually had some of those letters in our proposal's appendix saying, don't use this approach.

So the methodology was that we first reviewed literature, identified defensible methods since EPA hasn't put ozone in AERMOD yet, picked photochemical modeling.

Roger, you're supposed to laugh.

We modeled several prototype new sources using the photochemical grid model. In this case, we used CAMX for reasons I'll explain in a second. And then we used social city method which is a higher order, decoupled direct method and CAMX developed a parametric model of the prototypical source impacts.

The ozone pack of the source was a function of the source's NOx emissions, VOC emissions, and the source location.

And then we implemented this parametric equation in a spreadsheet, an Excel spreadsheet. It can run on Windows. And that's developed a screening tool. And then with the New South Wales folks, they developed criteria to evaluate the impacts using a tiered approach where if you can show the emissions are too small to cause an impact or that this level one screening uses parametric approach is less than a significance threshold. So, they pick -- then you're presumed not to have a significant impact and you can build your new source. Whereas, if it goes above their significance thresholds, then you need to go back, go and do a full blown photochemical model application what the ozone impacts of that source will be.

Okay, so this is the Sydney Greater Metropolitan Area. The domain is a three kilometers domain. 25 verticals up to 8,000 meters. Two episodes were run. December 2003 and January 2004. That's two months' episodes. That's Australia so summer is in winter or however it works. The other episode is December 2004 and January 2005.

I used CAMX version 5.3. You see the 05

chemistry. The meteorology comes from the Australian model. Met model and air pollution model called TAPM. The air pollution model is what it stands for. That may be presumptuous of me.

The emissions are the anthropogenic emissions from the Office of Environment and Heritage and then we ran the MEGAN, the biometric emissions model, to get the biogenic emissions. The boundary conditions for that domain was from the Mozart Global Chemistry Model.

To look at these sources, we divided up the area into these five different locations and since we had to pick a source location for those five locations, we used the emissions weighted centroid of each one of these five locations. The five locations is Newcastle, I think in the North. Wollongong in the south. Sydney and for Sydney we had an east, west, central, and west. And those little blue dots are the emissions weighted centroids of each of those areas. And then we looked for the nearest industrial area to that location because in some case they ended up being over the water and we figured that wasn't likely.

For each one of these prototypical sources, we had combined VOC NOx emissions of 500 tonnes per annum. Tonnes are metric tons and annum is

a year. It's metric tons per year. And we had a VOC NOx emissions ratio that is 1.24 and then the stack parameters we picked to essentially release the -- have minimum plume rise, release these emission source near the ground to get maximum ozone impact.

Okay. So, high order B coupled direct method is a sensitivity method that's one of the probing tools in CAMX and it's like the D coupled direct method only it has higher order terms to describe the effects of VOC and NOx on ozone. You think about DDM, you think it's like Taylor's theorem of linear -- you have a curve of ozone that's linear approximation, but with these higher order terms we are able to fit the parameters and so the bottom shows that the change in ozone is a function of all of these derivatives of S1 through S2. First order and second order derivatives and X -- the equations are NOx emissions and Y is the VOC emissions. So, you run each DDM on each one of these prototypical sources and you get this parametric equation to describe it.

And the way that's done is we look at all of the days in those two month periods and pick high ozone formation days that satisfy certain model performance requirements and then we pick the highest sensitivity coefficients from those days for each one

of those sources. So, you're getting kind of a worst case day to get this ozone increment.

So, doing this we evaluated the parametric equation by doing brute force runs where we run the model with different sources and we use sources at the same strength, 500 tonnes per annum, and then we did ten times that and then 25 times that because with the sensitivity tests, as you go away from what you ran, your model may deviate from the brute force and so one times NOx and VOC, you'll see ten times, and then 25 times.

So, we use that to evaluate it and the example here is a HDDM using a parametric equation for a source in one of the domains on the left. And then the brute force we did zero out run for the Central Sydney source and you see the location there on the right. You see the pattern matches fairly well for the 500 tons per year source. Maximum is .23 ppb versus brute force of .25. Then, even when multiplied by 25, these parametric equations are developed under a 500 ton per year source, multiply it -- we put in 25 times the emissions and we get values that are also fairly close patterns are very close to using the brute force.

So, we're trying to look at what is the range of this parametric equation? How high of

emissions is it still valid? We kind of say that 25 times it still looks valid.

And then here's another way of looking at the valuation of the parametric equation of the increment of ozone by HDDM on the left axis and then the brute force ozone on the right axis. Looking at a combine VOC NOx sources, this is ten times the initial 500 tons emissions. An only NOx source in the middle. Then only VOC source. It's not surprising that these parametric equations work better with VOCs than NOx since NOx chemistry is much more complicated.

So, we had fairly good agreement and the differences are within about five percent of what brute force gives us in parametric equation up to about 25 times the original source that was done.

So, the fact was that -- they're considering for evaluating ozone impacts is what's the magnitude of the source impact? Is the source located in the ozone monitoring attainment area? And then sources, of course, must satisfy all of the regulatory requirements which I don't know what they are in Australia, but I'm sure they're -- they have other criteria.

So, the preliminary results of the tests in the New South Wales Office has developed a

significant impact level where if they do this level one screening and the source impact is less than .5 ppb, they figure it's not measurable and so you satisfy the level one screening analysis and you're done. You can build your source as long as you satisfy everything else. And then there is a maximum allowable impact level. And for non-attainment areas they picked one ppb. So, if you're above .5 ppb but less than one ppb, you satisfy your requirement.

And for attainment areas, they looked at the difference between the maximum ozone is to the standard and you got, you know, you've got -- you blow the standard by so much and you get 25 percent of that to use up by that one source. So, if the standard is 80 ppb and the background ozone is currently 60 ppb, you've got 20 ppb to go and you get 25 percent of that and so you get a 5 ppb. So, if your source's level one screening is less than 5 ppb, then you can build the source.

So, if the source impact is below the maximum impact level, you can build. Level one analysis satisfied and you can do it. If you go above it, then you need to go to level two which involves doing a site specific photochemical model application of that particular source using a model like CAMX or

CAFM or CMAQ.

Okay, I'm not sure if you can see this, but -- so you start up here and there is two pathways. One is for attainment areas. One is for non-attainment areas. The first thing you do is look at the emissions of the source. If the emission is less than certain thresholds, the NOx emissions plus VOC emissions is less than a certain threshold, then you're off. If your attainment area is less than 90 tons per year, then you just need to go to the best model practice.

The second screening is the level one screening with your less than .5 ppb deciview. And then you move it. You keep on moving down and you do your level one screening. Are you above the maximum impact level for the attainment area? If you do, then you keep moving down and then you're eventually do that and you don't satisfy any of these things, you move on to the level two screening which is a full blown photochemical model application. But I don't think you can see any of that because I can't see it.

The screening tool is in a spreadsheet and what you do is you input your source inputs as a fraction of the nominal source prototype and you tell it whether it's an attainment area or non-attainment area and you give it the baseline ozone and it tells

you what your maximum allowable increment is. This is a test source that was applied for the original prototype source because the emissions are the NOx and VOC are the same as the test source. Then you press the go button and it gives you your increment. In this case it's .74 ppb which is less than the maximum increment back level of 7.8, so this source would pass the level one screening and therefore you would not have to go to the level two and do a full blow photochemical model application.

Now, for the -- was also ran HDDM getting sensitivity coefficients out for each of the individual VOC species. And the one below I showed had defaults speciation which is kind of the default overall sources. Here, you can put in all the species -- the explicit species of your source and this is the way they want you to run it. And it calculates the sensitivity coefficients for VOC individual species because there is a wide range of reactivity for these sources and so a lot of alkanes or something is one thing. If you have a lot of alkenes, it could be totally different and make a lot of ozone.

This is an example on the bottom of the different reactivity coefficients where you can see the 05 species. You put in all the individual species.

You see the 05 species and you can see the actual things like alkenes; ethane, olefin, isoprene are much higher than other species like alkanes like paraffin and some species like toluene have a negative ozone reactivity. Increasing toluene actually decrease ozone. These reactivities actually are tailored towards each one of those five locations that we have. The Newcastle, Wollongong, and the three Sydneys. So, the speciation reactivity is different in those areas. So, you have locational specific reactivity information.

So, the summary is that we developed a sufficient screening method for screening tools for Sydney. It's very location specific. It's using a location specific photochemical model application. We divided up into sub-regions based on where we think the chemistry would be different. We think it's scientifically defensible and robust. We have, like I said, location specific speciation VOC sensitivities and we find the range of applicability. I think I didn't mention that flowchart because I couldn't read it, but if your emissions are higher than that 25 times that source -- test source, then you immediately drop down to level two to the explicit application of the photochemical model. So, there's a range of

applicability of the screening. So, if the emissions are really high, you've got to run the model for that source.

It's suitable for smaller sources. That's what we're focusing on. And then you use your resources, your photochemical modeling tool to focus on the big sources. There's no reason you can't apply this general technique to other regions. Of course, other jurisdictions will want to have other screening and significant impact levels and maximum achievable impact levels. But you have to redo the photochemical model run with HDDM specific for that region and come up with new coefficients.

So, anyway, the framework is there. It's developed for local standards. It's not yet finalized. I think if -- the idea of what a significant ozone impact is something that they're wrestling with. I think we're doing the same thing here. But there is no reason why it can't just be adapted for new locations.

So, I'm done.

**MR. BRIDGERS:** Thanks, Ralph, for that presentation.

**MR. FOX:** So, I would just like to thank all the speakers today and yesterday. We've had a lot

of information flowing. Hopefully, you all stuck through the first two days. We have the public session tomorrow and so I would just say that as a reminder of the process and the like that I talked about this morning, all I wanted to do is just reinforce that given all the information flow and the like that your comments are critically important in terms of providing us with information about what you've seen.

I'd also like to emphasize that given the constraints that we all have, if there's any priority that you see in terms of the items that are of most concern to you; either your industry, your particular interests, and the like. That would be very useful to get comments for us to get in this context of this conference.

As I said, we will be summarizing those public comments and either providing our internal priorities or a reflection of the priorities that we saw in those comments from you all and others.

The comment period has been extended or will be extended until the end of April. So that will hopefully facilitate a review of a lot of the information that has been provided. We recognize that there has been quite a bit and the reports are now up on SCRAM and the presentations that were provided in

these two days as well as the reports that are still due up on SCRAM by the end of the month or early April. Hopefully, there is sufficient time to provide those comments.

In the context of the PM2.5 guidance and even in other situations, that doesn't slam the door, hopefully, and we do expect to continue to use existing avenues of communication and coordination with you all so that there can be a continued evolution of the digesting and information sharing and further coordination on activities and the like.

I recognize that there may be some things that you all have seen and hear here that you may want to be a priority and not just a priority in the context of updating Appendix W, but also maybe providing updated guidance and the like. So, to the extent that there are those types of items that would be very important to know so that as we engage in the next steps after this, we're looking at a two-pronged attack in terms of looking at the feasibility and suitability of taking some of these things and improving upon the existing guidance as well as looking down the road and what we need to do to get where we'll need to be in time for the 11th Modeling Conference.

And I would just also reinforce the

suggestion of a specialty conference or other types of avenues for us to get together either broadly as a community or in specific sectors or certain interests in late 2012 or early 2013 so that we can continue to have the dialogue and actually reserve time for probably more detailed discussions at those times with planned activities in the interim such that we can then deliver results and compare things at those times.

So, we're looking forward to engaging with you all throughout this process. We're committed to undertaking both in new guidance where appropriate and looking forward to modifications to Appendix W. I think in a lot of the evaluations will be critical. The screening tools and other types of approaches, you know, just as a reminder, you know, updating Appendix W to address chemistry and the like. We're going to have to be pragmatic in looking at that and doing that and provide techniques or tools, short of full scale modeling in some cases. That may be a viable option for use and so please consider those things as well.

I think we're going to have a question and answer session so, I will turn that over.

**MR. BRIDGERS:** If we could get the speakers back up here from the late afternoon session, not that we need the slide, but it's there. So, Bret

and Tom and Kirk and Jim.

And again, if you could announce who you are.

**AUDIENCE MEMBER:** Mark Bennett with CH2M HILL.

This is not so much a comment, but a -- or not so much a question as a comment in response to something that Tyler just said in terms of specialty conference and to continue cooperation and coordination between the modeling groups and the regulated community and the EPA. So, I'm going to represent in this comment as chair of AB-3, Committee of Air and Wind Management Association.

We're having a meeting at the Courtyard Marriott directly after this to talk about specialty conference and coordination. Any AB-3 members are welcome to attend. See me if you didn't get the email and anybody who is really interested in becoming an AB-3 member, maybe we can do something about it and make you an honorary one in the meantime.

**AUDIENCE MEMBER:** Are we allowed to bring beer?

**MR. BENNETT:** I have the liquid refreshments covered.

**AUDIENCE MEMBER:** I am Biswanath

Chowdhury from Sage Management and we are developers of SCICHEM, so I just wanted to comment on Jim Kelly's on ozone rings.

So, you can get the ozone rings, but the default that is a splitting criteria for the puffs, so if you change the criteria then you'll get a lot more puffs and then it resolves the ozone wings. I just wanted to comment on that.

**MR. KELLY:** Would that configuration cause problems if we wanted to do long annual time simulations?

**MR. CHOWDHURY:** Yes. If you have more number of puffs, then it takes longer to run.

**MR. KELLY:** But for these specialized aircraft campaigns, that would be the way to go?

**MR. CHOWDHURY:** Yes. And Nouri Galani from Union City, Alabama. He did a study. He did work on ozone rings using SCICHEM.

**MR. KELLY:** Thanks.

**AUDIENCE MEMBER:** Bob Paine, AECOM. I have a question for Ralph.

On the long range transport evaluation, the long range transport models now are typically used for class one assessments for distances 50 kilometers and up and I noticed that the models were all compared

to much further distances and the Great Plains and Savannah River Lab were not used for the model inter-comparisons and I was wondering why those were omitted?

**MR. MORRIS:** Well, I was limited to what runs were done and EPA did plenty of runs. There's no shortage of runs, but those experiments were limited to just two ARCs of receptors and they were focusing on looking at the changes in CALPUFF performance from the 1998 study to current study. The other two studies have a much higher density of receptors and for the ETEX, we did look at the -- we were concerned about the long distance because we were looking at distances of 500 or 1,000 kilometers downwind in addition to close to the source. When they did look at model performance close in and the evolution of the statistics, some, and they didn't change that much. The models didn't perform better closer to the source.

I think the main thing was just resource restraints.

**MR. ANDERSON:** I can probably add since I was the perpetrator of the problem.

There's a number of practical considerations. The first one is that -- and Joe, please tell me if you did get all that, I believe there actually were model inter-comparisons on those other

experiments, at least for the Great Plains Tracer experiment.

Ralph will tell you that in the flurry to try to get report together, we didn't have time to compile all the results and everything else, but I do believe that there were comparisons of all of the various models that were involved in the six study or the six model comparison.

For Great Plains Tracer experiment, the first problem is that it's my judgment that those two experiments, there is limitations with each of those. I think the Great Plains Tracer experiment, I think, is a very useful one, but the Savannah River one, I really question the usefulness of it. That's the first thing there.

The second aspect of it is that while there are results for the Great Plains Tracer experiment, one of the things is that we have to go back and do diagnostics on it because the approach that EPA took in 1998 was to create a 12 hour integrated average across that six, the 100 -- or in the case of the 100 kilometers ARC, it was a three hour integrated average and for the 600 kilometers ARC, it was a 12 hour integrated average. Actually, when you go back and you look at the performance or you go back and look

through the actual observational database, what you'll find is that there is an unexplained second occurrence that occurs. Essentially where the Tracer cloud apparently shows up at somewhere 24 to 36 hours after the release, shows back up on the 600 kilometers ARC. So, there is a question there about all of the models were evaluated against that first transit across the ARC, but none of the models were including CAMX were able to pick up that second one. As a result of that, we needed to go back and look at to see whether or not the positioning of the 12 because when I did the MM5 modeling for this one, it was initialized within EPRI analysis data and so those that are familiar with that it's a two and a half degree by two and a half degree every six hours.

The difficult is that you don't want to take something that starts at a two and a half degree and next go right down to a 36 kilometers. So, standard procedure was to start with a 108, then go to a 36, then go to a 12.

We were concerned about the fact that when you're using a single nest to represent the meteorological field, whether or not that 12 kilometers domain was large enough to capture what apparently looks like the recirculation on that and so none of the

models on the Great Plains Tracer experiment, when you look at it from the ATMEs framework where you weren't looking at that 12 hour integrated average, never picked up that second wave of Tracer. It was like an ordered magnitude smaller than the first wave that crossed the ARC, but it raised questions as to whether or not the 12 kilometers was sufficient, whether it was sufficiently large to capture if there was recirculation occurring. So, that was something that we do need to go back and look at.

**MR. PAINE:** One other quick comment.

The ratings of the models were like this model won and this model lost, but the Mississippi Primary last night, I think the candidates were within one percent of each other, so were they significantly different? Were the models significantly different? I have no idea.

**MR. ANDERSON:** And I think you need to take a step back and look at it -- you know Kirk made a very good point which is while we have presented presentations or information at prior conferences, this is a work in progress.

For example, the issue that Ralph mentioned with respect to use of fractional bias as one of the indicators in there, you know, where you get

that. What is developed -- when we did the evaluation paradigm, for example. When we tried to look at an evaluation paradigm, we did look at what tried to make the most sense given, as Tyler was talking about, the fit for purpose type of evaluation paradigm, but the issue becomes is that when you take somebody else's method off the shelf and you look at it and you try to use it within the context of a regulatory evaluation, not everything works out quite the same because the things that they've placed as an emphasis on those four broad categories and the metric that they used to represent each of those four broad categories may not be sufficient for that purpose. For a regulatory purpose.

Like we were saying, for regulatory application of a model as an example, the issue of fractional bias is we are concerned in a regulatory capacity, you're concerned that the model is -- that there isn't a systematic bias towards under-prediction and as a result of that fractional bias, when you look at the way that it was used within that rank metric, it takes the -- it takes the one minus the absolute value of fractional bias divided by two. So, it treats over- and under-prediction as the same. As a result of that, it's not a good -- for regulatory purposes, it's not a

good fit.

We learned about this after the fact. So, for example, we started going back and looking at a lot more of the work that, you know, Steve Hanna and Joe Chang have done with the boot statistics and looking at fractional bias false-positive, fractional bias false-negative, and then to start looking at confidence intervals to look at the statistically significance between those and so this is a work in progress.

So, you know, I think the point was in Tyler's presentation was the fact that there were a lot of lessons learned in the prior evaluations and one of them was that if you go back and you look at the results coming out of the Rocky Mountain Acid Deposition Model Project, basically what they did was they took all the models that were involved in that first eight model study that EPA did back in 86 and then they just weighted -- you've got issues of data organization. Pairing in time and space. Not pairing in time and space. They looked across each of the statistical categories and the data pairing and weighted them the same.

So, in that case, and I think, you know, Mesopuff and Mesopuff II were the models that were

involved. I think Joe probably remembers this because you were heavily involved in that. Mesopuff II was the best performing model for purposes of unpaired in space and time and ARM3, Ralph's model, was the best performing model for purposes of pairing in time and space and they were ranking 21 and 20.

The problem became then the fit for purpose which is, okay, and so that question is is that because you use for long range transport assessments for AQRVs in class one, you're very concerned about space time pairing. And so weighting the two the same in that context for regulatory applications -- when you're trying to decide which one is best for regulatory purposes, didn't do that.

So, this is a work in progress in trying to establish a paradigm that makes sense where no paradigm has existed and so we certainly -- I certainly hope that through the comment process during this period that if there are concerns about what was done, that those be brought forward, but bear in mind that that is a work in progress.

**AUDIENCE MEMBER:** This is Ron Lai from BOEM. I have a question for Ralph.

I looked at the met. It is 20 percent over the ocean.

**MR. MORRIS:** I didn't understand the question.

**MR. LAI:** I looked at the Australia study. The met. 20 percent is across the ocean. Is there a special approach to modify with respect to the ocean or no?

**MR. MORRIS:** Well, as part of the sub-domains do go over the ocean and you might see high ozone over the ocean because of the reduced mixing. So, if the plume travels over the ocean, then we do account for that.

**MR. LAI:** How do you account for that?

**MR. MORRIS:** Well, it's part of the modeling domain.

**MR. LAI:** The model, the meteorology accounts for that?

**MR. MORRIS:** The TAPM model? The TAPM model, the meteorological driver is a fairly sophisticated prognostic model that incorporates observations if I remember right. I don't know how they ran it in this study.

**MR. LAI:** Would you just use the meteorology model for the driver?

**MR. MORRIS:** Yes. We didn't run the met model.

**AUDIENCE MEMBER:** My name is Rick Graw and I'm with the U.S. Forest Service Air Quality Program.

I have just a few comments to make as we start thinking about emerging models and techniques from the view of the U.S. Forest Service.

One has to do with ozone. As we start thinking about the continued development of ozone models and how we might apply them perhaps beyond just a short term period in the urban areas, considering that a lot of the ozone metrics that the Forest Service uses are based upon other metrics that might be a sigmoidal weighted average over time for exposure and we have a number of additional metrics that aren't currently being considered in the output of these ozone models that I think would be really helpful for us and the regional modelers to help identify areas and see where the injury to vegetation is occurring as we continue to develop our understanding of air pollution effects on the ecosystems.

So, that's a request issue. I'll move forward with the future development of these tools.

Second has to do with deposition. Another request is I believe we're at a point and have been for quite some time where we could at least ARC

graphic clouds modeled fairly accurately in these models and cloud water deposition is of concern to the Forest Service and again, that's not currently being considered and I would like to see if or at least plant the seed so-to-speak in terms of consideration of including cloud water deposition into the future model developments.

Thank you.

**MR. BRIDGERS:** Thanks, Rick.

Other questions? This is the last chance because tomorrow it's all comment.

Well, I thank everybody again for sticking around this afternoon. If you stay around the campus too long, the gate over at Hobson Road closes at six, but I don't think that's going to be much of a problem right now.

Thank you again. I think we owe the speakers another round of applause. We will start at eight-thirty in the morning.

Have a pleasant evening.

(WHEREUPON, the conference was concluded.)

CAPTION

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

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

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