GOVERNMENT OF THE UNITED STATES

ENVIRONMENTAL PROTECTION AGENCY

SECOND CONFERENCE ON AIR QUALITY MODELING

MONDAY, AUGUST 10, 1981

MORNING SESSION

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The conference was held in the Thomas Jefferson Auditorium, South Agriculture Building, 14th Street and Independence Avenue, S. W., Washington, D. C., Joseph Tikvart, Chief, Source Receptor Analysis Branch, Conference Chairman, presiding.

PRESENT:

JOSEPH TIKVART	Chairman
RICHARD RHOADS	Panel Member
JAMES DICKE	Panel Member
G. THOMAS HELMS	Panel Member

ALSO PRESENT:

Environmental Protection Agency

WILLIAM COX	Source Receptor Analysis Branch
THOMAS CURRAN	Monitoring and Reports Branch
BRUCE JORDAN	Chief, Ambient Standards Branch
BERNARD STEIGERWALD	Director, Office of Regional Programs
EDWARD F. TUERK	Director, Office of Program Management Operation
BRUCE TURNER	Chief, Environment Operations Branch

TRC Environmental Consultants

NORMAN BOWNE	Vice President

Systems Applications, Inc.

C. SHEPHERD BURTON	Vice President

Schwartz and Conolly

STEPHEN CONNOLLY	President

Environmental Research and Technology, Inc.

BRUCE EGAN	Vice President

Forest Service

DOUGLAS FOX	Chief Meteorologist, Rocky Mountain Forest and Range Experiment Station

Hunton and Williams

LEWIS KONTNIK

ALSO PRESENT (CONT'D.)

Pennsylvania Department of Environmental Resources

JAMES SALVAGGIO — Chief, Air Quality Planning Section

Los Alamos National Laboratory

MICHAEL D. WILLIAMS

Mobil Research and Development Corporation

STEVEN WISE — Reserach Department

Federal Aviation Administration

N. SUNDATARAMAN

Federal Highway Administration

HOWARD JONGEDYK
WILLIAM CARPENTER

National Oceanic and Atmospheric Administration

JEROME HEFTTER
ROLAND DRAKLER

U. S. Geological Survey

JOHN GOLL

Nuclear Regulator Commission

EARL MARKEE, JR.

National Park Service

DONALD HENDERSON

ALSO PRESENT (CONT'D.)

Department of Energy

ROGER SHULL

State of Texas, Department of Transportation

RODNEY MOE

Oak Ridge National Laboratory

ALAN WITTEN

Alcoa

ROBERT KOHM

Salt River Project

DONALD MOON

Meteorology Committee (TT-3), APCA

JERRY PELL

Utility Air Regulatory Group

RAY WRIGHT

Dames and Moore

RALPH SHRADER

Others

MR. MISHRA
RICHARD TENT
MITCHELL WORNBRAND
PHILLIP YOUNGBLOOD

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CEJ

P R O C E E D I N G S

(8:58 a.m.)

MR. RHOADS: Good Morning.

If you'll all take your seats, we'll begin this conference. In fact, we'll begin it, even though there are some people coming in the door.

My name is Dick Rhoads.

I must first announce a slight change in published agenda. We had intended to open with a formal welcoming address. Unfortunately, Kathleen Bennett, who will be our new Assistant Administrator for Air, Noise, and Radiation, in EPA, had to be in Ann Arbor today.

And Ed Tuerk, the current, acting Assistant Administrator, had a las minute schedule conflict.

So in lieu of a formal address, let me, on behalf of the United States Environmental Protection Agency, welcome all of you to the Second Triannual National Conference on Air Quality Modeling.

That's quite a title isn't it, Joe. The Second Triannual National Conference on Air Quality Modeling.

I am pleased to recognize a wide diversity of organizations participating in this Conference.

I see representatives of several other Federal Government agencies, State governments, local governments, National Science Foundation.

I believe we have representatives from both the United States Senate and House of Representatives staffs. As well as representatives of industry, many consulting groups, several universities, and the scientific community as a whole.

And, possible, we should call this an international conference, because I notice the Ministry of Environment of ONtario is represented.

I am sure that this wide diversity of views will be of great value to the Conference.

I, now, want to introduce Mr. Joseph A. Tikvart, who is our Conference Chairman.

Joe is Chief of EPS's Source Receptor Analysis Brânch, with officer in Research Trianagle Park, or Durham, North Carolina.

And in case any of your are wondering, Source Receptor Analysis is our gobbledygook term for air quality modeling.

Joe will first provide us with some general information on the conduct of the Conference.

Joe Tikvart.

MR. TIKVART: Good morning.

I am Joseph Tikvart, Chief of the Source Receptor Analysis Branch of the Office of Air Quality Planning and Standards. I will be your Chairman for this

Conference on Air Quality Modeling.

Participating with me on the hearing panel, to solicit your view, and take your contents are Richard Rhoads, who you've just me, who is Director of the Monitoring and Data Analysis Division; Tom Helms, Chief of the Control Program Operations Branch; and James Dicke, of the Techniques Evaluations Section; all of the Office of Air Quality Planning and Standards.

I also would like to thank you for accepting our invitation to attend this Conference.

The dates and the purpose of this Conference were announced in the Federal Register on June 12th, 1981.

I am looking forward to the generation and exchange of many ideas over the next several days.

This Conference is being held in response to requirements of Section 320 of the Clean Air Act. A Conference on air quality modeling is required at three-year intervals to help standardize and improve modeling practices within air pollution control programs, such as prevention of significant deterioration.

The Conference held in December of 1977 addressed EPA's "Interim Modeling Guideline." This year's Conference will focus on a major problem in the utilization of air quality models for regulatory applications. That is, given the accuracy of existing models, how should

decision makers incorporate uncertainty in their regulatory programs.

This year's topic is a result of statements made by the public at several meeting on modeling guidance that were held in October 1980. Through these meetings, EPA had entered into a public hearing process to revise the Guideline on Air Quality Models.

However, attendess at these, and subsequent meetings, made it clear that model accuracy and uncertainty were major and pervasive issues. These issues overshadowed the more narrow problems of what specific models and data bases should be applied to individual air pollution sources.

Thus, EPA decided to delay revisions to its modeling guidance and to direct the modeling conference to the issues associated with uncertainty.

Since the technical community has not yet come fully to grips with the quantification of model accuracy and the treatment of uncertainty in air quality management activites, additional background information was thought to be necessary for a productive conference. A focal point was needed.

As a result, in May of this year, EPA sponsored a workshop on the role of atmostpheric models in regulatory decision making that was held at Airlie House.

The workshop was attended by, approximately, 40 invited individuals, ranging from lawyers to mathematicians. The attendees represented a broad rang of interests. Their training, experience, and current responsibilities made them highly qualified to deal with these complex technical and regulatory issues.

The summary report from that workshop has been provided to all who expressed interest in today's Conference. Additional copies are in the rear of the auditorium.

We hope that this report will serve as a honing tool for your ideas and recommendations.

The Conference is designed to encourage an information exchange, rather than solicit comments on specific EPA proposals.

Topics to be addressed are the use of modeals in regulatory processes, and the accuracy and reliability of models.

We are interested in recommendations for improvements, both in modeling procedures and in regulatory processes, with the goal of ensuring the optimal use of air quality models in all programs which require their use.

Specific technical revisions to the Agency's modeling guideline are not an agenda item. However, ideas presented at this Conference will be carefully

considered and factored, as appropriate, into EPA's overall modeling policy.

The guideline, when modified later this year, will certainly reflect the discussions and suggestions made at this Conference and the meetings in 1980.

We anticipate a public hearing on the guideline early next year.

We have a full schedule for the next three days. I would like to briefly summarize it for you.

The first day of the Conference is organized to bring you up to date on the issues of model accuracy and the use of model uncertainty in decision making.

Tuesday morning is set aside for presentations by other governmental agencies. Tuesday afternoon and Wednesday are open for comments and presentations from the general public.

Throughout this period there is ample time for questions and discussion.

We have divided this morning into two panel presentations. The first deals with model evaluation and accuracy, with presentations by EPA, Electric Power Research Institute, and the American Meteorological Society.

The presentations are followed by a period for questions and answers.

The second panel is concerned with regulatory

aspects of the uncertainty problem and will summarize current EPA activities in this area.

These presentations will, also, be followed by a question and answer session.

This afternoon will be devoted to a discussion of the Airlie House workshop which was held last May. Included will be a summary report, a panel discussion, and time for open exchange between the Panel members and the audience.

We have, specifically, invited those governmental agencies identified in Section 320 of the Clean Air Act to participate in this Conference.

We have also tried to include any other agency we know that has an interest in air quality modeling.

The following agencies have requested time for presentations and will appear Tuesday morning. They are by the Federal Aviation Administration, Mr. Sundataraman. By the Federal Highway Administration, Dr. Jongedyk and Carpenter. By the National Oceanic and Atmospheric Administration, Mr. Jeffter and Dr. Draxler. By the U. S. Geological Survey, Mr. Goll. By the Nuclear Regulatory Commission, Mr. Markee. By the National Park Service, Mr. Henderson. By the Department of Energy, Dr. Shull. And by the State of Texas, Department of Transportation, Dr. Noe.

Prior to today, we have received request from the following individuals to make oral presentations. They are Alan Witten, Richard Londergan, Richard Hanson, Michael Basta, James Peterson, Robert Kohn, Donald Moon, Jerry Pell, Ray Wright, and Ralph Sklarew.

These presentations will begin late tomorrow morning or tomorrow afternoon.

If there are any Government representatives or members of the public who wish to make a presentation, and I have not read your name, or you have not made arrangements this morning, please see Ann Asbill at the Registration Desk or Charlotte Hopper in the back of the auditorium.

I will announce an updated list of speakers after lunch today. Also, a list of the names I have just read is posted outside the auditorium for your further reference.

As required by the Clean Air Act, a verbatim transcript of these proceedings is being maintained. The reporter is Mike Anderson of the Neal R. Gross and Company, Incorporated.

Speakers are encouraged to provide extra copies of their presentation for the convenience of the recorder and the Chairman.

Interested persons will be permitted to enter

into the record any written comments they do not present orally.

The record will remain open for written statements and comments until September 14, 1981.

The transcript and all written statements will be maintained in Docket Number A-80=46, in the Central Docket Section, Mail Drop A-130, of the Environmental Protection Agency.

The address Waterside Mall, 401 M. Street, S.W., Washington, D. C., 20460

If you would like individual copies of the proceedings of this Conference, the verbatim transcript, please contact Mr. Anderson, the recorder, directly.

The comments and discussion during this Conference will be informal and non-adjudicatory.

While several longer presentations are scheduled for today, individual presentations on Tuesday and Wednesday should, generally, be limited to 10 minutes.

When making your presentation, please give your written statements to the recorder, and summarize your remarks, if they are lengthy.

Come to the podium for your presentation. Projection equipment, namely, a 33 millimeter slide projector and an overhead projector, are available.

When making a statement, identify yourself,

your organizations, and your address, both verbally and on any written statement. If, at any appropriate time, you have a question or a brief observation, go to the nearest microphone, either one of the two on the floor or the one, here, on the podium. Clearly state your name and affiliation for the recorder, before you proceed.

I should also like to briefly mention facilities. Rest rooms are avilable down the side wings of this building. Men's to my left and lady's to my right. Public telephones are also available in those wings.

In wing three, there is a very nice careteria which is available for breakfast and lunch. If you wish to go cutside to eat, you are on your own.

Ann asbill, at the Registration Desk, will try to be of assistance with any problems or questions you might have.

Now we would like to proceed with the formal agenda as you have it.

And with that, I'd like to re-introduce Mr. Rhoads, who will give us an overview of the background to this Conference.

Thank you.

MR. RHOADS: Don't worry. I'm not going to be coming up here after each and every speaker. I was

just filling in for our missing Mr. Tuerk and Ms. Bennett.

We are convening this Conference on Modeling Uncertainity during a period of general uncertainty in the entire air pollution control community. But it is also a period of great opportunity and challenge. General uncertainty exists because the Clean Air Act is in the process of revision. Opportunity and challenge exist because EPA is in the process of reevaluating our old ways of doing business, and developing more efficient, effective, and scientifically supportable ways of protecting the environment in which we all live.

I don't believe it is productive for this Conference to engage in conjecture over possible changes to the Clean Air Act. Rather I recommend that, for the purposes of this Conference, we assume that the two fundamental stategies of the current Clean Air Act remain. By fundamental strategies, I refer to technology based emission standards for, primarily, new sources, and air quality management for both new and existing sources.

The purpose of technology based standards for new sources is primarily to minimize new pollution and hence maximize the potential for future expansion and growth. Although air quality impact is a consideration in establishing technology based standards, detailed

modeling plays a relatively minor role.

The purpose of Air Quality Management, on the other hand, is to directly protect the public health and welfare as defined by ambient standards, and to prevent significant deterioration of air quality however it may be defined. Air Quality Management does require a direct assessment of the impact of emission sources on air quality.

There are, generally, only two ways to quantitatively assess the impact of emission sources on air quality, that is, Ambient Monitoring and Air Quality Modeling.

Ambient Monitoring is a valuable too, but it is expensive, time consuming, and of somewhat limited utility. In this country, we spend approximately $30 million per year on air quality monitoring. It typically costs anywhere from 5 to 50 thousand dollars to set up and operate a single monitor, and normally requires at least one year before we obtain useful data.

Monitors usually cannot be located at the precise sit of maximum air quality impact, and cannot record small incremental emission impacts (on the order of the current PSD increments). Of most importance, monitors cannot assess the effect of future actions. They cannot evaluate the effectiveness of proposed emission limits

or alternative control strategies, nor can they predict the impact of proposed new sources.

Ambient Monitoring is appropriate for evaluating air quality trends, for assessing urban or mult-source air quality, and for development and evaluation of air quality models. But since monitors will not be able to replace models, air quality models will be indispensible tools for the forseeable future. We therefore need to find the most effective ways to use these tools.

For the next several days, we will be discussing ways to use a wide range of models, including single point source models, complex source models, mobile source models, area source models, photochemical models, and special purpose models which address visibility impacts and long-range transport.

When discussing models, we must recognize that the input data becomes an inherent part of any model, and hense, the accuracy of the input data has a direct impact on the accuracy of the model output. We know that emission source parameters (and particularly mobile source parameters) often contain substantial errors. We know that the quality of meteorological data can vary widely, and that large differences in model output result depending upon whether the meteorological data is obtained from a sophisticated on-site meteorological station or from a

distant National Weather Service Station.

The perfect data base does not exist. Improved data are becoming available, but development if improved data is expensive and time consuming, and we must recognize that the limited resources of regulatory agencies will often preclude acquisition of data of the high quality generally used for research. The optimum balance must be achieved between the quality of the input data, the quality of the basic model algorithms, the reliance we must place on the model output, and the ultimate use of the model output.

We use, and must use, models for a wide variety of purposes in air quality management. For example, we use models to make yes/no decisions on construction of new sources at specific locations. We use them to decide on the need for controls more stringent than required by the conventional technology based standards. We use them to decide which specific sources within a group of sources require controls, and what levels of control required. We use them to decide the relative effectiveness of alternative control strategies for entire metropolitan areas, and may soon be using them to decide strategies for large multi-state regions.

These decisions often have tremendous economic and environmental impacts. Often (probably too often)

these decisions are based almost solely on the results of air quality models -- models which we all know to have varying degrees of uncertainty. Uncertainty and ways to accomodate it in the regulatory process are the major themes of this Conference.

This morning Bruce Turner will provide us with an overview of EPA's rather extensive research program to develop new and improved models. At least my script says Bruce Turner will do it this morning. He's sitting on an airplane at Raleigh-Durham. He may speak to use later this morning, or early this afternoon. However, he will be here. He will out line EPA's research program.

But I submit that regardless of how much research effort we devote to developing and improving models and their data bases, some degree of uncertainty will always exist in the output.

One of our current problems is that our knowledge of the degree is uncertainty of models is far from perfect, and far less than what it should be. There are several reasons for this. With the implementation of the Clean Air Act of 1970, models very suddenly became primary regulatory tools. Under the extreme urgencies of that period, regulators, industry, and environmentalists, alike, were forced to apply whatever models were available at that time. Many viewed the accuracy of the models

as of little significance, because regardless of accuracy, they were by far the best, indeed the only, methods for predicting air quality impact.

After the initial surge of use in the early 1970's, great effort went into improving the internal algorithms of the more widely accepted models,and effort into developing new models to handle problems which were not amenable to solution by existing models. Still, not much attention was devoted to quantifying the accuracy, because each improvement in the models was generally conceded to be an improvement in accuracy and,therefore, tended to reduce the problem.

Especially within the last few years, however, it has become generally recognized that, despite the best efforts of the scientific community, even our best models will continue to have inaccuracies or uncertainties which are significant. By significant, I mean of sufficient magnitude that (1) the uncertainty should be quantified and documented, and (2) the uncertainty should be explicitly considered in decisions which are based upon modeling results.

The major impediment to quantifying uncertainty has been lack of suitable data bases. If one defines uncertainty as the different between the model output and some absolute "truth," then in order to quantify

uncertainty, one must quantify, that is, measure, the "truth."

Under our current regulatory structure the difficult "truth" which we must somehow measure is the second highest ambient concentration at any location and at any time. This measurement theoretically would require an infinite number of monitors operating for an infinite time period in all possible types of terrain, all possible weather conditions, and all possible source configurations and operating conditions. This is, obviously, impractical. However, this measurement, even in a practical sense, still requires a very large number of monitors operating for extended periods in the type of terrain and with the source configuration of interest. The quantification of modeling uncertainty is a resource intensive task, and it is very time consuming. It requires careful study design and meticulous data analysis and interpretation. But it is a task which is absolutely essential, if we expect to explicitly consider modeling uncertainty in our regulatory decisions.

Later this morning, Norm Bowne will discuss the Electric Power Research Institute's very estensive ongoing project to develop a set of data bases for evaluation and analysis of model uncertainty.

Also, Bill Cox will discuss EPA's ongoing multi-

year program to evaluate model accuracy, and Doug Fox will discuss the American Meteorological Society's on-going work on model evaluation. These three model evaluation programs, as well as several other programs which will be discussed during this Conference, should significantly improve our ability to quantify and understand the uncertainty of the models which are used for regulatory purposes.

Given that models are essential regulatory tools and given that it will be possible to determine the uncertainty inherent in models, or at least in specific model applications, it is also our task to consider how this information can be used to improve our regulatory decisions.

For example, we know that most conventional models are more accurate when predicting long term averages than when predicting short term (such as 3-hour or 24-hour) averages. This means that when the air quality regulatory constraint is based upon a short term average (such as the current short term PSD increments and the current short term ambient standards) the impact of model uncertainty is inherently magnified. Unforotunately, most of the health effects on which the ambient standards are really oriented or based on the effects of short-term pollutant dosages -- a subject which will be discussed

by Bruce Jordan later this morning. It would be difficult to develop long-term surrogates for those short term effects, although Bern Steigerwald will discuss some possible regulatory mechanisms which we are considering to alleviate some of these problems.

Also, for example, we know that most conventional models are more accurate when predicting the upper percentiles of the air quality distributions (such as the 90th or 95th percentile) rather than when predicting maximum or second maximum values. Certain regulatory approaches such as statistically based standards, which will be discussed by Tom Curran a little bit later, and the allowance of more than one exceedance of a standard, are also being considered to alleviate some of these problems.

The fundamental issue, however, is something I have not yet mentioned. Even after we have improved our models to the point of high accuracy with minimum uncertainty -- even after we have altered our regulatory programs to minimize the effect of that uncertainty on our regulatory decisions -- even after wehave evaluated our models such that we know explicitly what the residual uncertainty is -- what should we do with that knowledge? How do we incorporate that knowledge into an individual regulatory decision?

For a simplistic illustration, let us hypothesize that our model says a source must have an emission limit of X to just barely attain the standards. And further hypothesize that the uncertainty about X is plus or minus 20 percent. What emission limit should be established? Should the limit be X plus 20 percent, thus minimizing the risk that we spend too much for controls? Should the limit be X minus 20 percent, thus minimizing the risk that public health be jeopardized? Or should the limit be simply X, in some attempt to equalize the risk of each impact? This is not an easy decision with even such a simplistic example. Imagine the complexity of the decision if we were faced with a real world example in which both public health and millions of dollars are at stake.

I'll leave these questions hanging for the next several days -- and months -- but hopefully not for years.

To help us prepare for this Conference, a group of experts convened a workshop several months ago outside of Washington to scope out and discuss the major issues involving model accuracy and uncertainty in the regulatory process. I believe that most of you received a summary of their deliberations. This elite group did not find all the answers, but they did a good job of finding and defining the problems. After lunch, Shep Burton

and some of the group will present and discuss their findings.

I said the workshop group did not find all the answers. Please recognize that the answers do not exist. They will have to be created. The impact of emissions on air quality if inherently a stochastic or probabalistic process. Hence by definition there is no single answer to the technical issue. Similarly, there is no single answer to the public policy issues.

We need some creative ideas from this Conference. We need some innovative approaches. We need some sound technical facts. And most of all, we need some collective thinking focused on the fundamental issues. If we can accomplish this during the next 2½ days, then we can consider this Conference a success. And I look forward to helping you make this Conference a success.

Thank you.

MR. TIKVART: Next on the agenda we have for you a panel discussion on the issues of model accuracy, determinations, and investigations.

We've got a panel of four individuals, all of which, who have done a fair amount of work in this area, and I think some very interesting and informative information to present to you this morning.

If the individuals will come forward and join

us on the stage, we have a place for you.

Those are Bruce Turner, who is Chief of the Environmental Operations Branch. Hopefully, Bruce will be here sooner or later. He was warned. Second, Mr. Norm Bowne, who's vice president of the TRC Environmental Consultants, who will be speaking for the Electric Power Research Institute. Norm,would you come forward, please. Third, Mr. William Cox, who's a staff scientist in the Source Receptor Analysis Branch. And, fourth, Mr. Douglas Fox, who's Chief Meteorologist of the Rocky Mountain Forest and Range Experiment Station, with the U. S. Forest Service, who will be speaking for the American Meterological Society.

Bruce Turner was scheduled to be first. He will now be last. And, if you don't mind, Norm, would you be willing to proceed.

Norm Bowne, vice president, TRC Environmental Consultants, speaking for the Electric Power Research Institute.

Would you please hold your questions and observations until after all the panelists have spoken. We will have ample time for questions,and answers, and observations at that time. But please hold them until after all three presentations have been made.

Thank you.

MR. BOWNE: My name is Norman Bowne. I am Vice President and Chief Consulting Scientist for TRC -- Environmental Consultants, Inc., 125 Silas Dean Highway, Wethersfield, Connecticut. I am appearing today to present preliminary results from the Electric Power Research Institute's Plume Model Validation Project. I will refer to the- as EPRI and PMV in the future. While the work reported here was sponsored by EPRI, any conclusion I present are my own and do not necessarily represent conclusions or opinions of EPRI or its member utilities.

Air quality models, or more accurately, atmospheric dispersion models used to predict air quality have been elevated to the role of quantitative, analystical devices for estimating various environment impacts of pollutants. The use of existing models to predict concentrations for material emitted from tall stacks has called into question the accuracy and reliability of these models in various terrains. A statement frequently heard is that plume models predict surface concentrations to within a factor of two. But rigorous evaluations to demonstrate that models are capable of predicting concentrations with a factor of two or any other factor are rare.

EPRI recognized the need for model evaluation and developed PMV as the most comprehensive program of plume concentration measurements ever undertaken to

to determine the accuracy of models typically used to assess the impact of fossil fueled electric generating facilities. EPRI formed an advisory committee of scientists from the EPA, NOAA, National Laboratories, universities and utilities to plan and review the PMV program at each step including the results I will describe today.

Our conclusions are that existing Gaussian plume models are not capable of accurately predicting maximum ground-level concentration or position of the maximum on an hour-by-hour basis. However, when comparing the maximum predicted concentration for the four months of data examined to the maximum concentration observed during the same period, the agreement is within 25 percent. Predicted plume widths were half those observed. Distance and direction to maximum concentration were frequently in error.

A technical report, Preliminary Results from the EPRI Plume Model Validation Project -- Plains Site, EPRI-EA-1799, April 1981, which describes the EPRI program, is attached as Exhibit A to my testimony. I will limit my description of the program to those measurements that were actually used in our analyses in the report. Please note that PMV is an ongoing project and more information will be available in the future, not only for

the level-terrain plains site but also for sites in more complex terrain.

The four main objectives of the Plume Model Validation project are:

1. Establish by statistically rigorous procedures the accuracy and uncertainty of model predictions of concentrations of airborne pollutants emitted from tall stacks,

2. Assess the performance of a given model over a rang of meterological, topographical, and source concitions,

3. Create and archive an extensive data base of measured power plant plume behavior with the accuracy of the data certified through external, third-party audits, and

4. Develop and validate improved plume models for topographical, metecrological and source conditions that are inadequately treated by existing models.

We examine the accuracy of model predictions by testing the model's ability to predict certain features of the ground-level concentration. The features we have examined are the maximum concentration, the postion of the maximum concentration, and the width of the plume.

In the belief that the analysis would be more tractable in a flat rural area with a single source, the

Kincaid Generating Station near Springfield, Illinois was selected. It is owned by the Donmonwealth Ediwon Company. The plant is a baseload facility located in central Illinois and is a coal-fired, mine-mouth plant employing two 660 megawatt generators. Both units are vented through a single, 187-meter stack with an exit diameter of 9.0 meters.

Two types of concentration measurement programs were conducted. Standard air quality monitors were used to measure sulfur dioxide concentration at distances between 3 and 30 kilometers, the distance range of anticipated maximum impact. These monitors operated continuously to provide a detailed record in time at 38 locations. The second concentration measurement program used a tracer gas that sampled by portable monitors at 200 locations. The tracer was released through the stack. The intensive tracer measurement program was conducted during three weeks in April-May 1980 and again for three weeks in July 1980. These monitors operated six to nine hours each test day to provide a detailed record of the ground level pattern of concentration.

The routine air quality monitors were distributed as shown in Figure 1. All of the figures may be found at the back of my prepared testimony. The tracer monitor positions were established for each test day in a pattern

to bracket the ground-level concentration maximum.

Winds and temperatures were measured during the routine program at the Plains Site on a 100 meter meteorological tower at four levels. The tower, solar and terrestrial radiation equipment were located at a site located about one kilometer east of the Kincaid Plant. Other meteorological measurements made at the site included routine weather observations and vertical wind and temperature soundings.

During the intensive periods, meterological measurements made during the routine program were continued, but the measurement frequency was increased. Wind and temperature soundings were made on an hourly shcedule when tracer was being released.

Continuous measurements were made of stack-gas emissions and stack temperature at the 137-meter level of the stack. The SO_2 emission rate, exit velocity and temperature were calculated also from the plant operating information, daily fuel consumption data, hourly electrical load data, and daily coal analyses.

Quality of the routine field measurement data has been assessed through external audits on a quarterly basis by ascertaining compliance with commonly accepted procedures and the challenging the instruments against recognized standards.

Four Gaussian models were selected for evaluation. They are:

o CRSTER - Selected based on its regulatory status as an "approved" model in the EPA Guidelines. CRSTER is commonly used in the licensing applications for isolated power plants.

o MULTIMAX - Selected in a modified form CEQM, as a model that predicts values exactly the same as CRSTER, but at user specified receptor locations. This has allowed the PMV project to predict CRSTER-equivalent concentration values at sites corresponding to actual field monitoring locations.

o TEM (Texas Episodic Model) -- Employs a unique feature to adjust the horizontal dispersion parameters for various observation averaging times. TEM was included in the PMV project to investigate the effects of this feature on Gaussian model performance.

o MSDM -- Chosen because it contains a unique algorithm for estimating plume rise.

Evaluation of TEM and MSDM revealed that certain features of those models affected accuracy of the prediction of concentration. TEM predicted concentrations, especially plume-center-line ground-level concentrations, that were too low because of the dispersion adjustment. The MSDM model did not permit the plume to

to break through stable layers near the top of the stack and ground-level concentrations were predicted as much as 20 times the highest concentration observed during the entire program. These models are dropped from further discussion here and the rest of my remarks pertain to the EPA CRSTER model as reflected in the CEQM version.

Four performance measures were selected to characterize the observed and predicted tracer concentration patterns from the 200-station network for model evaluation:

1. Magnitude of the maximum (highest) concentration for each hour,

2. Magnitude of the highest concentration for each hour at a given distance from the source,

3. Location (distance and direction) of the highest concentration by hour, and

4. Plume width, that is, crosswind standard deviation of ground level concentrations at a given distance from the source.

Statistical comparisons of observed and predicted sulfur dioxide concentrations were performed for the continuously operating 28-station network. They were:

1. Compare the 30 highest observed values with the 30 highest predicted values, regardless of location or time. (Compare the upper ends of the cumulative frequency

distributions of observed and predicted concentration.)

2. Compare each of the 30 highest observed values, with the corresponding predicted value at the same location and time, and compare the 30 highest predicted values with the corresponding observed values.

3. Compare the highest observed value for each hour with the highest predicted value for the same hour regardless of location.

4. Compare the set of all observed values with the set of all predicted values for all hours and stations. Either the observed or predicted value was required to be above the threshold level of the monitor (15 ppb) to eliminate comparison of insignificant concentrations.

Model calculations were made without any attempt to fit the data just as they would be used to assess a proposed source. We used measured emission rates, stack temperatures and velocities rather than assuming a constant rate which must be done for a proposed source. The meteorological data preprocessor from the EPA CRSTER code was employed to format the Springfield meterological data for input to the model.

Results from both the routine network, using sulfur dioxide as the pollutant of interest, and from the sulfur hexafluoride tracer network were used to de- [illegible] I will now describe.

Prediction of distance to maximum concentration for each one hour tracer test exhibited no predictive ability as shown in Figure 2. If there were predictive ability the plotted points would be distributed along the diagonal line. The largest differences between predicted and observed distances occurred for slightly unstable (Category C) and neutral (Category D) stability conditions. The maximum concentration was usually observed closer to the source than predicted for these stability classes.

Prediction of direction to maximum concentration is dependent on the wind direction used to drive the model. The results for the 10-meter wind direction at Springfield are illustrated in Figure 3. Most of the points are below the diagonal line of perfect agreement. Considerable scatter is also obvious. Some improvement is achieved by using the wind direction observed at the Site 100 meters above the ground as shown in Figure 4. It is not surprising that the 100-meter wind is a better indicator for a plume that typically achieved heights of 300 to 500 meters above the ground.

Width of the plume on the surface was examined by comparing the observed standard deviation of the cross-arc concetration pattern with the model standard deviation, sigma-y. Figure 5 illustrates the plume width

results. All but 2 of the 50 points indicate observed plume width larger than predicted. The observed concentration pattern indicated a plume two and a one-half times wider than predicted at 3 km from the source. Figure 6 shows a continuation of the same bias at 7 km from the source. The average difference in plume width was a factor of 2.2 at this distance.

Up to this point we have shown the model has a bias in prediction of direction to maximum concentration when using 10-meter wind observations, a bias in prediction of plume width and no ability to predict distances to maximum concentration for hourly patterns. The next comparison that I will show you is predicted and observed maximum sulfur dioxide concentration from the routine monitoring network. We paired the highest concentration observed in the 28-station fixed air quality network with the highest predicted value for the same hour over the network. That is, the predicted and observed concentrations are from the same time but not necessarily the same location. Figure 7 shows the general disarray of this comparison. But, please note on feature, the highest observed and highest predicted concentrations are both about 100 ppb larger than the second highest values. Figure 8 shows that both the 30 highest predicted concentration and 30 highest observed concentrations clustered

between 150 and 250 ppb, although the observed values are in a much tighter cluster. But at no time did one of the 30 highest predicted values correspond to one of the 30 highest observed values. In fact the highest observed value associated with the high predicted values was only 100 ppb. A similar result is indicated for the observed high concentrations. This indicates no capability for model prediction on an hour-by-hour basis.

There were some unexpected results. Figure 9 shows the maximum sulfur dioxide concentration observed at the ground plotted against emission rate for the 30 highest observed events. The coal burned is relatively constant in sulfur content so the emission rates reflect the plant load. Apparently increased plume rise association with larger plant load ballances the increased sulfur emissions and produces little change in highest ground concentration. Figure 10 illustrates the maximum relative observed trace concentration, that is ground concentration divided by emission rate, as a function of distance from the stack. Model predictions of maximum concentration show an abrupt climb to a maximum at 1 or 2 kilometers and a decrease at greater distances. The observed tracer data indicates a maximum first occurring at 3 km and very little change in highest values to a distance of 20 kilometers.

Another indication of the model's inability to predict consistently as a function of distance may be seen in the sulfur dioxide data. Figure 11 compares the 30 highest ranked predicted and observed values. Data points are paired by rank, not location or time, for stations within 3 to 5 kilometers of the stack. The highest point are overpredictions. Finally, at distances of 5 to 10 kilometers shown in figure 12, there is systematic but small overprediction. Finally, the data observed and predicted at locations between 15 and 20 kilometers were grouped as shown in Figure 13 and underprediction is indicated.

Models are used to predict highest or second highest concentration for regulatory purposes and they are not used to predict impacts in real time. We have demonstrated a total lack of accuracy in the real-time prediction, but in viewing the high concentration there appear to be bounds in both the model predictions and the observed concentrations. The comparison of highest centerline predicted concentrations with highest observed concentrations for 38 test hours are shown as a cumulative distribution function in Figure 14. For these one-hour tracer tests, the model underpredicted, but the upper end of the distribution indicates reasonable agreement.

A similar comparison of the cumulative frequency

distribution of hourly observed and predicted sulfur dioxide concentrations is shown in Figure 15. Again there is reasonable agreement at the upper end of the frequency distribution although, in this case, the model overpredicts. One perplexing problem that needs further examination is the overprediction exhibited by sulfur dioxide data and underprediction exhibited by tracer data. Is it due to different spatial density of sampling? We will have to examine the remainder of our data in an effort to answer that question.

We have examined a limited number of three-hour average sulfur dioxide predictions at this point. The preliminary results of the ranked pairs of 30 highest predicted and observed concentrations are plotted in Figure 16. A slight underprediction of the high values is indicated except for the single highest value.

In summary, our preliminary results from a tall-stack, buoyant source in flat terrain indicate that EPA air quality prediction models exhibit significant scatter or range of observed concentrations associated with predicted concentrations and some bias or error in average values. Bias was apparent in predicting direction to maximum concentration when using a low level wind direction and in predicting plume width at the ground. There was bias in terms of overpredicting distance to

maximum concentration for near neutral stability. There was so much scatter for hour-by-hour prediction of highest concentration that any bias is completely masked, but certainly there was no skill in hour-by-hour predictions. Relative concentrations decreased as plant load increased, and highest ground-level concetration was essentially the same over the range of load conditions examined. Relative maximum concentration was virtually unchanged with distance between 3 and 20 kilometers. Finally, there appears to be a bound on highest concentrations in both the models and the atmostphere as indicated by comparison of highest predicted and observed concentrations.

These results indicate that the EPA CRSTER model may predict the maximum hourly concentration with an accuracy of plus or minus 25 percent, but the location and time of actual occurrence will be completely independent of the prediction. These results imply that use of the model in a multiple source situation or added to short-term background is not feasible because of errors in position, plume width and timing.

Thank you for the opportunity to appear today. I will be happy to respond to your questions.

MR. TIKVART: Thank you, Norm.

I'd like to proceed with our second panelist then. That's William Cox, staff scientist on the Source

Receptor Analysis Branch.

Bruce, would you join us on the stage, please.

MR. COX: I would like to begin by reviewing the results of several studies that have been recently completed to evaluate the reliability of models. After that review, I would like to briefly describe EPA's plans to more systematically evaluate the performance of all models being used or proposed to be used for regulatory purposes.

Before going over the details of these recent model evaluation studies, I would like to make two general observations. The first observation, summarized on the first slide, is that these studies seem to confirm what leading atmospheric scientists have stated for some time; that is, (1) models are reasonably reliable in estimating the magnitude of highest concentrations occurring sometime and somewhere with an areas; (2) models appear to do a better job of estimating concentrations that are averaged in time or space; and (3) plume models perform poorly in predicting concentrations at specific locations, primarily due to uncertainty in knowing precisely where the plume is located for any given hour. Much of the current criticism concerning the accuracy of models usually focuses on their inability to reliably estimate concentrations at specific locations

in times. However, this criticism overlooks the fact that in most regulatory uses, the specific location and exact time of high values are of little importance. Only the magnitude of high concentrations is used to determine regulatory needs and models currently used for regulatory purposes are much more reliable in this type of analysis. In fact, as you've heard, errors in peak-estimated concentrations of 10 to 40 percent are found to be typical, now these errors are certainly well within the often quoted factor-of-two accuracy that has long been recognized for these models. For this reason, statements on modeling error must be interpreted very carefully and conclusions reached only in the context of how model estimates are used by the decision maker.

The second general observation is that todate there has been a notable lack of consistency among the various studies with respect to the statistical techniques and graphical presentations used to describe model performance. As a result, it has been difficult to intercompare the results from different studies with a great degree of confidence. This problem will be somewhat alleviated in the future when the AMS-recommended performance measures are widely used in model evaluations. In fact, [illegible] will rely heavily on the AMS measures in our model evaluation program.

As I just mentioned, several fairly recent studies have been completed including the EPRI study you just yeard about that supply information about the accuracy of models. Each study was designed to allow comparisons of available monitoring data with model estimates to see how well the model performed in estimating actual measured pollutatnt concentrations.

The first study was performed under contract to evaluate the performance of the CRSTER Model. In this study, SO_2 data collected in the immediate vicinity of two power plants located in rolling terrain were used in the evaluation. These two plants are the Stuart and Muskingum plants both located in Ohio near the Ohio River. Hourly SO_2 emissions for each of these plants were computed from hourly averaage monthly fuel sulfur and the megawatt generation values for each unit. The results, shown in the second viewgraph, are based on SO_2 air quality data collected from four stations at Muskingum and five stations around the Stuart plants in 1973.

Although a number of performance indicators were computed to describe how well CRSTER performed, a couple of simple statistics show this fairly clearly. Slide 2, the second viewgraph, compares the 99th percentiles of observed and estimated one-hour and 24-hour average SO_2 levels. The concentrations shown correspond

to the average values occuring at the individual stations. As you can see, the ratios for the one-hour concentrations were somewhat closer to one than for the 24-hour concentrations. While the one-hour results indicated no significant tendency for CRSTER to under- or over-predict, the 24-hour 99th percentile values were underestimated by about 40 percent. One other point is worth noting, and that is correlation coefficients between estimated and observed SO_2 concentrations at specific sites were generally low. This indicates that plume models such as CRSTER are not now as reliable in tracking concentration variations from one time period to another as they are in estimating maximum concentration levels.

It is interesting to compare these results for the Stuart and Muskingum plants with the results of EPRI's study. Viewgraph 3 shows a comparison of estimated and observed concentrations for several points on the frequency distribution of the tracer data set collected at Kincaid. These statistics indicate that CRSTER slightly underestimates hourly measured values but these differences are relatively small ranging from about 10 percent fot the maximum concentration up to about 30 percent for the upper quartile.

Comparisons of model estimates and measured concentrations from the routine SO_2 network show similar

results. In viewgraph 4, the cumulative frequency distribution of observed and estimated concentrations for 482 hours is displayed. The distribution of estimated values parallels the observed levels, especially for the 70th through 98th percentiles. The model appears to slightly overestimate the higher concentrations; the median of the 30 highest predicted values exceeds the median of the 30 highest observed values by about 10 percent. Okay, viewgraph 5 summarizes one other important point that seems worth emphasizing from the EPRI Study. The summary of findings for that project indicate that overall, CRSTER showed no systematic bias of over-prediction or under-prediction of highest concentrations and that CRSTER demonstrated the least bias when compared with two other plume models.

As expected, correlation coefficients between observed and predicted concentrations were generally low, indicating that CRSTER was not particularly useful for predicting the hour-by-hour history of observed SO_2 or tracer concentrations.

The second example I have involves an evaluation of the RAM model using St. Louis SO_2 data collected as part of EPA's RAPS project in 1976. The model was used to estimate hourly average SO_2 concentrations at 13 selected stations using hourly SO_2 emissions and

meterological data as inputs. The hourly averages of measured and predicted SO_2 values were then used to calculate SO_2 concentrations for various averaging times from three hour averages up through annual averages.

Okay, viewgraph 7 shows a comparison of the composite 13-station cumulative frequency distribution for hourly averages of predicted and observed SO_2 concentrations. Overall, the agreement is very good when viewed across the entire network. Where frequency distributions from individual stations were examined, it appeared that concentrations at the center city sites are slightly overestimated. Further analysis of this indicated that this over-prediction is probably related to the way emissions from area sources are handled and not, necessarily, to the model itself.

When hour-by-hour comparison between estimated and measured SO_2 are examined, the matchup is not nearly as good. As expected, the correlation coefficients between observed and predicted concentrations were generally less than 0.3, indicating reasonably poor agreement. Low pairwise correlations are fairly typical of plume oriented models, as I mentioned, and reflect the problem of accurately accounting for plume positioning during a particular hour.

One other anticipated result emerged, and that

is that differences between measured and predicted SO_2 levels improved as averaging time increased. In other words, on an absolute basis, the RAM model estimates for quarterly and annual averages were closer to measured levels than they were for hourly and daily concentrations. However, on a percentage basis, it appeared that differences between model estimates and measured values were not a strong function of averaging time.

The next viewgraph, slide 7, summarizes the major results and conclusions from these evaluations. The first conclusion is that peak short-term concentration levels are estimated fairly well when the precise location and time period are ignored. The error in estimating highest concentrations was typically in the 10 to 40 percent range depending on the model and averaging time.

The second conclusion is that each model was less successful in reproducing measured concentrations at a given station and time period as indicated by the generally low correlation coefficients between measured and estimated concentrations. Both RAM and CRSTER had low correlations between pairs of measured and estimated values probably since transport winds crucial for plume models are not known with precision.

Our overall conclusion from these limited number

of evaluations is that models seem to perform reasonably well in estimating concentrations that have been of greatest concern to regulators and decision makers.

As I mentioned earlier, EPA has developed a plan to perform a systematic evaluation of models used for regulatory purposes. The purpose of these evaluations will be to document the strengths and weaknesses of each model using the measures of performance recently recommended by the AMS.

In the past, the availability of good data bases and the time of regulatory demands have limited our evaluations. Now, however, several comprehensive data bases collected by private industry and government, are available which should be reasonably adequate for estimating the accuracy of both proposed and recommended models. As performance evaluations are completed, independent technical peer reviews are planned. With the completion of these reviews, results will be included in EPA's modeling guidelines.

The next viewgraph, slide 8, shows the models that are to be evaluated. The models are grouped into eight categories that will be evaluated sequentially beginning with the evaluation of rural models. In all, the eight categories of models include, as you can see, (1) rural models, (2) urban area models, (3) industrial

source comples; (4) reactive pollutants, (5) mobile source, (6) complex terrain, (7) visibility, and (8) long-range transport. Each of the models in a given category will be evaluated together using the same data sets so that relative comparisons of model performance can be determined.

Right now work is underway to evaluate potential data sets needed for the evaluations and also to develop a generalized data base structure to accommodate multiple data sets and model categories. Selected data bases meeting minimum criteria in terms of representativeness and quality will be created and installed on EPA's UNIVAC computer for subsequent use in the model evaluations.

Along with the data base review, we are building in-house expertise in (1) applying performance statistics to actual data sets, (2) gaining knowledge about model performance, and (3) identifying problems related to interpretation of performance measures. Initially this involves the preparation and testing of computer software to calculate performance measures. Existing software packages are being used to perform limited analyses of how model performance measures respond to random variations in model input parameters such as transport winds for plume models. If we can establish practical bounds

on model performance caused by data uncertainties, when we may be able to suggest elementary standards for model performance some time in the future.

We expect that it will require several years to fully evaluate the models shown. A contractor has begun work to evaluate the category of rural models which we hope will be completed by late this year. Detailed planning is also underway for evaluating the category of urban models, and hopefully, these performance evaluations will be completed in calendar year 1982. Depending on the availability of resources, models in the other six categories will be completed in subsequent years on a schedule consistent with the availability of data bases and resources.

A number of issues will come up as more experience is gained in evaluating models. The problems of space and time correlations of model estimates and measured values can seriously affect confidence statement on model performance statistics. For this and other reasons, no one really knows yet how to interpret performance statistics nor does any one realy know how to translate performance statistics into measures of uncertainty that can be readily used by the decision maker. The active participation of modelers and statisticians is going to be needed so that these problems can be better understood and

treated effectively.

As I indicated earlier, we plan to incorporate the results of these performance evaluations into a scientific peer review of models used or proposed to be used for regulatory purposes. EPA is now discussing potential mechanisms for conducting the peer review with the AMS. We should know more about how this will proceed as the rural model performance evaluations are wrapped up early next year.

To avoid any unacceptable delay in having at least some understanding of how models perform, EPA is developing under contract a report on model accuracy that should be available in early 1982. This report will provide us with a summary statement on the accuracy of each model for which performance information is available. To the extent possible, these limited statements will use statistics of the form and type recommended by the AMS, recognizing that extensive reanalysis of old data bases is impossible in this short time frame.

Thank you.

MR. TIKVART: Thank you, Bill.

The next panelist is Mr. Bruce Turner, who is Chief of the Environmental Operations Branch of the Office of Research and Development in EPA.

Bruce will make a presentation on EPA research

acitivities.

MR. TURNER: Thank you, Joe. I am very pleased to be here and see friendly faces that I couldn't see through the fog in Raleigh-Durham this morning.

The work that I am reporting is conducted by the Meteorology and Assessment Division. The Division is part of the Environmental Sciences Research Laboratory and is located in Research Triangle Park, North Carolina. Most of the meteorological talen in the division is provided by scientists on assignment to EPA from the National Oceanic and Atmostpheric Administration (NOAA), Department of Commerce.

The major extramural research effort being conducted is the Complex Terrain Modeling Program consisting of field studies and theorectical model development. The conduct of the study is viewed as a five-year program, starting with a small, relatively ideal, terrain feature and then progressing to more complicated site configurations more typical of source-siting problems.

In seeking a conical shaped terrain feature on a plane, Cinder Cone Butte in Idaho, about 100 meters high was selected. The size is amenable to more dense sampler coverage for a set budget than a larger feature, and its nearly ideal shape is extemely useful for comparison with physical modeling studies using EPA's stratified

towing tank.

Field work was completed in September 1980 on flow visualization experiments used in planning the 18 tracer experiments which were run in October and November, 1980.

Of 99 hours of tracer releases during the first 15 experiments, 54 hours were identified as having the tracer plume move toward the hill with the tracer sampling network in good operation. For 38 of these hours the plume centerline went over the butte depicted schematically in this diagram (Figure 1). For 16 hours the plume was below the top of the butte, shown schematically here (Figure 2.)

These data are being used to test selected models including a potential flow model.

Most of the continuing work the rest of this year will consist of analysis of the data collected last fall and the proper archiving of that data for continuing use. No field work will be conducted this calendar year.

Future plans point toward a cooperative field study in the fall of 1982 with other interested parties in order to utilize the funds available most expeditiously.

Complementing the terrain work being accomplishe

by contract is an in-house effort in fluid modeling.

In this program a series of experiments were performed in the towing tank to duplicate the field experiments for one hour in particular (0500-0600 case 206) representative of very stable atmospheric conditions.

Figure 3 shows the measured tracer concentrations for t-e field study.

Among the conclusions from the towing tank tests are:

Gross stability classifications are not sufficient for characterizing concentration distributions. However gross classification would appear to be reasonable for predicting the values of maximums (but not locations).

Surface concentration distributions around the hill are extremely sensitive to changes in wind direction. The location of the maximum concentration shifted throught an angle of approximately 60 degrees looking from the source with a shift of only 10 degrees in wind direction.

Because of the absence of low frequency fluctuations in wind speed and direction in the tank, the concentration distributions observed in the tank were exceedingly narrow; maximum concentrations were 5 to 10 times larger than those observed in the field.

An attempt was made to simulate the low frequency wind fluctuations by superposing concentration patterns

from tows done at a series of discrete wind speeds and wind directions. The resulting pattern is shown in this figure (Figure 4). This attempt was moderately successful in that 80 percent of the model concentrations were within a factor of 2.5 of the field concentrations. The highest model concentration was also a factor of 2.5 higher than the field maximum.

This study demonstrates the considerable usefulness of physical modeling for simulating dispersion under stable conditions.

A comprehensive evaluation of four photochemical air quality simulation models has been performed. The four models are the Photochemical Box Model (PBM); the Urban Airshed Model (UAM), also referred to as the SAI-AIRSHED model; the Langrangian Photochemical Model (LPM), also called the ERT-ELSTAR model; and the Livermore Regional Air Quality Model (LIRAQ).

These models, although differing markedly in characteristics and level of detail modeled, were run in parallel using data from the Regional Air Pollution Study (RAPS) in St. Louis. Ten high-oxidant days were selected for the study from the two-year period.

In tests by EPA, the LIRAQ was found to grossly underestimate oxidant and overestimate NO, NO_2, and CO and, also, the model is not easily transferrable to

to other computers, is not likely to be suitable for further intensive investigation.

Summary statistics for model performance of oxidants are found in this figure (Figure 5) which has two sections. The top section shows comparisons between predicted and observed maximu, at the time and place of the observed maximum noted with the subscript S to indicate "specific". The bottom section gives comparison of observed maximum with predicted maximum (regardless of time for PBM and regardless of time or position for UAM) noted with the subscript I to indicate "independent". The average difference is used as a measure of bias. The mean absolute difference is a measure of scatter. The independent estimates will be equal to or higher than the specific estimates so that the average difference will become more negative in going from specific to independent.

The Photochemical Box Model is seen to overestimate concentrations in the mean, both for the specific and for the independent maxima, with somewhat larger scatter for the independent maxima. The Photochemical Box Model is most applicable to stagnation conditions when the entire urban areas is most likely to act like a huge reaction vessel.

The Urban Airshed Model has a bias toward

underestimation and the scatter decreases when considering the independent maximum.

Although the Langrangian Photochemical Model has very small bias, the scatter indicates both large over- and underestimates for particular cases.

It should be noted that these measures should be examined relative to the magnitude of the observed concentrations shown at the bottom of the table. Since the spatial resolution is greater for the UAM and LPM than for the PBM the observed concentrations over the smaller areas are higher. Scatter is about one-third the mean concentration for the PBM and about one-fourth the mean for the UAM for independent maxima.

A technical report, "Evaluation of Four Urban-scale Air Quality Simulation Models," is being reviewed and should be available from NTIS about the first of the year. Also, two papers will be presented at the San Antonio AMS meeting in January 1982.

Evaluation work on an additional 10 days data is underway for the three models including modifications which may improve their performance. Completion of this work is expected in early 1982.

In beginning a program of research on long range transport, deposition and removal by precipitation, the long term model RURMAP developed by SRI International

was adapted and applied to Eastern North America in 1979. The adapted model, ENAMAP, was used to calculate monthly, seasonal, and annual distributions of sulfur dioxide and sulfate concentrations and wet and dry deposition. Model calculations were based on emission data that included both specialized data prepared for the Sulfate Regional Experiment (SURE) and the EPA's National Emissions Data System (NEDS). Emissions are compiled on a gridded format with k0 kilometer resolution.

Comparison have been made between calculated and measured SO_2 and sulfate concentrations considering squares 140 kilometers on a side, for 6 months between August 1st, 1977 and October 31st, 1978. Measurements consist of about 50 EPRI-SURE sites for SO_2 and sulfate plus additional data stored in SAROAD.

This figure (Figure 6) shows the calculated and measured monthly SO_2 concentrations for August 1977. The maxima are all that I want you to note. The calculated maximum of 69 micrograms per cubic meter occurs in southwestern Pennsylvania. The measured maximum of 47 does occur at the same location.

The next figure (Figure 7) shows the calculated and measured monthly averaged sulfate concentrations for August 1977. Note that the maximum is predicted to be the same of -- or both, the maximum predicted is the

same as the measured, but they do occur at one grid square away from each other.

Research is continusing along the direction of making a multi-layered model for SO_2, sulfate, and NO_x (the current ENAMAP is one layer). Under the memo of intent between the U. S. and Canada, four U. S. models and three Canadian models are being evaluated with a common data base for January and July 1978. We are responsible for the evaluation of ENAMAP.

A major in-house effort continues to be the development of the Regional Photochemical model as part of NEROS (Northeast Regional Oxidant Study). This model, being formulated in modules, is designed to simulate short term (one to three hours) mean concentrations of NO, NO_2, O_3, CO, PAN and four groups of hydrocarbons. The domain of the model is approximately 1,000 kilometers on a side. It will initially be applied to the northeastern U. S. for comparison with data resulting from NEROS. Currently, data gathered in 1979 are being utilized. Budget cuts have delayed the availability of data gathered in 1980 until early next year.

The model contains four layers for vertical resolution. The surfaces that separate each of the layers are variable in both space and time, and are calculated in a way that allows the model to take into account in an

optimum manner many physical processes, including:

horizontal transport and turbulent diffusion,

buoyant rise of urban plumes,

meso and larger scale vertical motion induced by terrain, and divergence,

low-level stratification of surface emissions at night,

cumulus cloud venting of pollutants,

subgrid scale chemistry effects resulting from point and line emissions into the 18 by 18 kilometer grid cells,

surface deposition, washout, and rainout.

The objective of this effort is a validated model which can be used to assess the impact of oxidant control plans on regionally transported ozone from one urban center ot others. The regional scale modeling program is designed in a manner which allows for the logical development, refinement, evaluation, and verification of the model and its subcomponents; and provides a framework from which model research and development can evolve to meet future requirements concerning long-range transport. The model is constructed in a generalized form with modular components describing the various physical and chemical processes occurring on the regional scale. Such a framework allows for refinements, deletions, and additions of phenomenological processes with no

re-tooling of the basic model.

The development of the regional oxidant model is scheduled for completion in 1984 and is considered as phase one in a multi-phase program covering other issues relevant to regional modeling.

The addition of modules to treat the transport and fate of fine inhaled particulate matter will begin in 1982 and should be completed in 1985.

Other possible additions include: long distance transport and fate of sulfates and nitrates, regional patterns of regional acid precipitation, and regional patterns of visibility degradation.

Additional models are included in UNAMAP (Version 4) which was made availble by NTIS in March. An eight-page handout giving brief paragraphs of descriptions for each of the 21 models is available.

A principal research effort pertaining to the modeling of the impact of point sources is development of a dispersion scheme utilizing fluctuation statistics as input. The work is proceeding and all available dispersion studies which include fluctuation measurements are being utilized in the development of the method.

We are anxiously awaiting the availability of the EPRI data for the Kincaid study as this will be the first independent data set that can be used to evaluate

the scheme.

Upon recipt of the final reports on Evaluation of the RAM Using the RAPS Data Base from our contractor on the project, we recognized that some additional work could complement the usefulness of those reports. That work emphasizing model performance near the extremes, for the second-highest once-a-year values, has been completed and will be presented later this month at the 12th International Technical Meeting on Air Pollution Modeling and Its Application sponsored by NATO. Some results are shown in the Summary Table (Figure 8). Since there was one station, 104 that suddenly started measuring high concentrations during the last two months of the year, we suspect a change in emissions, the network second highest concentration is greatly underestimated when all stations are included. The green ratios are estimates over measurements. And for the three hour period is .39. And for the 24-hour period is .25, showing very severe underestimates of the model. When station 104 is neglected, and only the other 12 stations are considered, the measured concentrations come down in the range of the model estimates, but still results in underestimates for the 3- to 24-hour averaging time.

The ratios in red for three-hour of .89, and for 24-hours of .72, result. In all fairness, I should

indicate that when station 104 is eliminated the model overestimates the annual concentration, having a ratio of 2.21, overestimating by more than a factor of 2.

Another evaluation study is underway related to extremes, specifically, again, second-highest once-a-year concentrations. This time for point source modeling. Data for this study was obtained through technical reports prepared by contractors for EPA. In addition to comparisons of model estimates to measurements for each monitoring station, study of the maximum second-highest concentration for each network was examined. For 12 network-years of data for the 24-hour averaging time, the model underestimates the network second-highest 8 out of 12. Only one value, an overestimate, is off by more than a factor of two. For 11 network-years of data for the 3-hour averaging time, the model overestimates the network second-highest 7 out of 11. Two values, both overestimates, are off by more than a factor of two.

It takes a lot of data to be able to look at the extreme. And you will note that there are just very few measures that you can make with 12 network-years of data. But that represents an awful lot of effort on the part of the people doing the sampling. And I thank them for that effort.

By adding to the historical record of model

performance these studies will aid in assessing model usefulness in impact studies.

Thank you.

MR. TIKVART: Thank you, Bruce.

Our last speaker on this panel is Dr. Douglas Fox. Doug is Chief Meteorologist of the Rocky Mountain Forest and Range Experiment Stations, with the U. S. Forest Service.

This morning, Doug is speaking on behalf of the American Meterological Society, and is reporting on the AMS-EPA cooperative agreement.

Doug.

MR. FOX: Thank you, Joe.

I notice that there is at least one change since we were here talking in 1977. And that is that the podium is now on the right side of the stage instead of on the left side of the stage where it was before. I don't know if that's a significant change or not.

It's a real pleasure for me to be here to present the results of the American Meterological Society-EPA cooperative agreement on air quality modeling.

As many of you know, EPA and the AMS entered into this cooperative effort in September of 1979 for the purpose of providing technical assistance and advice to EPA focussed on applications of modeling mandated by the

Clean Air Act.

In particular, the AMS to the following four points. Number one, conduct a review of, and make recommendations on previous work concerning air quality models accomplished by EPA.

Two, undertake a general review of the state of knowledge in air quality modeling.

Three, offer suggestions concerning recommended air quality models and criteria for their selection.

And, four, evaluate data base requirements for use with models.

The AMS motivation for this cooperative effort is straight forwardward. Namely, to attempt to provide a funnel through which the atmospheric sciences community might more directly feed new scientific information into EPA programs and policies.

The cooperative agreement, which has been underway for some 18 months, had been under the direction of a small steering committee consisting of two former chairmen of the AMS Committee on Turbulence and Diffusion, namely Darryl Randerson and Steve Hanna, and two former chairmen of the AMS Committee on Air Pollution, namely, myself and Bruce Egan.

Dr. Fred White. Fred would you stand up. I know you hate to do that, but acknowledge yourself. Fred

has served as staff director of this activity on a part-time basis. And that's a small statement that doesn't convey anywhere near the time, effort, sweat, and blood that Fred has given to us. And I think the whole community of air quality modeling is well served by his efforts.

Larry Neimeyer, Herb Slater, and Joe Tikvart have been our principal EPA contacts,

The steering committee met 20 times during the course of this project, to date. We have formally utilized the advice of over 40 individuals, and 100's of others on an informal basis.

There assistance and thoughtful suggestions are gratefully appreciated.

We have prepared a report to EPA documenting some of these activities. Unfortunately, the report is in the final stages of review and not available at this Conference.

We have asked Mr. Tikvart to send all registrants of this meeting a copy of our report when it becomes available.

The AMS members of the steering committee have attempted to draw a fine line between offering advice on scientific aspects of air quality modeling and becoming involved in regulatory decisions.

For example, we have not been involved in the

drafting of a review of proposed new guidelines. Nor have we commented on any specific EPA modeling decisions. We took no formal part in the Airlie House workshop, which Mr. Tikvart mentioned earlier.

We have, however, felt it necessary to comment on EPA's use of scientific information, the EPA's posture toward acceptance of new ideas, and on some of the knotty problems confronting regulatory modeling.

In this regard, our report, probably, will not represent a totally objective, unbiased, purely scientific reflection of the state of the art in air quality modeling. Rather it represents a collection of thoughts on the subject, colored to a large extent by on-going debates about the appropriate use of models, and modeling, in the management of air resources.

We are prepared today to read the recommendations that are drafted in our report. But, first, we wish to underscore the scientific validity of using dispersion modeling in air resource management.

In a real sense, it is this management, or the need for management, that drives the use of air quality modeling in regulatory applications.

While scientists have been attempting to predict ground-level concentrations of pollutants, for many years, the legal mandate to make specific economically significant decisions, based on such predictions, has

grown increasingly important as the Clean Air Act has evolved to its current status.

There is overwhelming scientific support for the hypothesis that the ground-level concentration of a pollutant can be related to the amount and nature of emissions, the meteorological elements, especially the wind speed and direction and atmospheric stability, to the chemical transformations, and to the amount of atmospheric turbulence present in the intervening space between the emission and the measurement point.

However, nearly all the parameters of this very complex system are stochastic, or random, in nature. They are also poorly sampled, or measured, for any particular application, and must be predicted to make projections of what might happen in the future.

These facts lead to uncertainty in, and inaccuracy of, any particular concentration prediction.

The scientific reality, which we wish to underscore, is simply that models will also predict a number which has some degree of uncertainty associated with it.

It is expected that better models and better data will reduce the uncertainty, but not eliminate it. Nevertheless, the presence of this uncertainty does not, in our minds, eliminate the usefulness of modeling. Indeed, to the extent that models reflect our best

understanding of the relevant physical processes, they represent a scientifically sound and objective means for taking into account difference in source geometries and topographic and meteorological settings and assessing ambient air quality impact.

Thus, models provide a logical and environmentally equitable basis for decision making.

We feel that an appropriate scientific response to the reality of uncertainty is reflected in your recommentions. Basically, the recommendations represent three generaly lines of thought.

Namely, one, improved research is needed to reduce the uncertainty of modeling estimates.

Two, EPA must strive to quantify the uncertainty of model estimates.

And three, decisionmaking should be cognizant of the relative degree of uncertainty in various alternative ways to use model estimates.

The recommendations are presented, roughly, in this order, leading from the more general to the more specific. And, now, I'll read our specific recommendations.

Number one, we recommend that EPA strongly endorse the use of atmospheric dispersion models for assistance in making decisions related to the management of

of air quality. Based on our review we have concluded that these models are useful and appropriate technical tools, but the interpretation of model predictions should include a consideration of statistical conficence intervals.

Number two, we recommend that EPA continue and enhance research and development activities to increase our understanding of the following topics, which I emphasize are not, necessarily, listed in order of priority.

A. Long-range transport and transformation of pollutants thought to be involved in acid precipitation.

B. Dispersion in regions of mountainous terrain.

C. Modeling of chemically reactive pollutants of concern to the formation of ozone and other important atmostpheric constitutents.

D. Modeling of the atmostpheric transport, depostions, and ultimate fate of toxic materials in the ecosystem.

And E, identification of representative meteorological information.

The scientific basis for models and the quantification of modeling accuracy will require efforts that are many times greater than they are currently receiving.

Current Federal research in this area, supported primarily by the EPA, DOE, NOAA, and the National Science Foundation, is not sufficient to keep up with ever increasing and changing demands by those who use the results.

Upgraded research should be the first priority for the future.

Our recommendation number three. We recommend that EPA take greater initiative to anticipate the direction of regulatory modeling needs. Researchers must be more sensitive to the regulatory context of their products and are encouraged to provide a finished product in a timely fashion.

Regulators must be willing to incorporate new improved procedures when they become available.

To bring researchers and regulators together, we recommend jointly conducted workshops or other procedures, drawing heavily on scientists outside EPA, to improve the use of new schientific findings and to provide mutually acceptable direction to EPA programs.

Our recommendation number four. We recommend that EPA adopt and issue specific guidance, for example, as a part of the PSD regulations, relating to meteorological data.

The importance of using appropriate

meteorological data inputs in air quality models is stressed.

Suggestion from a recent workshop held by EPA, which -- a review of which is published in the February 1981 issue of the Bulletin of AMS, focus on the importance of on-site measurements of turbulence intensities, wind velocity, temperature gradient, and mixing depth. We endorse the recommendations of that workshop.

Number five, we recommend that EPA adopt a specific set of measures to statistically evaluate new and existing model performance. One suggestiong of such measures is based upon results of a workshop conducted under the AMS-EPA Cooperative Agreement in September 1980 at Wood's Hole, Massachusetts. Results of this workshop were published in the May 1981 issue of the Bulletin of AMS, which, by the way, I understand is being issued, right at the present time, in the mail, and they have been discussed by previous speakers here this morning.

Number six, we recommend that EPA review all models, including those currently recommended in the modeling guideline, and those being considered for regulatory applications.

This review should be based upon a statistical evaluation using the performance measures recommended above and/or a scientifically based peer review.

The peer review is intended as an evaluation of the scientific merits of the model. We believe that the peer review should have the following characteristics.

A. Adequate financial and staff support should be provided to allow the conduct of the statistical evaluations, and to ensure feedback of review findings to model developers.

B. No less than five reviewers, knowledgeable in the science of modeling and in the regulatory applications of models, should be used.

C. Reviewers must be provided the results of the statistical performance evaluation before they conduct the rest of their review.

And D, reviewers should consider all models in a given category. All the rural models. All the urban models. Etcetera.

We further recommend that a group distinct from those responsible for regulatory modeling within EPA assume overall responsibility for this review.

Recommendation number seven. We recommend that EPA refocus its efforts to achieve consistency in modeling applications. The focus should be shifted away from issues of specific numerical agreement between alternative models (and the use of a limited set of models), and toward issues of consistence of approach to modeling

problems.

We are concerned that EPA has overdrawn its mandate to specify models with reasongalbe particulatiry to imply numerical agreement. Instead, EPA should develope a review process that is consistent, for example, in terms of meteorological data requirements, types of dispersion model outputs used, and the like.

This review process should also treat new models responsibly, recognizing the admonition to EPA contained in Judge Robinson's Alabama Power decision, that in questions of modeling controversey -- and we quote Judge Robinson, "EPA should have the -- should move to adopt the more accurate procedure."

Our recommendation number eight. We recommend that EPA recognize differences between two numerical results as being significant only if they are outside a confidence interval statistically constructed about the prediction.

For example, model components, such as plume rise of dispersion coefficients, may be based on different theories or data sets, but still may produce results that agree within the expected accuracy of the method.

Our recommendation number nine. We recommend that EPA consider adopting a procedure to evaluate compliance with the short-term standards on the basis of

more robust statistics than the highest second-highest value. For example, consideration of a NAAQS violation, or PSD increment consuption, could be reformulated to the 95th percentile.

This approach would necessitate an adjustment of the numerical value and/or the manner in which a standard is calculated to ensure that emission limitations, in general, would not be more or less stringent than they are presently.

Such adjustments may need to differ for exceptional circumstances. For example, those involving complex terrain settings.

The basis for this recommendation is that more frequently occurring meterological events can be predicted with greater confidence than rarely occurring events.

We suggest that not only would this change permit models to predict the more frequently occurring events with greater accuracy, but also could allow a prediction -- a reduction in the quantity of meteorological data presently required as input to models.

The procedure would have the additional benefit of reducing the subjective judgemental aspects about what constitutes realistic worst case conditions.

Finally, our recommendation number 10. We

recommend that EPA call for the use by decision makers of more of the results presently available from modeling -- or models to supplement the predictions of the highest second-highest value, or a substitute as discussed above.

Specifically, current modeling practice forces the calculation of concentration for a large number of time periods, and at a large number of receptor points, in order to identify the highest second-highest values.

This output provides valuable supplemental information to the decision maker on the frequency distribution of high values, as well as the geographic area of exposure, and the relationship to meterological patterns over different time intervals.

This information, now largely ignored, should be used in the decision making.

This completes by brief comments on behalf of the American Meteorological Society.

I have a few Xeroxed copies of these comments, so if anyone is particularly interested in receiving them, please see me during the various breaks.

I do, however, want to emphasize that the recommendations which I read to you are still undergoing a rather detailed and thorough review. Some changes may occur before you see our report in print.

Thank you for your attention.

MR. TIKVART: Thank you, Doug. And thank you to all the panelists for your presentations. I think they were all quite informative.

If you'll all take a seat, I think that you can use the microphones in front of you to answer questions, or to respond to discussion items.

I would like to begin the discussion, this morning, of the issue of modeling accuracy by asking a question of Doug Fox.

That is, I believe in two of the recommendations you mentioned, there's indication of confidence intervals around model estimates. Does the AMS report make specific recommendations on how these confidence intervals should be calculated or presented? Or would you care to make a general observation on that?

MR. FOX: Well, the report does not make any specific recommendations on that. I guess the thing I would say is there is still a fair bit of uncertainty associated with evaluating model performance, itself.

Quantifying the methodology and the procedures are fairly straight forward from a statistical perspective. But I think we don't have very much experience with using the various kinds of techniques.

So, I think it would be our position that

before anything specific is recommended there's a fair bit of evaluation and study done, and various types of techniques are tried.

We don't have any specific recommendations.

MR. TIKVART: Okay.

Does anybody else here on the haring panel have a question or comment for the speakers?

MR. DICKE: I'd like to adress a couple of comments to Norm Bowne on the EPRI report.

With regard to the EPRI plume model validation project, I'm a little confused by some of the findings that Mr. Bowne stated, and curious as to why some of the other statements were not made.

I made about four observations that, inaddition, somewhat piggyback on Bill Cox's presentation, and I'm wondering if Norm would be willing to respond to some of these, as time permits.

First, I wonder if we're being reasonable in faulting atmospheric dispersion models for poor skill in estimating concentrations of specific location and time. For example, we've heard several cases, even this morning, that the ability to -- of model users to specify transport, winds, and the rate of atmospheric dilution, in any increment of time, is certainly limited.

Also the technical literature indicates clearly

that due to the random nature of the atmosphere we should not have great expectations for the accuracy of individual concentration estimates.

As a result, I wonder if we may not have reinvented the wheel by finding limitations that, in actuality, were taken for granted by early workers in atmospheric dispersion.

And, in addition, I believe that the EPRI report states that spacial density of the monitoring network is insufficient for evaluating the predictive skill on an hour by hour basis.

Secondly, I was concerned about the finding of the maximum concentrations were independent of emission rate. And I thought that was a little bit counterintuitive, because we all know that if emissions from a source are reduced to zero that there can't be any ground level concentration from that source.

And in reviewing the full EPRI report, I came to the conclusion that this independence was a little bit premature, and that a much more thorough analysis of the data were needed, especially in terms of the width of the error bars.

Some of the factors that need to be further looked at are the relationships among load, emissions, and plume rise and the meteorological conditions associated

with the highest observed concentrations.

In the same vein, I wonder if the relationship between maximum concentration and downwind distance might not also be questionable, because of the limited number of data points presented in the EPRI report. I'm wondering if they may not be insufficient to state with any confidence that the observed relationship is significantly different from the predicted, using a classical dispersion model.

And, of course, Norm did not present any critical information on the effects of sample size and the variable meteorological conditions.

And, lastly, I was curiour as to why two of the most important findings of the EPRI study were not mentioned, as Bill Cox did. And I'd like to reiterate those that for the highest concentration estimates, the commonly used CRSTER model showed no systematic pattern of over-prediction or under-prediction. And that, on balance, that CRSTER demonstrated the least amount of bias for predicting the high concentration values.

Now, if there are any of these four particular points, Norm, that you'd like to respond to --

MR. BOUND: Yes.

MR. DICKE: -- you can do so now, or, of course, you can have later time to write in.

MR. BOONE: I'd like to respond right now, Jim.

First, yes, I think it's reasonable in faulting the ability of models to predict a specific time and locations the way I did today, because much of our audience are not professional modelers. And I did that to demonstrate to them something that we've known for a long time, that you can't predict specific time and location.

And I think that in my presentation I demonstrated why you couldn't. Primarily, wind direction and distance to maximum concentration. Errors that are part of the stochastic nature of the model. And I did that for that reason, to bring out the point.

Second, the spatial density being inadequate. Certainly, it was not adequate from the standpoint of really defining the plume using the sulfur dioxide network, because there are only 28 stations.

However, the spatial density is entirely adequate for the tracer network. We had 200 monitors. And in every test that we used in our analysis, we bracketed the plume, both the going from one side of the plume to the other and being sure that the maximum concentration was after the first arc and before the last arc of the ground level measurements.

Second, you point out the independence of the maximum concentration with respect to the emission rate.

And you are right, we need more data. However, that was based on the high 30 concentrations that were actually observed during that period of time. They covered a rather wide range of emission rates, from about three-quarter load on one unit to full load on two units.

And I don't say that that will -- that particular event will occur at every power plant. But it just happened to occur, here, at Kincaid. I did not find it a result that I expected. And, therefore, thought it worthy of presentation.

The meterological conditions over the highest 30 observed concentrations were relatively well distributed in the unstable range of meteorlogical conditions.

Third, there you are concerned with the maximum concentration versus distance. Figure there are insufficient data. We will have more data to look at within the next six months. That was based on approximately 130 hours of tracer data that were actually used, where we had joint prediction and observed values.

However, the full 232 hour data set were examined. And it happened that those were the highest concentrations for the whole data set. So even though we only used the first 130 hours, or so, of joint predictions and observations, it did happen that we caught the highest [illegible] in the full set.

Finally, two important findings that you said I overlooked. No over or under prediction on the part of CRSTER. I did say that CRSTER was plus or minus 25 percent of the highest concentration. And, I thought that I implied that CRSTER gave the least bias by throwing out the other two models that we described before we went on to them by describing their bias.

But I agree that at the high end of the concentration -- and I'm still perplexed by our data on why we over-predict for SO_2 and underpredict for SF_6. And we're not done yet.

MR. TIKVART: Do any of the panelists have any additional observations, or questions, of another panelist that they wish to make?

No. Then, we're open to questions,or observations, from the audience. Feel free to use the microphone around the left front -- my left front, or the middle right.

Please state your name and affiliation clearly before you make your question or observations.

Gentlemen -- two gentlemen, here, on the left, first. Yes, sir, please.

I think it's one. Just make sure you speak directly in.

MR. [illegible]: I have a question for Mr. Cox

or possibly Jim Dicke. What I'd like to express regarding models to be evaluated, some I'm not familiar with. I wonder if you could identify. In particular the long Z short Z models, RTDM dash WC, plumes, 3141-4141.

MR. TIKVART: Before you go too long. Jim, why don't you answer these one at a time.

MR. DICKE: Okay. These models represent, by and large, all but maybe a couple of the models which were received by EPA in response to the solicitation for non-EPA models. And these happen to be the acronyms that the model developer uses.

MR. TIKVART: Start over again, please. One at a time.

MR. DICKE: Long Z short Z is a pair of models developed, primarily, by the H. E. Cramer Company for use in western Pennsylvania, essentially for Region Three of EPA.

MR. GOODIN: RTDM-WC.

MR. DICKE: That is a model prepared by Environment Research Technology, Incorporated. RTDM stands for rough terrain dispersion model. WC is for worst case.

MR. GOODIN: Plume.

MR. DICKE: Plume Five. I'm sorry. That's Plume Five. That's a model prepared by Pacific Gas and Electric Company. Happens to be -- I guess pretty soon

we may have Plume Six. And I guess some time there was a Plume Four.

MR. GOODIN: 3141, 4141

MR. DICKE: If you are familiar with the workbook for comparison of air quality models, you know that there are a series of four numbers by which you classify applications, etcetera. Dispersion models. Enviroplan developed a number of different models to look at various situations. And 3141-4141 represent two different models which essentially look at particularly terrain applications. So those two models happen to fit the workbook of comparison of air quality model nomenclature.

MR. GOODIN: Visibility models.

MR. DICKE: To my knowledge, that is the title, which, again, ERT gives to their visibility degradation model.

MR. GOODIN: And NPSDIS.

MR. DICKE: That is a model from Rockwell, Incorporated. I'm not sure of the exact name. But it's an acronym which they give especially to their long range transport model.

MR. TIKVART: All the -- of all the models listed, those that are not EPA models, are models that were submitted to us as a result of the Federal Register solicitation of models that appeared in March of 1980.

Approximately 30 models from developers outside of EPA were submitted.

Lew.

MR. KONTNIK: My name is Lewis Kontnik. I'm with Hunton and Williams, representing a utility air regulatory group. And I have a question --

MR. TIKVART: Lew, will you kind of face, partially, so that everybody can hear you.

MR. KONTNIK: Sure. Lew Kontnik. Hunton and Williams, representing a utility air regulatory group.

I have a question for Mr. Rhoads and Mr. Cox having to do with the EPA plans for model accuracy determination. And that is, basically, what kind of provision is EPA making for public access and comment in that investigation?

MR. TIKVART: Maybe I best can answer that.

Exactly how the -- excuse me. Ask your question again. The key part of it.

MR. KONTNIK: Okay.

MR. TIKVART: I want to be sure I answer the right question.

MR. KONTNIK: Well, maybe I should leave it vague.

What I asked was what provision is EPA making for public access and comment in the model accuracy

determination exercises that were described here?

MR. TIKVART: Okay. You mean the model evaluation plan that Bill Cox discussed?

MR. KONTNIK: Yes. And other plans. If there are others that I'm not asking about.

MR. TIKVART: Okay. That's a two phase plan. The first of which involves,stepwise, each category of models. And the results of that evaluation, for each cateogry will be made public one way or other. Exactly how I'm not positive yet. And we will accept public comment on that. Obviously, the results of that will have an effect on what models we recommend for specific applications. That work is underway now. The first set of models, hopefully, -- the rural models, hopefully, will be avilable for public review shortly after the first of the year.

How quickly we do the other categories of models is based on resources available to us. And I think, literally, we're talking about several years here.

The second phase is a peer review. And also the results of that peer review will open for public review and comment. I think that peer review will properly have to lag about six months after the completion of the statistical performance evaluation.

So we intend to release publicly for information

and comment the results of these. They will be factored in to our recommendations on models for specific applications. But the entire mechanism hasn't been worked out yet.

Okay.

Mr. Youngblood.

MR. YOUNGBLOOD: I'm Phil Youngblood. Director of air programs for Conoco, Incorporated. And I have a question about the general area of the presumption that a single source model, specifically CRSTER, does pretty well, if you compare predictions versus observations on an unpaired basis, but doesn't do to well in real space and real time.

There were a couple of allusions made -- well, given that, my question is what are the implications of this failure to compare to well in real space and time, what are the implications of that to a, say, a multi-source model, such as RAM or MPTER.

And Bill Cox, I think, I'm not sure, but I'd like to ask you, first of all, will this be looked into in the validation program that you described. And was it already looked into in the case of the RAPS data that you presented this morning?

I wasn't too sure if you were showing data which showed that, at a given receptor, if you disregard the time

of occurrence, that short term concentrations, predicted versus observed, you can throw time. We're doing well.

Bill, would you generally address that.

MR. COX: Yes. The point you're making is that CRSTER plume models do not predict well at specific events. They don't predict well specific hours, specific stations. But -- and they do well at predicting maximum concentrations. We've spurred within a factor of 10 to 40 percent, 25 percent mentioned by Mr. Bowne.

But using the RAPS data base, in which we looked at concentration at specific stations, again, not matched specifically at time, but looking at frequency distributions, they matched fairly well. When -- the slide that I showed, showed the average of the entire 13 stations network, which is a still further averaging process. But looking at the individual frequency distributions, at specific stations, the pairings were still fairly well. And by fairly well I mean within about 20 to 30 to 40 percent.

MR. YOUNGBLOOD: Of short term highest concentrations?

MR. COX: These are short term. These are hourly concentrations. Correct.

MR. YOUNGBLOOD: If you throw out consideration of when, [illegible]?

MR. COX: That's right.

MR. YOUNGBLOOD: But you're pinning it down on location.

MR. COX: Specific locations. So what I'm saying, you have a multi-source environment, you have some cancellation of errors. And still you're doing a pretty good job of estimating that frequency distribution at given stations.

MR. YOUNGBLOOD: Thank you.

MR. HAMBURG: My name is Fred Hamburg. And I'm with Radiation Management Corporation.

I have two general types of questions. The first one refers to the models that were presented this morning. The question in my mind was: are these models that were presented and discussed, the same as the models that are used in the field?

Now let me explain. In discussing the models, like CRSTER, we heard talk about field programs, validation programs, in which everything was set up at a specific location. The meteorology was determined for that location. Etcetera. As actually used, CRSTER usually [illegible] people to go to the nearest airport and, there, you have a different ball game. Your airport data are quite different from on-site data. The way of determining [illegible] is quite different when you use airport

data as compared with the use of a tower.

So I would like to know whether or not there are studies to show the validity of these two parallel approaches. One in which you are using the Pasquill Gifford determination of stability by means of airport data, by means of the Turner algorithm. Or by means of a delta T off a tower. Or a sigma theta, or a sigma V or W, or whatever.

Also are astability conditions included. How is mixing height determined. And so on. Now, this is the first question that I have pertaining to whether or not CRSTER, as used in the field, is the CRSTER that is being validated here.

MR. TIKVART: Let's tackle that one first.

Norm.

MR. BOWNE: First, yes, CRSTER as used in the field is exactly the same. The information I reported on utilized Springfield Airport data. The closest airport.

The Peoria upper air data were used for the mixing height.

There were astability conditions. Most of them were captured during the summer last year. And we had special studies to make certain that we didn't miss any.

Finally, in our full report, we do compare the

meteorological tower data with the National Weather Service data. And, probably because we're not very far apart, and it's nice and flat out there, there weren't really any significant differences.

MR. TIKVART: Okay. Bill, maybe you can elaborate a little bit. But the evaluations that Bill Cox referred to also was the standard version of the model using, primarily, National Weather Service data. Any further qualification.

MR. COX: Well, the only qualification was that we used hourly estimates of emissions. Okay. And not assume worst case.

MR. TIKVART: Second -- we're going to have to speed up. Second question.

MR. HAMBURG: Okay.

The second question. I was wondering whether any work was going on in light of the fact that there's an admission that the short-term predictions are not quite as good as they ought to be, from the standpoint of reliability, whether or not there's additional work going on with regard to predictions for accidental releases of either hazardous or nuclear material?

MR. TIKVART: Norm, or Bruce. Either one of you want to take that one on.

MR. TURNER: There is some effort in our

Division to make available pop Barling techniques. And approaches using pop Barling techniques would allow fairly rapid calculation of concentrations.

Of course, under accidental release conditions, the estimate of the emission rate is usually the thing that is so very difficult to try to put a handle on.

MR. FOX: The U. S. Weather Service has recently revised its techniques for calculating toxic corridors. And uses the ocean breeze dry gulch equations instead of the standard Pasquill-Gifford methods. This is the standard which, essentially, DOD is beginning to us.

MR. TIKVART: Go ahead.

MR. SADAR: My name is Tony Sadar. I'm representing the Allegheny County Health Department, Bureau of Air Pollution Control, in Pittsburgh, Pa.

This is for Mr. Cox. I understand you're evaluating a variety of models. As your one slide showed, you have many models that will be examined in the future. I'm wondering -- I didn't see the Valley model included on there. Is that model still being -- or is that still endorsed at all? And is it going to be examined any further?

MR. TIKVART: All the models listed there are what we would consider to be refined models. Models that can look at hourly meteorological data. The Valley

model was left off. However, I don't remember, Bill, are Complex One and Two on that list?

MR. COX: Yes.

MR. TIKVART. So Complex One and Two, which in a cynical way, can be considered hourly versions of Valley, I believe are on the list. So the answer -- there are no screening techniques on the list.

MR. SADAR: Thank you.

MR. TIKVART: All right.

MR. MEYER: My name is Ned Meyer, with EPA's Office of Air Quality Planning and Standards.

And I have a comment and two questions.

The comment has to do with the need to, perhaps, look at the shorter term meteorological data, when one is interested in predicting hourly concentrations. For example, we've been doing some work to examine ozone concentrations in the vicinity of the St. Louis RAPS information. And we basically have found that, if one looks at a minute by minute accounting of the wind velocity, one winds up with somewhat different trajectories than if someone averages this over an hour period.

And so, again, I think part of the problem and uncertainty that arises in predicting hourly concentrations of any pollutant could, perhaps, be alleviated somewhat by looking at somewhat shorter averaging times, with

meteorological parameters.

The first question is really for Mr. Bowne. And that is -- I'm surious. You mentioned that the 100 meter wind measurements were a much better indication of the trajectory of a power plant plume than the 10 meter ones were. Do you have any feel at all as to whether or not that is because you're looking solely at an elevated source. That is to say, if one were interested in bottling and area with area sources as with point sources, would you still feel that the 100 meter winds were a more accurate indicator of trajectories that would be involved.

And then the second question is, if you answer to the first is yes, do you feel, based on your experience, there would be any way that one could use 10 meter winds and then, somehow, draw inferences about what the 100 meter winds would look like.

MR. BOWNE: The answer to the first question is no. The 100 meter wind, I think, was the best estimate that we had here, because it was the one that was the closest to the altitude of plume travel, in this particular case.

I think that the wind that's closest to the altitude of plume travel is probably the wind direction that should be used.

And I don't know of any way to project 100 meter wind directions from 10 meter wind directions, routinely.

MR. TIKVART: Next.

MR. GREYFORD: My name is Dick Greyford.

MR. TIKVART: I'm sorry. I can't hear you.

MR. GREYFORD: My name is Dick Greyford. I'm with Foster Wheeler Energy Corporation.

I have some questions. You're evaluating new programs. I'd like to know, will they be available to us through UNAMAP? And will they replace programs already in UNAMAP?

MR. TIKVART: The answer to the question is as the models are evaluated, and as decisions are made as to the relative performance of the models submitted, -- and those within UNAMAP, decisions will be made as to whether or not they should be included in UNAMAP.

Also another related questions here is, we're talking about models submitted by private consultants, in many cases, and they may, or may not, want their model in UNAMAP. They may want to market it individually.

So I would say whether or not the model becomes part of UNAMAP depends on its regulatory usefulness, how well it performs relative to other models, and whether or not the model developer wants to include it in

UNAMAP, or whether he wants to market it himself.

As long as the model can be made generally available, that is our only requirement for whether or not the model is released.

And that can either be UNAMAP or privately.

Lou.

MR. SHENFELD: My name is Lou Shenfeld of the Ontario Ministry of the Environment.

My first question is addressed to Norm Bowne.

Can you comment why, with such a commendable effort to measure air quality over this space, up to 20 kilometers from a source, why there is such little effort to locate the plume? That is determine the wind field over the area? Especially, if you are planning to validate the model on an hour by hour basis.

There is a lack of corelation being obvious from the start. Of course, if you had the corelations showing there, I would have been very suspect.

MR. BOWNE: Well, first, what I reported on was the application of the model in its regulatory setting here, where it's used with airport data. But we have not reported on, yet, the use of the other pieces of wind equipment that we had out there. The soundings. The acoustic sounders -- had to measure the wind field. The [illegible] for [illegible] conditions. And

because we consider that to be a scientific evaluation of the model, while what we have reported on to this point is an operational evaluation of the model, where we're evaluating the model with the same kind of wind information, or meteorological information that is usually available to the user, who wants to project the impact of new plant.

MR. SHENFELD: On a regulatory basis, the model isn't used to predict concentration at a certain point, or at a certain time, is that correct?

MR. BOONE: In the usual method of application by EPA, it is merely a maximum concentration over some time period, over a fairly wide range of distances from the source.

MR. SHENFELD: On that bases, the model did validate? Plus or minus 25 percent.

MR. BOONE: Yes, for the data that we have so far.

MR. SHENFELD: My next question is addressed to Bruce Turner.

You did not mention a time frame for your validation of long range transport models.

MR. TURNER: I'm not sure that I really know that, Len. I think with regard to a specific model, EPA-[illegible], those are monthly values that they are trying to

look at in terms of both air concentrations and in terms of deposition. And that is the primary time frame for the validation work and evaluation work that is being done jointly with the Canadians, as I understand it.

On the basis of the regional model, I do know that the basic time frame is one to three hourly periods for the current oxidant work. And I would assume that for long range transport, in the future, that concentrations over a longer time period would probably be used. But hose would probably result from arithmetic addition of the shorter time frame concentrations.

I'm not positive about that one, Lou.

MR. TIKVART: Can we move on, please.

MR. SHENFELD: Thank you, very much.

MR. TIKVART: I'd like to stay reasonably on time schedule.

I'm going to hold the questions to the remaining two questioners, Mr. Scheve and Mr. Moon. There'll be other opportunities to discuss, so please be brief.

MR. SCHEVE: George Scheve. Patco Environmental. This is for Mr. Fox and Mr. Tikvart. How will the recommendations that the NAS-EPA panel put together be implemented by EPA?

MR. TIKVART: We will have to formally receive the [illegible] -- the recommendations before I can answer

how. But we will make an effort to respond to those recommendations, once they are submitted to us. And I suspect at the time of the hearing on the revised guideline, which will be sometime in the first part of 1982, we will present a response to the recommendations at that time.

Don.

MR. MOON: Don Moon. Salt River Project, Phoenix. Short question, for Bruce, I guess.

We've addressed, primarily, the flat terrain comparisons today. And we've confirmed that they're accurate within a factor of two. What do you feel, right now, at this stage of the game, is the accuracy in complex terrain?

MT. TURNER: I think that is going to depend upon the averaging time, again. But I presume that you're, perhaps, talking about something like second-highest once a year, or something like that.

I don't think I've seen any data that would allow one to make very much of a guess as to what that is. Some of the comparisons that the Valley model has looked at, some of the concentrations are within a factor of two, but a lot of them are not. And some deviate by more like a factor of 10.

I don't think -- that's the area where we really need [illegible].

MR. TIKVART: Okay. Thank you. Thank you, to all the panelists. And thank you, for the good questions.

We now have a fifteen minute break scheduled. Don't wander too far. We will start promptly at 11:35.

(Whereupon a morning break was taken at 11:19 a.m.)

(11:33 a.m.)

MR. TIKVART: The next panel will be concerned with incorporation of uncertainty in regulatory decision making.

The second panel will be Mr. Bruce Jordan, who is Chief of the Ambient Standards Branch; Dr. Thomas Curran, who is a statistician with the Monitoring and Reports Branch; and Dr. Bernard Steigerwald, who is Director of the Office of Regional Programs. All in the Office of Air Quality Planning and Standards of EPA.

Collectively, they will discuss the incorporation of uncertainty in regulatory decision making.

Individually, Mr. Jordan will address the issue of air quality standards. Mr. Curran, or Dr. Curran, the inclusion of statistical information in work with standards and regulations. And, finally, Dr. Steigerwald will talk about the use of statistical techniques in regulatory analysis.

As we did with the first panel, please hold

all questions until all three panelists have spoken.

Mr. Jordan.

MR. JORDAN: Thank you, Joe.

I was hoping that you were going to say, until all the panelists have a chance to get out of the office, or out of the room.

Good morning. It's certainly a pleasure to be able to work with this group this morning. And to share a few moments with you, where we can both illustrate some of the problems that we have in trying to meet air quality goals.

You, as modelers, have problems. And those of us who are in the business of setting ambient standards, certainly, have problems. And collectively working together, I think we can begin to resolve some of the problems that will face us in the near future.

Now in the -- in opening my presentation, this morning, I'd like to make a couple of general observations for you. First of all, I think you can rest assured that there will continue to be national ambient air quality standards programs within the U. S. for some time to come.

All the major Act amendments that I have seen, basically, leave or recommend that the ambient air quality [illegible]

There are some fine-tuning, as people want to call it, but, generally, I think ambient air quality standards will be around. And you will be required to model them for some time to come.

I also don't think that you will see any new pollutants in the near future being added to those that already exist, for which we have national ambient air quality standards.

And, third, the data base, the scientific data base, for the basis of these standards,will be very, very closely scrutinized during the review of each standard. That is, scientists, medical people will require a much better data base upon which to make the decisions for the national ambient quality standards.

And I don't think that you, as modelers, can expect that your requirements will be any less stringent.

As you are no doubt aware, the Clean Air Act Amendments of 1977 required that each of the ambient air quality standards be reviewed at least once every five years.

This review process is a very complex process. It involves and extensive amount of data collection, review, and analysis of the data, to try and determine just where the standards should be set.

Thus far, we have gone through the complete

review only for one pollutant. And that being ozone. Carbon monoxide is presently in the proposal state. And we have work underway to revise TSP, NO_2, and sulfur oxide. We also have a proposal out to rescind the hydrocarbon national ambient air quality standards.

Now, as we have undergone these reviews, there have been some trends that have developed that I think can significantly affect the way that we go about modeling to demonstrate attainment in the near future.

The first of these is that the medical community seems to be prone to rely much more on what we call human clinical studies to set the levels of the ambient standards. By human clinical studies, I mean where you stick a human being into a chamber, expose him to a level of pollutant, and observe the effects.

The rationale for doing this is that these studies tend to be more scientifically sound, they can be easily replicated, and it's much easier to isolate the effect of the pollutant itself, as compared to an epidemiological study where you have all kinds of exposure going on simultaneously.

The medical community has also been prone to specify that the status protect against the observed effect. [illegible] clinical studies being, for the [illegible] we see that --

these studies tend to be short duration, or done over a short period of time. We began to observe the health effects from short term exposure. And there seems to be a trend toward a much more shorter term standards, particularly for some of the pollutants.

And a couple of examples, that we have looked at extensively, are SO_2 and nitrogen dioxide.

We in the standard setting business recognize that this puts quite a challenge to the modelers in trying to demonstrate attainment. The problem of asking models to perform in an area where they perform, probably, not as good as they could with some of the longer averaging time.

But, nevertheless, the medical community nots so much concerned about what the models can do, as they are for protecting the general public.

For this reason, I believe that you will see now a period of time when the modelers will be required to develop somewhat or surrogate to demonstrate attainment of the standards. And by surrogate I mean maybe some longer averaging time, modeling longer averaging time, but, at the same time, showing that these model results give a reasonable assurance of protection against the shorter averaging time.

We've also been concerned, as we review the

standards, over the old, what we called the deterministic form of the standard. Realizing, many times, that a major decision has to be made on a modeling result where you model and you find the worst of all possible situations, and a decision to disrupt a new plant is made -- yea and nay -- based on those modeling results.

To some extend, we're trying to do something about this by looking at the form of the standards. And working with the medical community, we have had some success in getting the medical to think a little bit differently than just the pure statistic form -- I mean, deterministic form of the standard.

I think you saw that when we revised the ozone standard, we went to an expected values. As we look at the CO standard, we're also going to, I think, an expected values, and, possibly, even one that will have some multiple exceedents to it.

Now to give you some illustrations of the types of things that we face in the standard setting business, I'd like to run through several of the standards, and just emphasis the kind of protection that the medical community, at least, seems -- some people in the medical community seem to indicate to us that are needed.

I'd like to start off with nitrogen dioxide.

There is a faction within the medical community that is convinced that nitrogen dioxide, in relatively low levels, has some adverse affects to human. Now, what I can understand of it, not being a medical doctor, myself, they explain it to me this way. They say that a relatively low concentration level, that nitrogen dioxide can cause some adverse effects in the body. And it starts off by the body being exposed to it, and the effect occurring, and,after a period of time, the body just dismisses-the nitrogen dioxide.

Now, if, during this duration of exposure, there is a second appearance, or he gets the same dosage all over again, the effects may be additive, or they may not be additive, but, anyhow, the medical community says that the idea that it would be desirable to,sort of,protect from this multiple exposure, or back to back, one hour types of exposure.

At the same time, the medical community indicates that there is a reason to want to protect against very high concentration levels of NO_2 for short periods of time by high, I mean in the order of 1.5 PPM, or maybe even somewhat lower than that.

So, in modeling, if that form of the standard comes, that we may get to a standard that will take on a form like we would like -- current exposure. We're

not so much interested in one hour, by itself, but in back-to-back type of one hours. We're looking at the possibility of having some form of a stochastic type standard. Which I understand would be advantageous from the modeling standpoint.

Also for carbon monoxide we have a pollutant for which the body tends to be an integrator. And it's important how people move about through space. So, for that one, this time around, we've got some exposures models built, but they're probably not totally acceptable to the medical community, at this time, where they will completely accept them. But, in order to account for some of the spatial exposure, we're thinking, very strongly, about the possibility of going to a multiple type of exceedance standard, where you have to have a concentration level that will be an eight hour, as well as a one hour, standard, but a possibility of allowing mutliple exceedances of that standard.

For total suspended particulate matter, we are having some substantial changes in the ambient standards. Instead of recommending a standard that takes into consideration all the mash in the atmosphere, we're going now -- we're thinking,very strongly, about going to a size specific pollutant, where they will be dealing with, for health standards, 10 microms, or less. And for this

one, it looks like the averaging times will still be in the ballpart of annual and, probably, 24 hours.

We also expect that we may see a secondary TSP standard which will require some long range transport modeling. It will most likely be based on something like aesthetic effects, such as visibility.

And, finally, for sulfur dioxide, we see some evidence, at this point in time, that sulfur dioxide may, in fact, cause some adverse health defects at very, very short exposure concentration -- very, very short durations of exposure.

So, we're looking at that. And we see the possibility that we may have to have like a one hour SO_2 standard.

All these are speculations, at this point. And it's only thrown out to you to get you to think about some of the ways that modeling may have to be changed, in the future, to meet some of the ambient air quality standards.

Generally, I see that there is a trend toward a shorter averaging time for many of the standards. And I see that we will, probably, be thinking much stronger toward going to a pure statistical form of standard. That is, trying to get toward such things as a 99 percentile, or a 95 percentile, or some form of that, and totally away from the not to be exceeded more than once a year

type of standard.

Thank you.

MR. TIKVART: Thank you, Bruce.

Next we have Dr. Tomas Curran, who will be speaking on transition to statistically based standards.

Tom.

DR. CURRAN: I should preface my remarks by indicating that my background is primarily in data analysis, rather than in modeling, so that my perspective is more from an ambient monitoring viewpoint.

Also, at the present time, the ozone standard is the only national ambient air quality standard that has been revised to incorporate statistical concepts.

And models are not as routinely used for ozone, in quite the same way they are used for sulfur dioxide. So our experience with statistical standards and dispersion modeling, at the present time, is fairly limited.

What I plan to discuss, today, is, briefly, background on expected exceedance standards, how they affect both monitoring and modeling, a little bit on what has come to be called the Ex Ex method, which was used to incorporate sulfur variability into coal-fired power plant impact assessment. I'll also mention some further issues that, probably, have to be considered.

As far as background, I think most of you know

that in 1971 the initial national ambient air quality standards incorporated short-term limits in terms of concentration values not to be exceeded more than one per year.

Certain advantages to this type of approach. The most obvious one being simplicity. But there are also certain disadvantages. Missing data is probably the most obvious.

What happens is there was no way, really, of conveniently accounting for the effect of missing data in the one per year format. So you got the type of thing where one site might have very complete data for the year, another one very incomplete data. And there was no way of really adjusting for the effect of incomplete data.

One of the ways you can get around that is to impose a minimum data requirement. But it still leaves you with the situation that the site that has complete data is more likely to detect a violation than one that just meets the bare minimum requirement, even if both sites are monitoring essentially the same air quality.

The other point -- the other disadvantage is a little more subtle, but it's still troublesome. And I'll define -- I'll use the term exceedance, as kind of a shorthand, to refer to a concentration value that is above the level of the standard.

And with a once per year standard, you're kind of in the situation where the implication is that it's acceptable to have a non-zero probability of having one exceedance per year. But it's not acceptable to have a non-zero probability of having two or more exceedances during the year. Which, in reality, it's more likely, that if you have a non-zero probability of having one exceedance, you're likely to have some year where you're going to have two or more.

So, in a sense, the once per year standard allows for the unusual event within a year, but not for the unusual year.

Now, one way to get around this was to go to an expected exceedance standard. And, basically, in 1978, when EPA proposed revisions to the ozone standard, one of the points proposed was the introduction of the concept of expected exceedances. And this was promulgated in 1979.

Including in the ozone national ambient air quality standard was an adjustment for missing data. And the standard, itself, was worded in terms of the expected number of exceedances per year. Now, the expected number of exceedances can really be thought of, intuitively, as a long-term average. In other words, if you say that the expected number of exceedances can't be greater

than one, well, what you could have is site that some years has no exceedances, some years it has one exceedance, other years it may have two or more. But the long-term average number of exceedances won't be greater than one.

Okay. That's the intuitive example. From a practical viewpoint with monitoring data, we're never really going to know what the true expected exceedance rate is, because we're not going to have an infinite number of years to average over.

So from a practical viewpoint, we ended up specifying that we'd use a three-year estimate, just to estimate the expected number of exceedances in terms of a three-year average of the observed number of exceedances, after adjusting for missing data.

It really is kind of a compromise. We recognize that the more years that you have the more stability you'd have, in terms of the estimate. But, on a practical basis, emissions are going to change over time, so that using just the most recent data may be more indicative of the current status of an area. And, there's also the practical viewpoint that using fewer years of data ensures that timely action can be taken.

Next overhead.

The facts of going to an expected exceedance

standard are kind of interesting. From a monitoring viewpoint, probably the biggest effect that people saw was the missing -- the adjustment for missing data. From a modeling viewpoint, it's more likely to be the averaging process. Certainly, with models, such as CRSTER, some of these dispersion models, that give a predicted concentration at a 100 receptor sites for every time period in the year, missing data is a relatively minor point.

And it's more likely to be the averaging process. The idea that you need to address the probability of an exceedance, or the frequency of an exceedance.

With an expected exceedance standard, the high values are not really ignored, but they're kind of weighted by how likely they are to occur. The obvious effect of this type of situation on a screening model,which assumes worst case condtions, is that with an expected exceedance standard, it no longer really suffices to show that under worst cases assumptions something can occur.

What you have to address, now, is how likely it is to occur. What is the probability of an exceedance. In a sense, for screening models, I think this is a situation that people have recognized in the past.

If you assume worst case conditions, and you don't have a problem, then fine. If you assume worst case conditions, and you do have a problem, then it really

is kind of a grey area where you have to address how frequently the problem will occur.

For other models, getting away from screening models, it may turn out that the change to expected exceedances is really fairly easy to handle.

I have an illustration, I guess, of the Ex Ex method. This sort of, briefly, indicates the type of thing that can be done. Some of you have have seen it. It's called the expected exceedance method.

Intuitively, all it is, is that you can run a dispersion model, such as CRSTER, with unit emissions. Say, for a particular time period, you get a predicted concentration of .07. And what you really want to know is what's the probability of being greater than .14 PPM.

Well, because CRSTER is linear in emission, the probability of being greater than .14 PPM is the same as the probability of having double the emissions.

So, if you have a frequency distribution of emissions, or some way of computing what was the probability that you would have double the emissions, during that time period, then you can directly compute the probability of an exceedance for that particular time period. And then you can just aggregate these over the year to get an expected exceedance rate for the year.

The actual implementation of this procedure was

done by SAI. And it was done in terms of a computer simulation to account for ommissions.

The intuitive idea is the same as presented here, but from an implementation viewpoint, the simulation allows you to handle more complex descriptions of emissions, either as autocorrelated time series. Also enables you to get certain probabilities more conveniently.

The point is that it does not change the model. Basically, all we've added is to take the previous model that was used in a deterministic framework over to an expected exceedance or probability violation framework. It was simply a translation step that took the predicted concentration and converted it to a probability of exceedance.

Next slide.

Okay. Up to this point, the potential effects of the expected exceedance, or statistical standard, may be -- may appear kind of negative, either makes use of screening models at little more difficult, or it's more work for the translation step, in terms of other models.

But I think the net effect on modeling will probably be positive.

And from an ambient viewpoint, I think people -- there's a tremendous intuitive appeal. People are

familiar with concepts, such as 10-year flood, or 100-year flood. And the idea that we're weighting for high values by how likely it is that they reoccur, just has a certain intuitive appeal.

Also, from a more technical basis, with an expected exceedance standard there's no penalty for using additional data.

One of the problems with the once per year format is it's second highest value for the year that determines your status.

As you look at more years, you look at the highest of the second high values over the time period. So, what you end up with is each time that you add an additional year of data, you cannot really lower the design value. All you can do is it increase.

And I think it is understandable that a source may hesitate to use additional years of data, because adding additional years has an inherent penalty, and there is no real gain for them.

Under an expected exceedance standard, it's possible that adding additional years could either raise or lower the design values. So, it does away with that inherent penalty.

Also has advantages, I think, in terms of the realism. A statistical framework, such as the problem

that really led to the development of Ex Ex method, was if you want to describe emissions statistically, which is a more realistic description than, say, assuming constant emissions for coal-fired plants, -- a deterministic framework does not really accommodate statistical, or probabilistic, descriptions that conveniently.

So, the modeling may actually find greater flexibility within the framework of a statistical, or expected exceedance,type of standard. In fact, not only in terms of the actual descriptions of the processes involved, but even, maybe, the structure of the model.

For example, to compute expected exceedance as a probability of violation, what is really required is that you have the probability of an exceedance for each particular time period. It may turn out that in some situations it's easier to develop a model that yields a frequency distribution for a time period.

In other words, you can't pin it down -- you don't pin it down to a specific concentration value that occurred for that time period. But you have a frequency distribution. You have an idea of, well, a probability that's less than .12 in that time period is such and such. The probability of .13 -- and from that it becomes possible to develope the probability of exceedances, or the expected exceedances, probability of violation.

In other words, model structures, that were not allowed under a deterministic framework, may become very convenient in terms of a statistical framework.

If you think about predicting frequency distributions, for a particular time period, rather than a single concentration values, it's kind of interesting to see what implications that would have for model validation.

I have just a couple of final comments.

I guess, reitering the idea that our experience to date with our statistical standards and dispersion modeling is fairly limited.

There are some things that are worth considering. One of the ones that I have up here is a CAP.

When I mentioned the ozone standard and the idea that we use a three-year average of ambient data to determine the expected exceedance rate, one of the things that's implicit in there is the idea that if I average the number of exceedances over a three-year period, and I say that can't be greater than one, no single year can have more than three exceedances, or you cannot attain the standard.

So the three-year averaging process for ambient data, in a sense, puts a cap on the maximum number of exceedances that can occur in a single year.

From a modeling viewpoint, it may be conceivable

to model a 100 years, or a 1,000 years. And one of the questions that comes up is what does it mean from a health viewpoint if the model predicts a 100 exceedances over a 100 year period, but all the 100 of them occur within a single year.

And, I think, perhaps, we should consider in modeling analyses things like putting a cap on the number of exceedances per year, or specify a higher concentration value that can't be exceeded more than once in 10 years, or this type of stuff. Which leads to some interesting observations of the implications of probabilistic interpretations for modeling results.

For instance, what does it even mean to say that you've modeled 100 years, or 1,000 years. Some of these implications will be discussed by our next speaker.

MR. TIKVART: Thank you, Tom.

With that introduction, next we have Dr. Bernard Steigerwald, who is going to speak on the use of statistical techniques in regulatory analysis.

Bern.

MR. STEIGERWALD: Tom only thinks that I'm going to talk about some of those things.

I have a hard time talking to modelers anyway. And I've never tried to talk to a hungry group of meteorological modelers. So, I will make it very short.

I'm going to be focussing mostly on some of the regulatory implications of using statistical techniques instead of deterministic techniques in modeling.

Basically, the message I have is I think we are moving toward statistical techniques. That it has a surprising impact.

I'm had a couple of years using it, or attempting to look at it, and it has a surprising impact on the regulatory end of the package.

And I think it, also, offers some opportunities to the modelers.

First slide.

I think that we are moving toward statistical techniques not only because of the changing form of the ambient air quality standard, but for several other reasons. I think because it's going to offer modelers and regulators a better way of handling error and uncertainty. A better format for expressing it.

I think that it has the potential for improving the reality or the appropriateness of the the input assumptions, of the non-meteorological input assumptions, if you want.

And I will talk about the Ex Ex method a bit later.

We are having an increasing difficulty

defending some of the input assumptions. Saying that the sulfur content of coal does not vary, when everybody knows that it does vary, is a difficult point to defend.

So that I believe that, even if we don't change the form of the ambient air quality standards, that we will be moving towards statistical techniques, because it's going to help explain uncertainty and describe it. It's going to improve some of the input assumptions. And, finally, because it may enable us to handle a little bit different kind of error than I've heard talked about today.

Doug Fox did talk about the lack of representativeness. The problem of a regulatory making a decision that should be over the lifetime of a new plant all based on one year of meteorological record.

So, that I think that the statistical techniques are coming.

Next, I have been playing around with something called the expected exceedances methods, which, as far as I know, from a regulatory point of view, is our first attempt to move from a deterministic into a statistical type of modeling.

It seems simple to me. You just vary the emissions over time, because the sulfur content of the coal varied over time. Everything else stays the same. I

didn't touch the model. I wouldn't dare.

We just changed the one input assumption. And I was quite surprised at the implications that this hand. And,in the next slide, I attempt to point out some of these, quite triefly.

I'm not sure if everybody can see that.

The first problem that came up is once you start saying I'm not going to say never. I will accept a certain amount of risk. It sounds better to say acceptance of a degree of certainty. But, in fact, it's accepting some quantitative finite risk that that emission limit will allow the air quality standard to be violated. Not to be exceeded once per year, but to be violated.

That forced EPA to pick an acceptable violation frequency. Not much precedence for that. In the Ex Ex method, we picked one in ten for the primary standard. Which means one year out of ten. You can have two days, or two three-hour -- you can have two days above the air quality standard number, and, if that occurs less frequently than one in ten years, it's acceptable.

For the secondary standard, we're in for the PSD increments, where it did not seem necessary to have the same degree of certainty, we'll be tossing out one in five years, as an acceptable risk.

Not much basis for those numbers in the Air

Act or any place else. But it -- when you go to the statistical technique, and you throw out never, you have to put something else in its place, as the basis for saying yes or no to a permit, as the basis for saying yes or no to an emission limit.

Secondly, the worst site doesn't become all holy anymore. You're talking about the probability of violating anywhere in the vicinity of that plant. A lot of us made a mistake. We felt that if the worst site attained the one in ten year probability we were home free.

In fact, you have to consider the probability for all of the sites, because the real question is what is the probability of that source causing a violation, in the vicincity of the source, not just as the worst site.

So, the whole concept we've had, we only worry about the worst site, goes out the window, and you worry about about all sites.

I think that the worst year, as Tom said, as you added more years of data, you had the potential for finding a year that had worse dispersion, a higher highest second high, and, therefore, a tougher regulation.

The Ex Ex method, since it is statistical, integrates, or allows you to put a bad year into perspective with other years, because the answer you are looking

for is how often am I going to violate.

So that, I think, as Tom said, more meteorological data, better data, means a better answer, not, necessarily, a stricter regulation.

Fourth, we have the form of the emission limit. Since the Ex Ex method, you are approving a source based on the distribution of emissions. You, essentially, have to enforce the distribution of emission. Or you have to pick enough points off of the distribution, so that you have a fair shot at enforcing the distribution.

In the old system, one number was used to determine yes or no. And you only had to enforce that number, always.

Now, we will have more complicated regulations, because you will have to do what you can to set regulations that, if enforced, limit the source to the distribution that you modeled.

Five, determining the degree of emission reduction, it tends to be startling to regulators, when they finally understand that if the criterion for acceptance or rejection is how often will that source allow a violation, that the answer is not proportional to the design value.

There is a great sense out there, now, that, if on the highest second high day, or highest second high

three hour period, we double the allowable increment, regulators say we have to half the emission. If you have double the allowable frequency of violations, it's not an obvious answer how much reduction is needed to come into a one in ten probability of violating.

And, I think, it is up to the modelers, now, to give the regulators a range of answers in terms of the acceptance-rejection criterion.

What does a little more control. What does a little less control. And, finally, what kind of control do you need to get to an acceptable frequency of violation.

The information base has to be expanded. And everything gets a little more complicated. If you toss in autocorrelation, if you don't assume random coal lots from one three-hour period to the next, or one day to the next, you then have to toss autocorrelation in. While we have that in the model, not many plants know much about the distribution of their emissions, much less the autocorrelation of that distribution.

Finally, it forced us to decide what we were going to allow to vary. Once you move from a deterministic, where everything except meteorology is aconstant, over into -- we are going to allow something to vary, we solve the problems of the acceptance criteria, you

then have to say why not let other things vary. Why not let load vary? Why not let BTU content of a coal vary? Why not let sulfur retention in the ash vary? Why are you only letting sulfur content of the coal, or the scubber efficiency vary?

Those are questions that have to be answered, and, I think, justified, once you move from a deterministic into a statistical technique.

So that you can see I didn't know that I was getting into all of these, when we just simply were going to model with varied emissions. But it does move me,philosophically, into a whole new ballgame. And I think that it's a good arena to be in. But I think it's got its difficulties, too.

Finally, I think there are opportunities. And if you can put up the final slide. I think it's got some opportunities for the modeler. As I said before, intuitively, it just seems that it will facilitate the handling of air, and the uncertainty in the expression of uncertain results.

And then, finally, a problem that has always bothered me is the one of making a decision on a plant that might be there for 60 years, and understanding, finally, that we use one year of meterological data to analyze the air quality impact. And we assume that that

one year held for the 60 years that the plant was going to be in existence. Not only that the year held, but that every day in the year was the same. So that we had one worst day and one best day that year, in terms of dispersion high, and that no day would ever be worse than that in the 60 year life of the plant. And that no day would ever have better dispersion than that. And each would show up only once. Not twice. Not zero. So I anticipate that we will begin looking into, possibly, the use of dispersion as a variable. Curve fit the Chi curve for each site for, say, 365 days. Or if you have five years, for 1,800 days. Extend the tails of that curve. And then pull a series of 365 day years out of that.

That can be coupled with variable emissions, or any other variable that you want. And, possibly, move us, a little bit, towards better representativeness.

I anticipate that you could put climate changes in and move the high distribution curve a bit. and get at it even better, that way. I'm not sure that any of those can be done. But I think going into the statistical technique gives us the opportunity to do that.

So, in summary, I think there isn't any question in my mind that we are moving towards statistical treatment of modeling, and of modeling results, and as

the basis for regulations. And I think it's a fundamental movement in air quality management system.

I would urge the regulators to understand what it means, and to demand it of the modelers. And I would urge the modelers to look at it as an opportunity to influence regulations and to get better regulations.

Thank you.

MR. TIKVART: Thank you, Bern.

Any questions from anybody on the panel here, or clarifications, or further points?

No. Okay. With that I'll be happy to entertain questions or observations from the floor. If you'll queue up at either one of the microphones.

While you're doing that, Shep Burton has asked me to request that all of this afternoon's panelists meet with him in the back at the conclusion of this session and before you proceed to lunch.

All of this afternoon's panelists meet with him before you depart.

Yes, go ahead.

MR. BEAR: My name is Mitchell Bear. I work for the U. S. Geological Survey.

I have two questions.

The first question is there a chance that we're going to see a short-term NO_2 standard in the next couple

of years?

MR. JORDAN: I wish I could give you a positive answer on that.

The status is simply this. We have gone through several attempts at putting together a data base from which to either set or reject setting a short-term NO_2 standard. And each time we think we've something to go on, it tends to get shot apart. Very simply, that.

At the present time, we expect to go before the Clean Air Scientific Advisory Council in October, or around October the 7th, with a next proposed type of approach for handling the short-term NO_2 standard.

I suspect that there will be a short-term NO_2 standard. I suspect that it will not be a very stringent short-term standard.

MR. BEAR: Okay.

My second point is I discern a transition in EPA's thinking with respect to the standards that seems to be very much in line with the Reagan's Administration position on the standards, that we're going to move away from a threshold type of standard to more of a risk analysis type of standard for all the pollutants.

Is this position being formally taken by EPA during the upcoming reauthorization of the Clean Air Act Amendments, or is it a kind of an informal position that

EPA has taken?

MR. TIKVART: Somebody?

MR. RHOADS: Sure. Why not.

I expect a -- I detect a note of surprise that EPA is going along with the Administration's philosophy. Don't be surprised. That's who we work for.

There've been options within EPA for a long time. We're learning things. We started in this business full-force in 1970. And we're learning that the old concept of threshold, in many cases, is not medically sound. We're learning that the deterministic approach to standard setting sometimes is overly stringent, sometimes is not stringent enough.

So we have a combination of technical knowledge, medical facts, and political philosophy, which just seems to be coming together, basically, right now.

MR. BEAR: Thank you.

MR. NOCEMSON: My name is David Nocemson from Los Alamos National Laboratory.

What people have been saying, in terms of the actual processes going on, they're stochastic, meteorologic, and even the emission processes. So it seems to make sense to go to some kind of statistical,or probabalistic, approach.

It seems like you're going to get into a problem

in terms of how do you validate these methods. The methods are going to depend on the dispersion models and also on the statistical techniques. And, basically, the bottom line which you're estimating is the expected number of exceedances. How are you going to validate your methods for expected number of exceedances, for these, looks like, worst case conditions? It seems like you need years and years of data to do that. It would be even more difficult than trying to validate the dispersion models.

MR. CURRAN: Actually, I think it's kind of interesting, if you listen to the people earlier, who were talking about model validation, the conclusion that they seemed to be reaching was that the frequency -- you know, the overall frequency distribution, if you ignored the spatial and temporal breakdown, that that is where the models were good.

I'm not entirely convinced that switching over to an expected exceedance standard is going to make it that much more difficult.

I think, in a way, that's almost what they're looking at right now.

But I agree with you there are some implications that will have to be looked into.

MR. NOCEMSON: But you're really talking about

the distribution -- well, looking at it from the statistical point of view, you have to look at the distribution of the expected number of exceedances.

MR. CURRAN: Yes. And right now they're looking at what the second max is. So both of them are kind of one number a year.

MR. NOCEMSON: And there's kind of uncertainty associated with that. You need -- you're talking about extreme values. And, usually, you need a long year's record, if you look at the work that's been done in terms of the water area. You need -- they're dealing with records of like 50 years or so. And those aren't always adequate to talk about the case of long-term, you know, a 10-year flood, or a 50-year flood.

MR. CURRAN: Oh, yes. There'll be problems in terms of the length of the data. But on a practical basis, I think that a lot of it won't different a whole lot more than it does right now, as far as looking at the second max.

Yes. I agree with you. I think part of it is going to be almost like a philosophical definition of what it means when we use these models. Do we really want to talk about the expected exceedance rate over a 1,000 years, or 3 years, or what.

So there are almost some philosophical

definitions that'll have to be made. And then, you know, some practical considerations.

MR. HAYNES: My name is Eldewins Haynes. I'm with the North Carolina Division of Environmental Management.

First of all, is there any plan to account for synergistic effects in the standards in the future?

MR. TIKVART: Bruce.

MR. JORDAN: That will strongly depend upon the capabilities of the medical community to demonstrate the synergistic effects.

MR. HAYNES: Okay.

Secondly, about the Ex Ex method. I believe there was some discussion about a autocorrelation, a correlation, I suppose, between various emission parameters and meteorology and time of day. Is that what was implied?

MR. STEIGERWALD: Autocorrelation, as I used it, was the correlation between adjoining lots of coal. Is it an appropriate technique to go into the distribution independently every day. Or, you know, every three hour period. Or does what happened yesterday influence where I should go into the distribution today.

So,when I use the term autocorrelation, I meant the ability to alter the way we pick tomorrow's lot of

coal to be burned. And it did not have to do with the meteorological variables. We do not modify, at all, the output of the meteorological dispersion model.

MR. HAYNES: No. I didn't mean the modification of the dispersion model, but incorporating the variation of emissions to the meteorology, or to the time of day. For instance, a power plant will have a variation of emissions depending upon those factors.

MR. STEIGERWALD: We have -- our Federal Register notice, in which we will be, I hope, proposing the use of the Ex Ex method, allows only the sulfur content of the fuel to vary, or the sulfur content of the fuel and the efficiency of the control device. It does not allow loading to vary.

The modeling is done with the current assumption of worst case load held constant. And that was done, partially, because we do believe that they aren't independent variables. That loading and meteorology, both, are some function of time of day, often. Or of season of the year, often.

So, we have not allowed loading to be considered a variable in the Ex Ex method, as we will be proposing it soon.

MR. HAYNES: Thank you.

MR. TIKVART: Jim.

MR. PRESTON: My name is James Preston. I work for Tenneco, Inc.

My question would be directed at the entire panel.

Basically, as long as we're considering Gaussian plume models, that do not modify the plume's trajectory with meteorology, or time, or space, we're looking at a problem that can fairly readily divorce the problem of time from the analysis, and treat it as a spatial statistical problem, at a considerable savings in machine resources, with no substantial modification results.

If we look at the deposition reaction mechanisms, and the transport, long-range transport considerations, has any consideration been given in terms of the significance of the time or temporal considerations that the future nolds in this area, in these considerations that we're talking about.

And the questions open to any one.

MR. TIKVART: Jim, that's kind of a complicated, involved question. I think the answer is we're still struggling with basic principles, and haven't gotten to the specific technical issue, or implementation question that you're asking.

MR.PRESTON: Okay. Let me put it in little more down to earth. September the 1st, the State of North

Dakota's going to hold a hearing on long-range transport models, for use in the regulatory matters. Inside EPA held several interesting articles on what will EPA do, what will they do.

So, what I'm getting at, the regulatory environment is going to walk into this problem very soon, regardless.

The question is: are we looking at the time aspects, like the presentations today showed that on a time correlated basis we don't have much results that look very favorable. And what I am indicating is the transport time factor, so long as it does not impact the plume trajector, like, for instance, in any way, then you can take the time out of it. Mostly, your point receptor correlations assume instanteous transport. There are some modifications with distance away. But it does not vary from hour to hour. If it takes two hours from the time it's released to reach the receptor, you're not changing the direction of the plume flight.

So, therefore, time is not a factor. But once you get to long-range transport, this is no longer true.

So, what I am getting at, if we make decisions today that divorce ourselves from the time and spatial aspects, will we not be crippling ourselves in the near term future when these become more significant factors?

Is any one looking at that? This is what I am asking.

MR. TIKVART: I think the answer is no one has looked, yet. But, perhaps, Martin Shock would care to comment. Or would he not?

MR. SHOCK: In the context of the upcoming hearing, no. No comment.

MR. TIKVART: In a technical context.

MR. SHOCK: That is correct.

MR. TIKVART: Jim, I don't think we've got a good answer for you. You're asking a valid question. But it's too far down the road for us right now to see the answer.

Next question.

MR. ARBRECHSTADT: Marcel Arbrechstadt. Motor Vehnicles Manufacturers Association.

It seems to me that ulitmately the stochastic nature of the instantaneous concentration of a pollutant has to be considered in the setting of the national ambient air quality standards. And I'm thinking of the inclusion of this factor in the risk analysis techniques that the EPA's working on towards improvement of this setting of national ambient air quality standards.

And I was wondering if Mr. Jordan would like to say a few words about how that process is proceeding.

That is, the inclusion of the risk analysis in the standard setting process.

MR. JORDAN: It's still, you know, -- I'd just like to say that we in the standard setting business got ahold of the term risk a couple of years before the new Administration came into being, so it's not something new with us.

We do have an extensive risk assessment program under development now. And, hopefully, sometime about the middle of next year that program should have its first test in an environment to see if we can, in fact, set ambient air quality standards based on risk. And have medical people tell us what the risk of being exposed to this level, or this pollutant, is, for these kinds of health effects.

I am hopeful that the work that Tom and Bern spoke of here will give us some framework then, as we go to try to implement the risk program in setting the standard, to go to the medical people, and talk to them in terms of what does it mean to be exposed to the pollutant four, five, six, and seven times, in that sense.

And so that ultimately, if everything worked out, you could probably get this standard set into a stochastic concept also.

MR. TIKVART: If there are no further questions,

I'd like to bring this morning's session to a close.

I'd like to thank you for your patience with us, because we had an awful lot of information to present to you.

This afternoon we will deal with a summary and a panel discussion of the Airlie House workshop. This afternoon's session will begin, promptly, at two o'clock.

Panelists, please meet with Shep Burton in the back now.

(Whereupon the morning's session was adjourned at 12:28 p. m.)

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CERTIFICATE OF REPORTER

I hereby certify that the foregoing transcript represents the full and complete proceedings of the 8/10/81 aforementioned matter, as reported and reduced to typewriting under my direct supervision.

Neal R. Gross

NEAL R. GROSS

GOVERNMENT OF THE UNITED STATES

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ENVIRONMENTAL PROTECTION AGENCY

- - -

SECOND CONFERENCE ON AIR QUALITY MODELING

- - -

MONDAY, AUGUST 10, 1981

- - -

AFTERNOON SESSION

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The conference was held in the Thomas Jefferson Auditorium, South Agriculture Building, 14th Street and Independence Avenue, S.W., Washington, D.C., Joseph Tikvart, Chief, Source Receptor Analysis Branch, Conference Chairman, presiding.

- - -

PRESENT: (First Panel)

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JOHN WOOD, Central and Southwest Services
LOWELL VAN VLICK, Tuscon Electric
KENNETH McGUIRE, Kentucky Air Pollution Control
LOU TOSIE
DAVID NOCHEMSON, Los Alamos National Laboratory
ELDEWINS HAYNES, North Carolina Division of Environmental Management
DOUG FOX
JAMES PRESTON, TENNECO

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P R O C E E D I N G S

2:00 p.m.

MR. TIKVART: I would like to begin this afternoon's session, if you all would get settled in, please.

This morning I read you a tentative list of speakers for tomorrow. There has been a number of changes and additions to that list, which I would like to document for you right now. The modified list of speakers is posted outside, but for the record I will read it right now.

For the Government agencies, those who have requested to speak will speak tomorrow morning starting at 9:00 o'clock a.m. in the following order: for the Federal Aviation Administration, Mr. Sundataraman; for the Federal Highway Administration, Doctor Jongedyk and Doctor Carpenter; for the National Oceanic and Atmospheric Administration, Mr. Heffter and Doctor Draxler; for the U.S. Geological Survey, Mr. Goll; for the Nuclear Regulatory Commission, Mr. Markett; for the National Park Service, Mr. Henderson; Department of Energy, Doctor Shull; State of Texas Department of Transportation, Doctor Moe; Ontario Mystery of Environment, Mr. Mishra; U.S.E.P.A., Region Five, Mr. Trout; Department of Health, State of Maryland, Mr. Banta; Louisiana Department of Natural Resources, Air Quality Division, Mr. Raol; and Kentucky

Division of Air Pollution Control, Mr. McGuire.

Individuals who have asked to speak -- and there are some deletions which I will not mention; I will only mention now those who have indicated a desire to speak. Those include -- those who are going to speak tomorrow afternoon will be Alan Witten, Richard Hanson, Robert Kohm, Donald Moon, Jerry Pell, Ray Wright, Ralph Sklarew, Richard Fine, Mitchell Wormbrand, Philip Youngblood.

Those are the list of speakers I have. With the increased list of Government agencies, I doubt that any of the individual speakers will get on tomorrow until after lunch.

One individual on that list has asked for permission to speak early so that he can make another commitment. If there is anybody else who needs to speak early tomorrow afternoon, please inform Charlotte Hopper or Ann Asbeal of that.

I would like to repeat what I said this morning and that is, there is a verbatim transcript being maintained of these proceedings. If anybody would like a copy of that verbatim transcript, they should talk to Miles Anderson, the Reporter here, who is with Neal R. Gross and Company, Incorporated. He will be happy to discuss with you obtaining a copy of the transcript.

Moving into this afternoon's proceedings, we

have a number of presentations and a panel discussion dealing with the workshop on model uncertainty that was held at the Airlie House this last May. This workship was held specifically as input to the Modeling Conference to develop ideas and concepts that you, the attendees at this Modeling Conference, could consider and comment on.

When we originally formulated the concept for this Modeling Conference and what its emphasis should be, we were concerned that it would be very difficult for you to grapple with the still somewhat abstract ideas of dealing with model uncertainty and decision-making; therefore, we got together the group of recognized experts in this general area, if there is such a thing as an expert in model uncertainty and decision-making, a group of invited individuals last May to deal with this complex problem in a relatively closed environment in groups that could deal together and come up with some meaningful ideas and recommendations.

That workshop was organized and chaired essentially by Doctor Shep Burton of Systems Applications, Incorporated. Shep will be the main speaker for -- to summarize the results of that conference this afternoon and will be more or less moderator of the panel discussion.

I would like to introduce the individuals who are participating in the panel discussion this afternoon.

Starting with my right is Doctor Shepherd Burton, Vice President, Systems Applications, Incorporated; next -- and the seat is vacant -- is Mr. Stephen Connolly, President of Schwartz and Connolly; Doctor Bruce Egan, Vice President of Environmental Research and Technology, Incorporated; Doctor Michael Williams, Los Alamos National Laboratory; Mr. James Salvaggio, Chief, Air Quality Planning Section, Pennsylvania Department of Environmental Resources; Doctor Steven Wise, Mobil Research and Development Corporation; Mr. Kontnik of Hunton and Williams; and Tom Helms, who you met this morning, Chief of the Control Programs Operations Branch of Office of Air Quality Planning and Standards.

Before I turn it over to Shep, there are any number of other individuals -- I should not say "any number," because the total was 40 -- but there are a number of other individuals in the audience who participated in the Airlie House workshop. I am sure that Shep would like to share the credit, if I may say credit, for the workshop results with you. Would these individuals stand because I am sure if I tried to name everybody I would miss somebody. Dick Londergan; Don Moon; Vern Walker; Lou Kontnik; Dick Kerch; John Wooten; Doug Fox; Norm Bowne; and Martin Shock there in back. Anybody else?

(No response.)

MR. TIKVART: If you are, you will not share the recognition.

Okay, with that brief introduction, I will turn it over to Shep Burton, and more or less leave this afternoon's proceedings up to him. I will be here as a traffic cop, more or less, and to keep the thing flowing. But, Shep, I would like to turn it over to you now.

DR. BURTON: Thank you, Joe.

Basically, where there is so many of us and we are trying to keep it to, on the average of about ten minutes apiece, I am not going to have that much time, but what I would like to do is to thank Joe and thank everyone for coming and for having us here and indicate that I am really going to serve as a moderator and then -- but prior to that, try to set the tone, provide some background on why we met and what we dealt with there.

As Joe indicated, we were really looking at the role of models in regulatory decision-making and we sort of -- there were actually 49 of us, Joe; some crept in or more EPA people showed up than we really originally thought. However you want to look at it.

As you indicated, there are a lot of people here today that were present and they are out in the audience. One person that you -- a couple that you did not mention that I know were there and are here today, but

probably took a long lunch, Bruce Jordan, Tom Curran and Bern Steigerwald, so let me mention those.

The -- as I mentioned, the workshop was held to address perceived growing concerns associated with using air quality dispersion models as an air quality management tool. Now, in planning the workshop, it was recognized that the practice and the technique of modeling has really grown considerably and in the past ten years there has been an unprecedented increase in the skills associated with modeling. In part, because of what you heard today in just the overall feeling of the participants in the workshop, I think that everyone would agree that we could expect the increase in skills to continue.

If the increase in modeling skill is to produce an increase an not a diminution in the quality of the clean air management decisions, there has to be a corresponding increase in wisdom and parenthetically I might add that the perfect model might still be misapplied in a regulatory or policy-making setting with one consequence being incorrect decisions.

But there has not been as great an increase of wisdom by either the modelers, the decision-makers or the regulatory system they serve. The official rules by which management decisions are made are still those that were initiated in the early 70's.

Notwithstanding this morning's discussion regarding future trends in modeling, modelers still dutifully provide decision-makers with a single concentration estimate located at some place for some brief period of time in a year and regulators still base decisions solely on such estimates with only infrequent and informal consideration of the uncertainties or the implications of such uncertainties in those estimates.

So, skill without wisdom appears to contribute to our problems. If they are to be cured, it will be not by a mere increase of skill, but by the growth of such wisdoms as the time demands -- the times demand.

Thus, the workshop devoted little or no effort to discussions directed at model improvements per se. The efforts of the participants were devoted primarily to identifying from their individual and collective experiences practical recommendations which, if adopted, should, one, reduce the doubts and risks concerning modeling methods employed in and conclusions derived from air quality impact assessments; and, two, should assure the wide acceptance of the use of modeling in air quality management by interested parties and by the public.

Now, the structure of the workshop consisted of three independently functioning groups of approximately equal size who addressed the same four sets of questions.

They addressed those questions and tested their answers or their approaches dealing with those questions against three practical applications: a PSD permitting problem in the oil shale area; a SIP revision problem in the Chicago AQCR dealing with SO_2 and ozone, changes in the SIP; and the transport of pollutants across political boundaries.

Each work group is represented on the panel today and the panelists hold a variety of interests and views about the recommendations, which individually they will share with you. They will discuss the findings of their respective groups, as well as comment to the extent they wish on the workshop recommendations as a whole.

There has not been any strong attempt to really orchestrate the presentations that will be made today. You might read as much into the summary report itself in that it was really a collection of the ideas of the participants and the recommendations of the participants.

Before I do this -- before they do this, let me outline in broad terms the recommendations offered by the workshop. They are five:

First, the utilization of cooperative processes whenever possible that provides for earlier and substantive involvement of interested parties and it encourages the anticipation, definition and resolution of potential

areas of conflict.

Second, the utilization of air quality impact assessment plans or protocols to identify and define models, tasks, analysis steps, data bases, potential disputes and the means for resolving them, and the schedule for accomplishing the impact assessment.

Third, the utilization of advisory groups to provide oversight, guidance and peer review.

Fourth, the explicit stipulation of uncertainties through the best available means in all modeling-related decisions; encouraging modelers and decision-makers to become familiar with methods of estimating uncertainties and the use of those estimates in modeling-related decisions; seeking the identification of measures other than rare events for use in decision-making and identifying other readily available modeling outputs to augment the rare event measure currently used in decision-making.

Fifth, the selection and application of new or modified modeling approaches rather than insisting that existing guideline models be used for all circumstances. Improving the methods used to convey changes in models, methodology and process; consideration of the establishment of a modeling center concept to centralize certain modeling activities and insulate the technical modeling tasks from the political decision-making process.

Now, all of those may sound like a lot of apple pie and motherhood and -- but, in fact, in a lot of instances, or at least instances of which I am aware, they have been practiced and they have worked. They may not work in all circumstances, and I would hope that people here could comment on that.

An additional recommendation that was not advanced by the workshop, but which did appear to me on reading all of the recommendations, to be harmonious with the workshop recommendations involved establishing a modeling-related quality assurance activity in air quality management.

In the summary report you can find, in general terms, not too specifically though, an outline of the principal elements of that air quality modeling -- modeling quality assurance activity.

Now, as Joe indicated, or as I indicated at the beginning -- and I indicated at the beginning, each panelist will discuss the workshop. We have two broad categories of participants and I would like to be able to refrain from putting labels on people, but at this particular point I cannot -- those who can speak for a particular work group as a whole. In other words, they do not really represent interest -- industrial interest, environmental interest or governmental agency interest and so on. They

come from the consulting or the research community.

The second group falls into the category of those who do represent someone, who do have some interest in addition to the broader research interest.

The first two will speak -- Steve and Bruce will speak on the first category and the last group will speak from a special interest. They are all free to comment on any aspect of the recommendations.

Steve?

MR. CONNOLLY: Thank you.

STATEMENT OF STEPHEN CONNOLLY

MR. CONNOLLY: What I would like to do is proceed in the following order: I will state for you what our -- Work Group Number Three -- problem was and talk a little bit about the membership of the work group and its organization; how we proceeded, the approach and methodology; and relate some specific concerns and then finally review briefly our, the group's generalization and specific findings.

Work Group Three's specific problem was the extent to which air quality modeling can assist in the development of a defensible regulatory posture with respect to the issues of the transport of pollutants across interstate political boundaries.

The group started by noting that it felt that

the problem was not to deal with the issue of trans-boundary for its own sake, but rather to explore it only in terms of the principal workshop questions, and as we shall see in a minute, we ended up concluding that the important issue was the long-range or mid-range transport and the modeling with respect to those and not so much the question of the intervention of boundary problems.

In terms of the membership, we had, out of about 15 or 16 members, we had representatives from EPA, including ORD, OAQPS and various regional offices, other departments in the Federal Government -- departmental agencies in the Federal Government, from two state governments, from industry, from the modeling community and from several law firms.

The work group approach and methodology can be characterized first by giving you some background on the context in which the group operated and then talk specifically about the approach and how we proceeded, step-by-step.

The group began by deciding to limit the deliberations to the question of utility of modeling and dealing with long-range and mid-range pollutant transport issues. The short-range transport problems were to be raised in other sections having to do with SIP revision and perhaps with PSD. Therefore, we felt that it would be better to

deal with the mid and long-range and leave the short-range to the other sections.

We also thought that the problems that would be raised by short-range transport were essentially the same modeling issues as would be raised in other contexts such as the SIP modeling and the PSD modeling.

We also focused on the modeling aspect of the long-distance transport problem and not, as I said, the trans-boundary nature of the problem.

We defined mid-range transport as transport over 20 to 200 kilometers. We defined long-range as that extending beyond the 200 kilometers.

Then a set of distinctions were made between modeling problems that the group felt were associated with long-range transport and modeling problems arising out of other scales of modeling.

Long-range must deal with multiple emission sources, in some cases, treating a few sources as individual point sources and other sources as area sources.

Another distinction was that greater difficulty was experienced in identifying and quantifying the contribution of individual point sources than ambient concentrations at any point down wind.

There is greater difficulty defining appropriate source groupings and geographic boundaries for area sources

treated in the models. It was agreed, however, that the use of longer averaging times tend to reduce the relative uncertainties in all model predictions, including those of long-range models.

Because of the number of sources and the distances involved, the data needed for long-range transport models -- that is, emissions, wind speeds and directions, deposition rates and so forth -- are frequently both more numerous and more difficult to acquire than it is for shorter-range modeling. Few, if any, well-defined procedures or precedents governing the use of long-range transport models have been developed for the legal and regulatory arena.

Finally, the air quality problems associated with sulfates, other fine particulates and acid rain precursors and the high stakes involved in regulatory decisions on these problems led the group to explicitly recognize the need for better understanding and further development and application of long-range transport models. A bow towards more research.

In terms of the approach of the group, we basically went through four or five phases or steps. First, we identified a list of issues and questions. We developed these from -- these issues and questions from the perspective of the regulator or the policy-maker.

These were questions and issues with respect to the long-range or mid-range transport which modeling might help eliminate. We basically asked ourselves the question: "What do we need to do if we were regulators or decision-makers? What do we need to do? What do we need to know to do it? What, if anything, can modeling do to help us in knowing this and in doing this?"

The list was designed to help guide the modeler in defining, prioritizing, designing and conducting modeling activities and in presenting modeling results in the form most relevant for the regulatory decision-maker.

Step Two, the group identified model capabilities with respect to long-range and mid-range pollutant transport. What we did was to develop a matrix which displayed model capabilities by general model type. This would be used to assist not only the modeler, which is obvious, but also the policy-maker or the regulator in determining and choosing the model most capable of dealing with the long-range, mid-range transport problems.

Step Three, the groups stepped back and attempted to identify attributes which can help determine the legitimacy of the regulatory process and the use of models in it. The work group determined that it would be useful to generate a list of the general attributes which helped to determine whether a regulatory process or the choice and

application of a particular air quality model is accorded legitimacy by all interested parties.

The group -- by legitimacy, the group meant is it accepted as fair and is it likely to produce outputs which all interested parties or most interested parties will accept. Such a list of "processed legitimacy attributes" was then generated. It would be used by policy-makers, regulators and modelers in attempting to choose regulatory approaches and tools which are most likely to be accorded legitimacy.

Step Four and Step Five togehter really was the generation of responses to the principal workshop questions and then generalizations and findings. To do this, we split into two sub-groups. Each answered the four principal work group questions; one with respect to long-range modeling and the other with respect to mid-range modeling.

Upon completion of their separate tasks, the two sub-groups reconvened as one, developed a consolidated set of responses to the four questions and then a set of generalizations and findings regarding the role and use of mid to long-range transport models within a regulatory framework.

Before I review some of those findings, let me just touch on the issue of legitimacy of current application of modeling and uses of model output in the

regulatory setting.

Work Group Three determined that the policy-maker, the regulator and the modeler each should be keenly sensitive to the need to assure the wide acceptance by interested parties and by the public and of the regulator decisions and processes and the application of technical tools in those decisions and processes.

It is this widespread acceptance, this legitimacy that assures that the process of government and the public policy decisions those processes produce is accepted by the public. The work group developed a list of attributes which were not intended to be exhaustive, but more as examples of process attributes which may legitimate that process and which should be attempted to be maximized by anyone who is attempting to operate such a process.

It was recognized to a certain extent some of these attributes may be opposite poles with the same continuim. It was urged by the work group that policy-makers, regulators and modelers consciously consider and balance each of these attributes and others which they may subsequently develop in constructing a regulatory process or selecting and using air quality models.

Let me give you examples of some of these attributes. They are not listed in any certain -- in any necessary order of priority. One would be the

minimization of resource use; two would be the maximization of simplicity; three, assurance of timeliness; four, consistency of process and results within a process and across various processes; five, accessibility for interested parties; six, that they are understandable to participants and to observers alike; seven, that they are both flexible and adaptable; eight, that they have the ability to handle uncertainty and express it well -- to identify it and express it well; nine, the ability to handle change, both in the process and in the inputs; ten, that it is a defensible process producing defensible results; eleven, that it can be publicly documented; twelve, that it is administratively easy or relatively so; thirteen, that there is ample opportunity for public participation, but without undue delay; fourteen, that it is scientifically valid.

The work group also recognized that the use of models and model output in a regulatory setting is more likely to be accorded broad acceptance and legitimacy if cooperative processes were utilized. Such processes will be particularly important in assisting the technical and policy analysts that make it clear to participants and the public the capability and limits of models in assisting the decision-maker in using the model results.

The process, whenever feasible therefore, should

incorporate adequate opportunity for early involvement of the interested parties, in a cooperative effort to anticipate and to define areas of potential conflict and areas of potential agreement and to narrow disputes and focus cooperative efforts on the early resolution of these disputes.

Let me review for you some of the key generalizations and findings. Within fairly large areas -- the example we cited was 50 X 50 kilometers -- estimates derived from long-range transport models by themselves are decreasingly useful for allocating control levels to specific individual sources. However, it does not follow that these models have no contribution to make to regulatory policy decisions. The group found that emissions from one grid square may have a significantly different impact on concentration levels in a given receptor area than emissions from another grid square.

Furthermore, emissions released on a windy winter day may contribute significantly less than emissions on a stagnant summer day. Consequently, long-range models can provide information on a regional scale and may be expected to quantify the relative contribution of a particular source's or area's emissions to pollutant concentrations in another area.

The air quality model application and selection

criteria for the medium scale and long-range scale pollutant transport and transformation evaluation are not appreciably different, the group found, except greater sources -- greater source and receptor geographic resolutions is obtained for the mid-range and for the long-range. Overall, uncertainties are less for mid-range models than for long-range models. These uncertainties typically decrease as averaging times increase.

As we go through these findings, I think you will note that, not surprisingly, some of them will sound fairly obvious; that, however, does not detract from their importance as a concensus finding of a working group of very different experiences and interests.

Another finding was that the availability and magnitude of data processing resources, both hardware and staff, technical know-how of the user in time and financial resources can significantly affect the resolution and uncertainty of the modeling predictions in any specific application. These factors must be taken into account in the process of it, to be legitimate.

Exceptability/defensibility of long-range transport models depends upon the level at which and the number of times model evaluations have been performed. This is true to a greater extent than for the more traditional and widely-used short-range model. The intended use of

long-range transport models should determine whether and how such previous model validation is required as a selection criteria.

Regulatory activity should rely on those models which have demonstrated that that is the model evaluation results.

Appropriate procedures for the validation and testing of MESO scale and long-range transport models should be developed and agreed upon by a reputable scientific group with extensive peer review.

Current lack of data and lack of adequate techniques make it difficult to identify and examine all sources of uncertainty in long-range transport models. However, sensitivity analysis, if explicitly developed and documented, combined with knowledge of the uncertainties and input data, model parameters and model algorithms will assist the decision-maker to identify, understand and express for the regulator the associated uncertainties arising in model results.

Dependence on single-number, numerical representations that do not express uncertainty should be avoided. Attention to graphical representations of model results may be particularly important for assisting regulatory decision-makers in understanding long-range transport model results; thus, for expressing their associated

uncertainties and also to demonstrate to the regulator what other information the modeling has generated other than simply that -- this one high number.

Uncertainties should be recognized and expressed at all levels of the decision process. Present regulatory process and structure, in some respects, is not well constructed to deal well with the uncertainty that is inherent in the use of models and model outputs. Therefore, dealing adequately with this uncertainty may require revisions of the regulatory process and structure.

It should be recognized that the use of uncertainty may be subject to misunderstanding, to misuse or to abuse as this information is passed up through the decision process. In order to limit the potential for such misunderstanding, misuse or abuse, decision-makers should be required to explicitly state on the record how he intends to take uncertainty into account and to identify, describe and balance, among other, non-modeling considerations that affect his decision.

One important response to dealing with uncertainty in model results and in the entire regulatory process should be the adoption where feasible of a cooperative process which helps the modelers and the decision-makers to find areas of agreement and disagreement and to focus on narrow issues of conflict.

Decision-makers should have the responsibility to recognize, to take into account the various levels of uncertainty associated with model results. They should make reasonable efforts to understand the effects of model uncertainty upon their capability, to determine the relative effectiveness of alternative control strategies for regulatory policies.

The last two, because of the regional nature of pollutant transport, statutory and regulatory frameworks, that is, the Clean Air Act itself and the implementation regulations, should more fully recognize the nature and implications of these regional transport processes.

Long and mid-range transport models are expected to undergo rapid development and improvements in the foreseeable future. The process may result initially in more complex models that are data-intensive. These models may require a high degree of technical skill to understand and to effectively operate the models. The cost of using these models could be high, compared with using short-range models. Such factors, obviously, may prohibit some users from having access to the better models or to the latest improvements.

One means of helping to reduce any potential problems would be to establish centralized air quality modeling center for the more important models and data bases

to be maintained and facilities for using the models to be provided to all interested parties.

We will be able to accept questions, I think.

DR. BURTON: The next speaker is Bruce Egan, who will describe the activities of Work Group One that dealt with the Oil Shale problem.

STATEMENT OF BRUCE EGAN

DR. EGAN: Thank you, Shep.

I am pleased to be here after seven hours on an airplane this morning from Boston. I might have driven if I had had one more hour, I suppose.

The Group One addressed the four workshop questions which were identified earlier in the context of the new source review process in the context of the Oil Shale region. Thus, problems associated with permitting increment sharing and permitting multiple sources located in the region of complex terrain, affecting both Class One and Class Two areas emerged from the discussions.

Much of the discussion within this group was oriented towards the basic need for physical and scientific realism in the application of air quality models and their role in providing information to decision-makers.

Without a solid technical basis, decisions made could not be defended and would be expected to have biases of various forms.

I would like to just briefly overview our deliberations and then go to some vu-graphs for specific items.

The contexts and the feeling -- the understanding was that the Clean Air Act, in effect, requires the air quality management process to rely, in part, on models or modeling techniques which are believed to represent or simulate a sound in scientifically defensible understanding of atmospheric dispersion processes.

It was expressed early in the workshop that because of the inherent turbulent nature of atmospheric processes and because of the limitations of technology, the air quality manager has to deal with the fact that numerical values predicted by any such model will not precisely mirror reality in detail and, therefore, comparisons of predictions with measurement will inevitably demonstrate that any predictions will have some uncertainty.

Sub-group One took the view that the presence of uncertainty by itself should not deter the air quality manager from using model results in the decision-making process, provided the model represented a scientifically sound simulation of the relevant adversary processes. The model, in essence, is asked to provide the best estimate of what will occur based on current scientific knowledge. The decision-maker, in turn, needs to incorporate

these findings, together with other information, into the decision-making process.

This context, Sub-group One discussion focused considerable attention on what should be done to provide the decision-maker with the most appropriate information.

The models used and their input data should be scientifically justified and technically appropriate. This is basic to providing confidence to the decision-maker on the scientific merit of the subsequent uses.

The group addressed what alternative output data, that is, alternative to that presently required, might form a better basis for air management decisions, especially alternatives which might reduce uncertainty.

Common practice in the making of decisions in the presence of uncertainty is to call for additional, sometimes peripheral, information to help frame the most defensible decision.

The Sub-group One report identifies additional information presently available in the routine output of computerized air quality models which could assist the decision-makers in this regard. The group also identified other information which can be obtained with only minor modifications to presently available models and which would further provide useful supplemental information to the decision-making process.

The group further discussed how information on the uncertainty levels of predicted advice might be provided by modelists and endorses the concepts of the need to have uncertainty information available.

However, two cautions were raised: that, one, it is difficult to do at the present time, that is, to provide information on uncertainty levels; and, second, that the process of decision-making may not necessarily be improved until guidance is provided to decision-makers on how this information should be used.

The report also draws attention to the upgrading of models and the need for that and mechanisms for such upgrading are outlined.

Let me now talk over a couple of the vu-graphs.

I have stated this viewpoint in my opening remarks, that basically the model, if it provides a scientifically sound simulation of relevant atmospheric processes, one would have uncertainty and one must learn to live with it in some way. Then, of course, the question is what is the magnitude of that. But from that context, the recommendations, general ones, I would summarize, strive for the most scientifically sound models; identify model output of better use to decision-makers; and have uncertainty in model predictions factored into the decision-making process.

We addressed four different questions basically. On the matter of the choice of models and this was in the context of the Oil Shale PSD Program, that conservatism in models presents problems and especially with respect to increment tracking and whereas one can be conservative for a number of reasons for a single source, for multiple sources, the model would probably not only over-estimate the magnitude of impact of a single source, but would mis-locate that magnitude such that when you have multiple sources, you have some trouble in the increment tracking process.

More generally, appropriate models would have this list of attributes not in necessary order of priority: availability, of course, because of the regulatory setting, and adequacy of documentation from the same point of view; previous model performance, in a positive sense, of course; that the input data requirements would match the type of input data available for use; cost of computations be some-what reasonable; simplicity by itself is an attribute in the sense that because of the regulatory aspect and the number of deliberations, it is much easier to deal with a simple model if you can than a more complex one; scien-tific validity and acceptability, very important that pre-vious use of it would demonstrate validity and overall accuracy and/or bias in outputs could be identified in

some way.

Now, at the end of the list we enter right into the matter of how does one provide uncertainty.

In the use of models, I think all of the groups came upon the conclusion that an agreed upon protocol was very desirable for complicated regulatory settings and in this particular case a protocol which selected the models as best they could beforehand which defined the meteorological and emission state or input requirements beforehand and which defined what performance evaluations would be -- would take place and what would be the measures of performance presumably, if possible, would all be agreed upon beforehand.

Overall, the theme was that scientific credibility regarding the representativeness and quality of the input data was important.

We rephrased this question slightly to what we really answered in our panel, which was how can uncertainties be dealt with. This was going beyond simply how do you quantify uncertainty, but the general notions of the need to reduce the uncertainty by improving models and/or by using models to predict quantities with more certainty. I will come to that in a moment.

A general feeling in the context of the above would be to strive generally for stating model predictions

in a probabilistic sense. Some discussion, I missed this morning, but you have heard about "XX" of the methodology. We are talking about basically having people think about model output in a statistical sense rather than as a simple worse case does it or does it not excede a standard type of thing in the absolute sense, supply additional information to decision-makers. Here, we were addressing the fact that presently, a typical use, a model is used through a computer to generate a large batch of information, of which only really one number -- the highest, second highest value at the worst receptor is used for the decision-making process.

The output, we feel, is rich in other information. We have some confidence that your model mirrors what is going on and the decision-maker should be very interested in, for example, the frequency of values which are, say, 80 or 90 percent of the standard or the increment, or generally the frequency distribution of high values.

The average of the top ten or, say, 20 highest values at the worst receptors, to give you some idea of how extreme the extremely high values are and how often they occur.

The episodic nature of the highest values. Do they occur randomly through the year or do they occur

sequentially with different sort of health implications? The location and extent of the geographic areas where standards are where increments are mostly likely threatened. Is it a remote area, largely unpopulated or it is an urbanized area?

Finally, we identified the fact that models can compute dosages; that is, concentrations times population density. We shied away from identifying this as a specific recommendation because it does imply a fundamental change of the motivation behind the Clean Air Act to consider looking at such matters, but as supplemental information, we think decision-makers would be interested in that.

On the item of reducing uncertainty by improving models and predicting quantities of more certainty, for example, there was identified that the 95th percentile or the 98th percentile is a more predictable event than the highest, second highest. So, again, try to reduce uncertainty by having models address something which they can do a better job of.

On Item Four, provide estimates of uncertainty associated with model predictions and the caveats I mentioned earlier about the real difficulty of coming up with numbers at the present time -- this does imply more model validation studies, more comparisons of models with data.

There is a separate question of how the

information would be used by decision-makers. We made one suggestion which was -- in terms of looking for a variance, to compare a variance with, if you will, -- if you think of a model as predicting a mean value of an expected rare event and if one were to provide air bars about that value, a measure is obviously the standard and another measure of the air bars would be to look at the air bars or uncertainty factors of safety, if you will, associated with the standard that you are looking at.

This would be -- and you could scale this to be anything you would like, one-tenth of the factor of safety or whatever. But it is obviously -- decision-makers need to have measures to compare the uncertainty with.

And Five, encourage decision-makers to deal with uncertainties in this type of argument I have been making.

Finally, our group looked at the process of changing models. How do you incorporate changes of models -- modeling methods to the decision-making process and we identified two things I have summarized here; that EPA should focus consistency needs on the process for selection of models rather than the models themselves. This came up in the context of the Oil Shale permitting problem that basically what was needed was a protocol and agreement of how to proceed, given the complexity and the

known deficiencies of existing models in that circumstance.

Secondly, that a review board, if you will, which should be established within EPA staff, would review new models and such a review would have an overview function -- an overview function would be provided for such review by a standing review committee of both government and non-government representatives.

I think that completes my summary of our deliberations.

I guess I will introduce Mike Williams, an analyst in my group. I should mention that Doctor Williams and Doctor Wise were both members of the Group One Panel.

STATEMENT OF DOCTOR MICHAEL D. WILLIAMS

DR. WILLIAMS: First of all, I should clarify, I am not representing Los Alamos National Laboratory for the purpose of this conference. The opinions I will give you are strictly my own. They are probably colored more with my past experience with environmental groups than they are with current activities with the Laboratory.

I have a number of concerns about the workshop. I think we had some useful ideas, but there were some potential dangers that I would like to go through a little bit in discussing some of our recommendations.

On the broader set of material in the workshop

summary, there were a few other issues that I felt worth commenting on and I will add those in, too. For instance, in Section III-2, it talks about principal issues emanating from the current regulatory use in models and talks about how management decisions are made.

In many instances, air quality management decisions are based on a single number; however, in the case of the Class I PSD considerations, the act is very explicit that a decision on increment consumption is only one of a part of a larger decision to permit facility siting. This provision in the law, to date, seems to have been ignored, although it appears to be consistent with many of the improvements in air quality management decisions which critics have been advocating.

Perhaps more attention should be given -- directed to implementing the air quality related values test which are part of the act and less on altering the increments or the modeling approaches.

Second, I have not found that most environmentalists believe that all uncertainty should be given to protecting the environment. In fact, I have observed environmentalists generally want to use the best unbiased estimate for decision-making. In some areas where models such as CRSTER have indicated under predictions and flat terrain, particularly the 24-hour average, for instance,

environmentalists have advocated making adjustments in measured results to reflect the apparent bias of the models predictions, but the emphasis is usually on a good, unbiased estimate.

The suggestion that in marginal situations all national interests, not just environmental protection, should be considered in the final decision has some difficulties. First of all, who makes and based on what process a decision that a project is in the national interest? Would the same test be applied in marginal compliance, that is, the facility would appear to meet the increments, as well as in marginal non-compliance?

Currently, facility siting is examined in a few limited contexts and there is no determination of the national interest. Suggestion that the decision be based on the national interest is a suggestion that we make a major overhaul of our decision-making apparatus with respect to facility siting. It would seem to unavoidably make the decision-making more cumbersome with a great deal more government intervention. In many cases, I doubt that groups such as major mining interests would be willing to open their books to permit a full exploration of the economics of a particular siting decision.

Getting to the sections that relate more to some of our work, Section 4.2, as long as the screening models

are truly conservative, they should be useful tools which do not create undue confusion. These models merely allow us to make inexpensive decisions now and deal with the more difficult problems later.

As long as all plants relative to this decision are considered with the appropriate model, when it is time to make the difficult decision, no confusion should arise.

Also, in Section 4.2 I understand that some regulatory agencies tend to consider best available control technology as new source performance standards plus what is needed to make the increments. I do not believe this is the intent of the Clean Air Act. It seems to me that that was intended to be independent of the dispersion modeling. If this is the case, the screening model used should have no impact on best available control technology decisions.

Specifically, with respect to the Work Group One report, Section 4.4 dealing with incorporating uncertainties, I felt a lot of the material in there was quite useful, but I am very concerned about a couple of aspects of it.

One of my major concerns is with the suggestion that we should change the short-term increment from a second highest to a 95th percentile or to the average of the ten highest. I believe there are several things wrong

with such an approach, including, one, change will move us further from the actual basis for the increments or standards; two, the change will even -- will be even less meaningful to the lay audience; three, the modeling will probably be less readily tested against measurements; and, four, such a change will benefit isolated major sources relative to complexes of sources.

In the case of ambient standards, the basis is generally -- generally affects data which shows that above certain levels of concentration, damages have to occur. The use of the highest, second highest is already a step away from the primary data. A change to the average of the highest, of the ten highest or to the 95th percentile would be an even greater change from the primary data.

In the context of ambient standards, I doubt if there is any justification for such a change. In the case of the Class II PSD increments, there is still a relationship between levels and damages.

I know several of you are thinking that secondary standard is set up -- set at the lowest levels at which damage can occur. However, if you examine the revised SO_2 criteria document and look at the chapter on Vegetation damage, you will find several cases in which SO_2, in combination with ozone or nitrogenoxide, damaged vegetation in three to four hour exposures at about 0.10

or ten part per million of SO_2 to two-tenths of a part per million of SO_2.

As our principal reference suggested, damage can occur at levels comparable to the Class II increments.

In the instance of the Class I increments, the function in the increment is, again, merely to define who carries the burden of proof. In view of this situation, it seems that there is no reason to change the Class I increment without changing the others. In other words, you generally like them to be consistent. Class I already has an appropriate decision-making mechanism built into it anyway.

It is currently difficult to explain the meaning and the rationale for the highest, second highest basis. It becomes even more difficult, that is to a lay audience, -- it becomes even more difficult to explain the 95th percentile or the highest average of the ten highest.

I believe the modelers should eliminate as many barriers to public understanding as possible and the proposed change would instead add yet another.

Using either the 95th percentile or the highest ten will mean that we are predicting a lower concentration which is readily compounded with instrument threshold or poorly characterized background concentrations. I believe that accurate measurements are the highest in the 95th --

of the ten highest in the 95th percentile will be more difficult; consequently, there will be less accurate testing of the predictions.

Finally, change to the 95th percentile or the ten highest will change the current set of equities. In the NCAQ 4-corners study we found that the ratio between the highest and second highest -- between the highest, second highest and the 95th percentile varied markedly from one site and source configuration to another. Such a change will benefit the isolated sources at the expenses of complexes resources or sources with higher wind frequencies in critical directions.

That is, basically we are changing the rules somewhat if we change this kind of increment. It is impossible to define a current -- an increment that is exactly equivalent to the highest, second highest in terms of the 95th percentile or the average of the ten highest.

In addition, we will have intended to make the short-term restrictions more nearly redundant with the annual restrictions and, thus, lost some of the control over the entire frequency distribution.

It is not evident to me that the changes suggested here will produce greater accuracy. In NCAQ 4-corner study, we found that the center line concentrations -- and these were based on measured data -- were relatively

insensitive to changes in meteorological changes. You had about -- with about the -- the restriction that we are talking about, the same stability classes. Within that range, you could change the wind direction and wind speed very significantly with little change in the concentrations.

While, of course, the concentrations were very sensitive to the wind direction for any particular monitor. Changes -- well, the center line would not be. Changes discussed here will move us away from the center line concentration, which appears to be relatively stable and toward the off center line conditions, which are not stable.

Another suggestion in Work Group One with which I disagree is the use of exposure or dosage estimates. Use of dosage estimates implicitly alters the right of the individual to clean air. It assumes that people in large communities have a greater right to clean air than people in small communities.

This is a major change in the concept of the Clean Air Act and I think it should not be made under the guise of a technical change.

The use of dosage estimates also implies that effects are linear with those seeking independent concentrations. In the case of NO_2, where concentrations are

on the order of a part per million, this is not the case. I expect that other contaminants may have a similar effect.

It seems to me that major changes are being suggested here which alter the equities between sources and the rights of the individuals under the guise of a technical improvement. I do not believe that such changes necessarily lead to significant verifiable improvements in model prediction. I also do not believe that the basic changes we are discussing here should be the sole purview of the air dispersion modeling community.

Many of the other suggestions we went through I think are very helpful and would be useful for decision-makers and represent contributions to the air quality management picture.

MR. BURTON: Jim Salvaggio.

STATEMENT OF JAMES SALVAGGIO

MR. SALVAGGIO: I would like to talk upon part of a subject that we discussed at the conference. It came up a number of times today already, and that is the use of uncertainty once you get into the decision-making process.

Since the early 1970's when the Clean Air Act was adopted, the entire decision-making process in air quality-related matters has been dominated by air quality modeling. Discussions on air quality modeling, in turn,

has centered on the technical factors involved in model selection and application. Such issues as worst case scenario, terrain adjustment, mixing height and fumigation analysis have been at the center of the debate. In the past, these issues were often debated in the decision-making process as if the underlying model uncertainty could be resolved.

This situation occurs because the air quality management process, as embodied in the Clean Air Act, is based on modeling. It requires that models be used to precisely predict relationships between emissions and air quality. It implies that if the existing models are unable to provide these precise relationships, then improved models will have to be developed.

This philosophy has resulted in modeling being the final determinant in many decision-making exercises. For this reason, air quality management has tended to concentrate on the technical issues related to modeling. Likewise, the decision-making process centers on academic exercises in diffusion modeling. The perception of the decision-maker and interested parties is narrowed. Bounds are placed on the range of problems and solutions. These bounds are intentional. Their purpose is to force the decision-maker to focus on meeting the goals of the Clean Air Act rather than wandering too far toward judging other

societal issues such as employment, cost and energy consumption.

This focus on air quality modeling has led to controversy. Over the years the air quality management community has come to recognize that modeling uncertainty cannot be eliminated or reduced to insignificance. This has created problems for both the modeler and the decision-maker. It has frustrated achieving the Clean Air Act goals by causing delays and no decisions. It has also confused and frustrated attempts to efficiently administer the Clean Air Act.

Recognition of modeling uncertainty in the decision-making process will deemphasize the importance of air quality modeling. It will no longer be the dominant factor in the decision-making process.

This is a first important step in air quality management. I am not saying that formal recognition of uncertainty in air quality modeling results in controversies and improve the decision-making process; on the contrary, it will merely shift some of the controversy from the technical aspects of modeling to societal issues.

The decision-making process will have to balance these societal issues with modeling uncertainty. It will have to more formally recognize and discuss these issues in the public forum. At times, it will be forced to

resolve conflicting societal needs. If the regulatory decision-making process is to be improved, this shift will have to be recognized. Policy, managerial and organizational changes, will have to be made which recognize modeling uncertainty.

There are a number of critical areas in which these policy managerial and organizational changes will be necessary. One is the requirement for demonstrations of attainment of the National ambient air quality standards and PSD increments.

In general, the current Clean Air Act and EPA requirements fail to formally recognize that attainment of air quality standards or PSD increments cannot be demonstrated with absolute certainty. In fact, the permitting of a new plant for promulgation of a control strategy attainment of the National standards requires such an absolute demonstration.

In general, these demonstrations are based on past meteorological conditions and anticipated pollution emission characteristics. In some cases, the demonstrations are further complicated by complex terrain or meteorological situations that are beyond the state of the art of air quality modeling.

Future events and situations beyond the state of the art of modeling cannot be predicted with certainty.

Nevertheless, the decision-maker must make an absolute decision. For example, he has to approve the construction of a new plant or not approve it. He has to implement a control strategy or not implement it.

Currently, decision-making process comes to a halt until the modeler produces a document that has an illusion of certainty. Formal incorporation of uncertainty in modeling results will force the decision-maker to make decisions without this illusion of certainty. He will have to formally balance the uncertainty inherent in the air quality demonstration with issues such as the stringency of emission controls, employment, energy and cost.

The current decision-making process is not formally permitted to balance these issues. If the role of air quality modeling in the decision-making process is to be improved, then the probability or statistical nature of air quality management must be effectively incorporated to the decision-making process and the decision-making process permitted to formally consider societal factors.

A second closely related problem is open and formal recognition that the decision-making process must proceed in spite of the limitations imposed by state of the art of air quality modeling. It should not surprise anyone that the lack of universally accepted source receptor relationships in many air quality problems have been

used as an excuse to delay incurring costs associated with their pollution control.

Considerable latitude exists in choosing the assumptions and input parameters for models. The incorporation of uncertainty and the modeling results will add another technical element to the list which experts debate and lawyers litigate. Lack of scientific certainty on source receptor relationships and the probability of violations should not be permitted to delay resolving environmental problems. Modeling results will have to be recognized as a tool to aid the decision-maker; not the final determinant. The decision-making process will have to be structured to focus on environmental problems and societal issues.

The technical aspects of modeling will have to take a secondary role. The final decision will have to be based on a balancing of issues related to public health and welfare, employment, cost and energy.

The third area of concern is equity and standardization. The incorporation of modeling uncertainty in the decision-making process will increase the need for equity and standardization among EPA regions and among states. Even if emissions are considered to only affect immediate vicinities of the emitting source, the issue of national equity and standradization is still important.

The PSD, and new source review programs, with their reliance on air quality modeling, affect the competition for new plants and economic growth. It can have major effects on the nature and location of economic growth.

Emissions, however, do not only affect air quality in the immediate vicinity of the source; emissions can be transported across state boundaries and over long distances.

Consumption of air resources and degredation of the environment in down-wind areas can occur. How meaningfully can the decision-maker in one state balance societal issues and modeling uncertainty as it affects a down-wind state? How often will a decision-maker in the emitting state unilaterally impose additional cost and burdens on his own state for benefits that will be realized in another state? Although equity and standardization problems already exist, they will be even more difficult to deal with when uncertainty is formally incorporated into modeling results.

Without national equity and standardization, the decision-making process will regress to the lowest common denominator. Further, confusion and controversy will result. Degradation of the decision-making process will occur, rather than desired improvements.

Incorporation of modeling uncertainty must be accompanied by equity and standardization improvements in the decision-making process.

In closing, I want to reemphasize that formal incorporation of uncertainty in a modeling results will not eliminate the controversy surrounding the use of air quality models; it will merely shift the center of the controversy. This could, in fact, result in degradation of the decision-making process rather than the desired improvements.

If this degradation is to be prevented in policy, institutional and organizational elements of the decision-making process, it must also be changed to accommodate the shift in emphasis. These changes should take place at the same time that modeling uncertainty is incorporated in the process.

Thank you.

DR. BURTON: Steve Wise, from Mobil.

STATEMENT OF STEVEN WISE

DR. WISE: As a member of Work Group One, as ascribed by Bruce Egan, I participated in the discussions surrounding the Oil Shale issue. However, many of the findings and discussions surrounded issues that were equally applicable to most industrial applications models.

For the most part, we found that the summary

report that SAI has put together did reflect an accurate representation of the deliberations and I am happy to say that we in industry have generally agreed with most of them.

Industrial use of models surrounds not only the regulatory permitting process, but it also surrounds the problem of design of new facilities. Therefore, not only do the model outputs have to satisfy the regulator, they have to provide some certainty to the builder or the designer of new facilities that what he is designing is going to perform as it was designed to do.

This means that models have to -- we have to have some confidence in model predictions; not just that they are going to predict second high results at some point in time and space, but we do have to know where in time and where in space that these predictions will be accurate.

For instance, the first slide indicates what the regulator would call a perfect model prediction. This represents a model study of the -- an industrial data base as being increasingly used by model developers that was put together at the Dow Midland Complex. It represents two power sources indicated by the west and the south, inside the plant boundaries, and nine monitors surrounding the plant boundaries.

There is also at the nuclear site, indicated at

the south boundary, an on-site MET Station. The diversity of the data that is available within this source has been described in several places. API was interested in looking at two model comparisons that would, in effect -- might be used in an industrial complex approximating a refinery.

This is probably the most complete data base that exists anywhere of this nature.

We looked, in this case, at the RAM Model which was an approved model at the time the study started and the IC Model which was on the scene, and I guess is still on the scene.

The "off's" and the "on's" refer to the method by which atmospheric stability was determined. The "off's" refer to off-site stability determined in an airport approximately 14 miles to the south and east of the site; the "on's" refer to stability being calculated through the older method, using 10 and 16 year wind shear available on the site.

The RAM Model used the urban coefficient and the RAM-R Model used the rural coefficient and the ICS Model was run in the rural mode.

As you can see, only the highest prediction was missed by the ICS Model, although the RAM Model managed to include it. Both models used actual hourly emissions. This is a little bit different from the regulatory version

of ISC because it really only uses average emissions.

Next, this vu-graph indicates for SO_2, of course, the three-hour and 24-hour values are what was important and even there, the ISC is doing a better job. Here is -- of 24-hour was actually hitting or exceeding the predictions or exceeding the actual emissions.

This is what we would call the perfection in model application in the regulatory sense. However, from an industrial point of view, we need to know where around that site we might have problems should we want to do further development or should another developer come within range of our site.

The next slide shows the problems that we can, therefore, get into. This looks at only the bullock monitor at the south and west corner of the site. Here you notice that the "X's" which are lost in these legends at the bottom right-hand side of the slide are for the most part over predicted by all three versions of the model, although the RAM Model comes pretty close for the highest two predictions.

If we look at the next slide, however, on the 24-hour and three-hour basis, we are in big trouble because not only are we out a prediction with the ISC Model, we are out greater than -- quite a ways, shall I say.

As we move around the plant site, however, in

the next slide, you see we begin to do pretty well again. As we go west -- east of the plant, again, the models seem to be predicting fairly well. The ISC Model and the RAM Model, the Ram-Rural Model is falling in line again. The next slide shows the 24-hour and three-hour averages -- again, we would call this a good model representation of the high values observed at those monitors.

The final slide, however, shows that we can get in trouble, particularly -- no, that is -- I think we are out of order. Keep going.

The final slide shows what can happen, however, under certain conditions. Here the ISC Model is under-predicting. This, we do not want either, particularly in this instance, since the Austin Monitor is in the city of Midland and the monitor directly north of it also was doing the same kind of thing.

We also have a problem, although the rural versions of RAM seem to be doing okay. It is the urban version -- I mean the rural version -- excuse me. The urban versions of RAM seem to be doing okay. It is the ISC Model that we are principally interested in in this instance because it contains provisions specifically aimed at a site like this which is the building down-wash outrythm.

Here, we have determined that it is not working

particularly well in this instance and it is probably leading to the overpredictions.

This leads to another point. This study has shown, first of all, that in a regulatory sense, a model does exist which might well be recommended by us for an industrial complex use. However, we have to recommend it with great reservations. There are problems with it and we have to know what those problems are and we have to know under what conditions those problems exist, to avoid getting in trouble not from a regulatory sense, but from an internal design sense. We just have to know better how those models behave.

Thank you.

STATEMENT OF LEWIS T. KONTNIK

MR. KONTNIK: I am Lew Kontnik with Hunton and Williams. I am here speaking, at least in part, on behalf of the Utility Air Regulatory Group. Steven and I had a bargain that would get Shep his exercise in Steve's presentation and I will not have -- I will try and make it quick by saying that we all recognize that air quality management is basically what we have been dealing with since the Act was amended in 1970, in the PSD area, in the visibility area and in the standards area.

Uncertainty inherent in model predictions creates a central dilemma in the management process.

Policymakers and decision-makers are often tempted to overlook this uncertainty, but that does not solve the dilemma; it just disguises it.

UARG believes that air quality management programs must be designed and implemented in a manner that uses models as the tools they are but does not demand the impossible of them. Models should be used to help investigate reality; they should not be used in a solipsistic manner to create reality.

Models should be used to aid in the air quality decision-making process; they should not become the decision-maker by default. We believe that realism and informed judgment, not computer printouts, should guide air quality management decisions.

Looking first at the design of air quality management programs, the basic tenet of the air quality management program is that ambient air quality, not merely emissions, should be regulated in order to protect the public health, welfare or aesthetic values.

The Clean Air Act requires EPA to protect these values with some limited margins of safety; it does not authorize the Agency to reduce ambient air pollution below these levels without legal justification.

This implies, however, that any effective air quality management program must strive to represent the

real world ambient pollution concentrations as accurately as possible.

The PSD Program is a classic example of a program which misuses models by demanding too much from them and by making them too important. The PSD divorces models from reality and creates an artificial system defined only by models.

The program ignores the emissions of certain sources, while modifying emissions from others. These features of the PSD frequently make real world comparisons between modeled and monitored results impossible. Moreover, because there is no link between the calculated concentrations and actual concentrations, any real world benefits from the program are largely fortuitous.

The PSD Program, with its tertiary standard increment system further shuns reality by requiring decision-makers to account for ambient air quality concentrations that are too small or too ill-defined to be monitored.

The Class I increments are below the threshold of the most sophisticated monitoring equipment and visibility cannot be monitored in a psychophysiologically meaningful manner. Finally, the PSD increments themselves have no firm epidemiological or welfare effects basis.

The utility industry has long opposed the current

PSD Program for a variety of reasons. And, while this is not an appropriate forum to debate the legislative alternatives to the present PSD Program, it is, I believe, the proper forum in which to point out, as I just have, those abuses of models which make it an unworkable program.

UARG believes that the agency and Congress should recognize these defects and should not extend them into new programs when the Clean Air Act amendments are discussed. Specifically, Congress should be cautious in the current debate over long-range transport to avoid divorcing models from the real world air quality conditions which are the basis of the legislative concern.

Models should be asked to provide information which they can provide; they should not be used to create new, artificial parameters such as the PSD increment system.

Now, I would like to turn to model use in the decision-making process under our current regulatory programs. Decision-makers use models in different ways; too often they are treated as predictions of immutable facts.

We believe that realism and rationality should guide the decision-making process within our current air quality management programs.

Decision-makers who administer our current programs must consider the inherent limitations of modeling

results together with the social value of the goals being protected and the potential burdens imposed by the decision.

Not to do so would be irrational and to claim that it is not done would be naive. UARG believes that the following examples illustrate a rational approach to using modeling results in the decision-making process.

Thinking first about attainment area designations. Non-attainment designations should not be based on modeling information alone. If computer models predict violations of a standard in a particular region, but no such violations have been recorded by monitoring, the agency should designate the region as unclassifiable and should undertake monitoring in order to determine whether the violations actually exist.

This area should be redesignated based on the monitoring information according to a specified time table.

Turning next to secondary standards and the "tertiary" standards or PSD increments, because the PSD increments and secondary standards protect values other than the public health, modeling calculations should be interpreted in a flexible manner.

For example, if computer modeling shows that a PSD increment or secondary standard might marginally be

exceeded, provisional permit approval should be granted. The approval should include an operational monitoring requirement designed to determine, within some specified time, whether the increment or standard is actually being exceeded.

The provisional approval could also include a condition specifying a mutually agreeable remedy designed to correct any subsequently monitored violations. This approach would help to prevent the imposition of unnecessary restrictions at the outset and it would give all parties notice at the outset of what the remedies might ultimately be.

Turning to primary standards. In the case of primary standards, designed to protect human health, the best estimate of the future conditions should be used as a guide in the decision. The agency should consider monitoring information, robust statistical measures, expected exposures and other relevant information in arriving at this best estimate.

We believe that such flexible approaches to the interpretation of modeling results would introduce more realism into the decision-making process and would allow decision-makers to exercise their informed judgment. There is a tendency in air quality decision-making to use exclusively the short and long-term not to be exceeded

basis of the standard in interpreting the air quality modeling results.

This is an unduly narrow approach. The agency has no obligation to use the highest-second-high value calculated by a computer model merely because the standard is written in that fashion. Rather, the agency has a duty to conform the computer results upon which a decision is based, as closely as possible with the real conditions which would actually occur.

In order to do this, the agency should strive to use whatever information is available from the modeling calculations and from whatever other sources, such as monitoring or physical modeling, to estimate the most probable results.

We believe that the agency has an obligation to use probabilistic methods in estimating ambient concentrations, to use more robust statistics, and to employ any other relevant information in interpreting modeling results.

We also believe that the decision-maker should weigh the values being protected in the particular environmental program and the burdens being imposed on the permit applicant and on society by the decision.

It would be irrational not to consider the tremendous costs involved in many air quality management

decisions, given the reversible nature of the decisions and the great uncertainty in most models. Should I say "all"?

Likewise, it would be naive not to consider the value being protected in the decision. As the motivating value decreases in importance and as the burden increases in severity, greater uncertainty in a model's accuracy counsels for a stronger presumption that the permit should be granted. Operational monitoring would then be available to test the correctness of the decision in the areas of concern.

This approach provies the needed balance which emphasizes realism and rational judgment.

This Conference comes at a propitious time. Congress is considering amendments to the Clean Air Act. We have had more than ten years experience with the use of air quality models. And we have seen them elevated to the decision-making role. We are becoming more acutely aware of their predictive limitations and we are at the point where we should redefine their use in the design and implementation of air quality management programs precisely to harmonize their uses with their capabilities.

I would just like to conclude with one observation from this morning's session, which is that the information concerning the accuracy of models presented today

is the kind of information which should be discussed in an agency-sponsored proceeding and it should be the kind of thing that all parties have an opportunity to explore and discuss before any results are accepted as being demonstrated levels of accuracy for existing models.

Thank you.

DR. BURTON: The last speaker is Tom Helms.

STATEMENT OF G. THOMAS HELMS

MR. HELMS: A couple of observations I would make real quickly. As I sit up here, I had had I think a large order of french fries, double hamburger, chocolate milkshake and a piece of apple pie. When the lights went out, I went out awhile ago; so I thought I would do a survey and I counted 18 people out there asleep, plus or minus five people for uncertainty. So, stand up for 30 seconds and stretch.

Okay, let us move on with it. I hated to talk to 23 people or 13 people asleep out there, so maybe I have gained five or six of you.

What I would like to do very quickly is talk about the SIP process. I was in Work Group Two. We dealt with the SIP process, the state implementation plan, revision process. Most of the discussion, at least that I participated in, at the workshop dealt with point source models for sulfur dioxide and particulate matter.

We talked about ozone, but I really will not address any remarks about ozone today.

Some of the comments I will make apply to new source review; PSD permitting, as well.

As I went into this workshop, I had a number of questions in my mind, a number of ideas that I wanted to explore. Some of these are just when do you model; when is it necessary to model? When can you take existing data, air quality data, emission data, and make a decision without modeling? I will not go any further on that today, but I think there probably is a role place and type to do that.

How much confidence do you place in the results of models? I was real curious about that. When do I go to my boss and absolutely put my job on the line, that the decision should be no, based on this model? When should other things be factored into a decision, an air quality management decision besides the results of an air quality modeling effort? When does the size of a plant, the age of a plant, the existing fuel supply, when should this be factored into a decision?

When can you just -- can you decide in a process that you are just really arguing for the sake of arguing and that the real world will not know the difference of the point that you are arguing about?

Finally, when you decide to make a new decision, how can you make this decision, implement new policy, come up with a new program without undoing ten previous decisions and disrupting all the ongoing projects?

So, I went into the conference with these things in mind. I should say "workshop" with these things in mind. There was a lot of discussion. I skimmed this morning over the summary report and pulled out, I guess, the following items that might somewhat address these points.

First of all, I am going to assume that we have decided to model. We have decided the correct way to make a regulatory decision in the SIP process is to model.

The first thing I would recommend, the participants recommended, -- Bruce, I believe you recommended it earlier on -- and that is to get together to set up some kind of protocol. I am presuming there is going to be a potential for disagreement right now in the modeling activity, so get together early on, set up a protocol, figure out which model is most appropriate, set up a process to resolve technical disputes because they will happen, as you all know. I have never seen two or three modelers get in the same room and come out with the same answer. They will usually compromise, but there is no black and white in the program.

Finally, getting together, you have got to be

-- to come up with a protocol, you have got to get a commitment from all parties to accept the results. It does not do any good to get 90 percent down the way and have one party, either industry, environmental, regulatory, or whatever, decide to back out; they do not like the way the results are going.

I think the second point I would like to make is that it is very important to provide the decision-makers with all the information possible, since they put a real world view on the decision. I heard confidence limits mentioned; I heard 95 percentile mentioned; sensitivity analysis may or may not have been mentioned; all these things are important. I think a decision-maker has got to divorce himself from the Sigma Y or Sigma Z and to try to look at what this decision will mean in the real world.

At the conference -- at the "workshop", I should say, there was a lot of discussion about striking a balance in modeling, a balance between standardization, rigid ways to model in every case versus flexibility. It is very difficult to strike that balance. In my present job, I see a lot of lowest common denominatoring. What do I mean by that? You take a model that is run one way in one area; it may be run correctly. Let us presume it is. A case comes along in another direction, another location and I find people wanting to go use that model.

Maybe it is not the correct model. It gives a better answer, either for the environment or against the environment, but people want to use that decision or that precedent in a second decision. So, I think we have got to work very hard to strike a balance between standardization and consistency on one hand and flexibility. Flexibility, meaning to allow you as modelers to select the most appropriate, the best model for a particular situation.

When you do make changes in models, when things are done differently. I think the participants at the workshop felt like there should be some way to grandfather past decisions in and not to disrupt things. That is easy to say and very, very hard to do.

There was some discussion at the workshop supporting a mechanism to convey changes in models, methodology and processes to all interested parties. There was some discussion of a modeling center. That is a good idea. It is going to be awfully hard to pull off in the future with everyone facing resource juts. I would be interested in some feedback on that.

Summing up, I think our challenge as modelers and as administrators to try to get the best available modeling information you can, temperate it with monitoring results, put it into proper perspective with all other factors that impact on the decision and come out with a

responsible, yet environmentally sound decision.

I will quit there, Shep.

MR. TIKVART: Shep and I have more or less agreed that now is the time to implement the 15-minute break that is on the agenda, so we will do that. When you come back at let us say around five-to-four, Shep will, I believe, kind of wrap up the panel discussion and then we will open the floor up for questions and comments and I would like to see a good interface or good discussion between you, the audience, and the panel members here.

Oh, one thing. The Department of Agriculture asks your cooperation in not bringing food or drink into the auditorium, so please bear with us on that. No food or drink in the auditorium.

(Whereupon, at 3:36 p.m. a break was taken. Discussion resumed at 3:54 p.m.)

MR. TIKVART: If you will take your seats, those of you who are still standing, we will continue with -- back to Shep Burton and a summary of the panel discussion and then perhaps some additional discussion among the panel members and then to you. We would like to see an exchange between the audience and the panel members to bring out the ideas that have been initiated here and to have -- to perhaps foster some new ideas.

DR. BURTON: I had not actually heard what each

of the panelists was going to say until you did and I was trying to write down a one-sentence summary and then it got to two sentences, three sentences and then I got about two panelists behind. So, I do not really have sort of an overall summary, but a few questions did come to mind that I thought I would ask the panelists and perhaps some of the panelists have questions that they would like to ask each other since they had not heard what each other was going to say prior to today either.

The one comment or one thought that I had in reading over the draft reports that came in from the various work groups was the extent to which all of the recommendations appeared to be a tremendous overlay of additional administrative burden -- if you will accept that phrase for the moment -- and to what extent, although offered in the -- with the best of intent, I am sure, would these recommendations cause further delays, something that people are already concerned about.

Tom commented -- Tom Helms, in his closing remarks, commented that in one respect the modeling center concept really presented EPA with some considerable resource burdens or additional burdens in time when the resources are being constrained. And there are probably a number of others which would do that.

So, there is one panel member who has had

experience, considerable experience, outside the modeling area and who has dealt with these kinds of process questions and that is Steve Connolly. I would like to ask him the extent to which he sees the recommendations that are raised that have been suggested would cause additional administrative burdens and delays.

Steve?

MR. CONNOLLY: I think if one is talking about systems of developing an advanced protocol for modeling processes, choice of models, use of -- choice of data and input assumptions, use of model outputs or if one is talking about cooperative processes or additional periods, it has to be conceded that there is the possibility for another layer of bureaucracy, another -- more opportunities for delay.

I would say that this is simply a factor of -- there is the same number of people to go through more steps, more stages. It is also a possibility, if you assume that either -- there is a tremendous difference in perception on, let us say, -- amongst the participants in a modeling or -- let us say, a permit proposal and permit review. There is a tremendous number of differences in the perception of the issues or the values or perhaps of the motives of the others or if there is bad faith on one side or the other; that is, that for one reason or another

one party to the issue would like to see the process stopped or delayed. Then this presents an opportunity -- it could be argued that a cooperative process presents another opportunity for delay.

The way I would respond to that is to say that the current adversary process can and is used to delay. That is perhaps one of its strongest features: one can never tell when it is being used for delay or when it is being used just as an adversary process.

The cooperative process, I think, has the advantage early on of first, assuming that all parties are operating in good faith, and assuming that the other side is operating in good faith. It has the opportunity in those situations to cut through the trash quickly and get to the issues that are of importance to people, those that are likely to come up somewhere down the line, get them fleshed out early, set the other issues aside and move on.

The cooperative process also has the advantage of making it more difficult for one side or the other to stonewall it without becoming very clear early on what it is they are doing. So, that if you do use a cooperative process, you are probably going to know what is important to both sides and whether or not they are serious about dealing with these issues; whether they want to see an outcome rather than a stalled process. Earlier than if

you are in an adversary process.

The other thing about a cooperative process, in terms of whether it will actually be another layer of bureaucracy and delay is that the key issues in any, let us say a modeling dispute, or let us back off and not call it a -- let us call it, in any permitting process in which there is the potential for disagreement and controversy over the proposed facility or its site, each of the issues that is going to arise in the modeling process is going to have to be addressed at some point. The question is: who addresses them and how and in what setting and when? Do they address them up front at the beginning; do they address them openly and together and attempt to decide which of the key issues for each side and why or do they address them separately in their own operations and, if they happen to come up at some point in the future, fine. Then we will fight about it.

If it is true that the key issues are going to come up at some point, then it seems to me that it is smarter to bring them up, get them hashed out, if it is possible, before there is a major commitment of resources of energy, and most importantly, of egos. And after there has been a lot of modeling performed and the controversy is in full bloom.

It seems to me that all the cooperative process

does then is acknowledge that which is going to happen inevitably, try to bring it out in the open, at an early date; try to identify areas of agreement and disagreement; try to focus on those areas of disagreement and come to an agreement, and if not, very early on identify for the ultimate decision-maker what he or she has to have; what kind of information he or she has to have and what the ramifications, regulatory or litigatory -- what the ramifications of a decision are.

In other words, it is just a way of saying let us take this whole process -- if modeling is going to continue to be used, if it is going to continue to be an important factor in the decision process, and let us take it out of the closet and let us take it out of the closet at the beginning, rather than bring it out of the closet when everybody has got enormous amounts of energy and time and ego invested in a particular outcome.

I do not think it has to be delay. In fact, I think it can expedite in most cases, the process; not delay it.

DR. BURTON: Would any other panelist care to comment?

One other question that you did not touch on, but that could also entail delays and introduce a lot of inertia in the system is the utilization of advisory

groups or external peer review groups and so on to approve models to recommend changes. Some people perceive that that could also add additional delays.

Do you have any comments on that?

MR. CONNOLLY: My only response is if I were trying for a permit, I sure would not want to wait for a peer review to be --

DR. BURTON: Well, I do not mean peer review; I am talking about now introducing changes.

MR. CONNOLLY: Well, I was thinking of it -- if I were introducing a new model which I thought was a fair representation -- a fair and scientifically credible representation of the real world, I would not -- I would not be overly concerned about a process that was cooperative and open which merely, at an early date, allowed me the opportunity to present that model and explain it and see where people are going to have trouble with it and try to deal with that, but if that were to degenerate into a process where, let us say, you plugged it into the idea of a national modeling center or whatever we are talking about, it would have to go through some extensive peer review there and would -- could not be used in a regulatory process until such time as that whatever peer review committee acted. I would be tremendously concerned about the opportunities for inertia and delay and, therefore, expense.

That is one area where you have to be careful.

MR. TIKVART: I would like to ask two questions: one is how formal should the process of agreeing on models and data bases be? How formal should that process be, the protocol? Do we need a formal written document or does everybody sit around a table and agree? The second question is there are going to be occasions when there is disagreement on the modeling approach to be used. Admittedly, this will result in delays. How do you deal with those situations? The last question deals with the answer you just gave to the former question and that is, you said you would hate to see delays associated with litigating or arbitration of a modeling technique; but admittedly, there are going to be cases like that. Is there any way to minimize the impact on time and resources in resolving those issues in the process mode?

MR. KONTNIK: I would just like to go back to Shep's question for a second. Sorry. I think it relates, I guess, to yours, too, Joe, about the delay point. One perhaps promising, but also troublesome thing about the summary of the workshop results was that it appeared that there were going to be a proliferation of different organizations, you know, of advisory committees, of modeling centers, of dispute resolvers, etcetera, and I just think it is really important that that be a streamline process

involving as few new organizations as possible, maybe one -- preferably one, and that it be adequately funded and staffed so that you do not get tremendous delay and it becomes an impediment rather than an actual benefit.

MR. TIKVART: Can you clarify exactly what you are referring to?

MR. KONTNIK: In the workshop, the summary of workshop recommendations, I think that there were at least -- there were two or maybe three distinct or possibly distinct groups: one for resolving disputes in the development of modeling protocols and another, which would be the modeling center, which would take on a variety of -- may or may not take on a variety of different functions; and the thind being some kind of advisory group empaneled to look at including new methodologies in the guidelines and so on and so forth. My point was just to say that it seems like it is valuable probably to have some kind of panel of experts to deal with those sorts of issues and that that panel should be composed both of agency and non-agency people. It would be unfortunate if that group wound up because of lack of funding, because of lack of staff, whatever, that it became actually an impediment to the decision-making and dispute-resolution than actually facilitating that.

MR. TIKVART: Okay. Then back to Steve. Given

that we have some group to do this, would you comment on your concern with this as an intent, as an impediment to the decision-making process, namely, given arbitration of what modeling techniques or what data base you should use for a given source application?

MR. CONNOLLY: I think my first assumption would be that you ought to set up a system so only under the most dire circumstances is there anything called arbitration. I think you have to set up the system so that it encourages cooperation and negotiation between the parties involved in the process; not bringing in outsiders, except where there can absolutely be no other recourse.

Now, I am sensitive of the fact that if you hold out some arbitration process and somebody wants to be a stonewaller, then that is where you are going to get sooner or later and so I have not thought enough about it to figure out how to make it very hard to get to arbitration or how to give people incentives to not to want to get to arbitration.

I think it is important that those who are the participants in a process come to an agreement on what the assumptions are, what the models of choice are, about what the data to be used are, and the techniques of modeling to be used.

So, I tend to think that you cannot have a

permitting process that is dependent upon -- except in the most extreme circumstances, and perhaps you ought not to have an arbitration permitting process; perhaps it ought to just go to court eventually -- but you ought not to have a permitting process dependent upon some arbitration center somewhere. You would be better off just flipping a coin than getting to that situation.

DR. BURTON: Steve, let me interrupt because I might be guilty of something in the report which I did not intend to do. I do not recall anyone at the workshop or in any work group actually proposing that a body of arbitrators exist who would be called on demand to resolve disputes. What I do recall being discussed was that within the context of each protocol, if issues are raised for which the resolution of disputes are perceived to be difficult in the future, that a scheme for resolving those would be identified in that protocol, which could be one -- one could be a project arbitrator, and that individual could be designated in advance and that could be whomever the group agrees to. It could be a member of one of the groups who is perceived to be particularly trustworthy or whatever. But it does not mean that there is on call a group of people somewhere in the country who would be brought out and it is possible that the demand could exceed the supply, and in so doing, introduce delays.

I just wanted to clarify that. In terms of the other two groups, -- I think there were two groups called for -- this general advisory group and the -- or peer review group, which would oversee the introduction of new modeling concepts -- and another group serving in the role of the modeling center, which sort of would dispurse those approved modeling --

MR. CONNOLLY: My notion of the modeling center was a technical assistance center.

DR. BURTON: Right.

MR. CONNOLLY: To move the new techniques, the new models out into the community.

DR. BURTON: And at most, I think most people that were present looked at existing groups within EPA. Perhaps with some slight modification, perhaps with hardly any modification, just the reorientation of priorities to serve the role of the modeling center or the center of technical excellence, so that, in fact, there would only be on group left and that is the peer review group which would pass judgment on the introduction of new modeling concepts, so I think that meets with Lew's concern about not a proliferation of groups.

As you might suspect, one of the reasons for this hasty ad hoc discussion up here was to try and give people a time to overcome what may be information overload

and for you to dream up questions or to pose questions that you have already dreamed up, so I would like to try and start doing that now, if people do have questions.

Vern?

MR. WALKER: My name is Vern Walker. I am with the law firm of Legal, Hawes and Symington here in town and we are currently representing a group of 14 utilities in the interstate pollution matter that is before the EPA.

If it was one thing we were very good at at Airlie House, it was coming up with long lists of -- especially of sources of uncertainty -- that in many cases were cumulative and attended model results.

We also seemed to be in agreement that a reasonable decision-maker would attempt to take into account as much as possible and appropriately what uncertainty there was in the model results. I would like to pose for the panel the following hypothetical: a decision-maker who has on the record before him or her modeling results, but does not have a quantification, a reasoned quantification of the uncertainty associated with those results. Would you advise the decision-maker to make any use of those results and, if so, why, and in what circumstances and with what safeguards?

DR. BURTON: I get to just sort of dodge these and reflect these over to people and I am sitting here

looking at Tom Helms, who does a particularly good job at those kinds of questions.

MR. HELMS: Let me see if I can -- first of all, I am probably going to have trouble recalling your complete question, so I am ramble.. Stop me if I am not hitting some of the points. Can we make decisions -- I am a decision-maker; I am sitting there; someone shows me modeling results. They do not give me the information, the uncertainty information associated with that; can I make a decision on it? People are making decisions on that information every day. What I think you need to do is -- at least what I would recommend is to put some real world thought into it. Again, go back to -- let us talk about power plants, for instance. How old is a plant? Where is it located? How much life is left in the plant? Is it a mine mouth plant? What options do they have for control? Are they washington their coal now? Are they even burning coal or are they burning something else? Do they have access to gas? Look at all these types of things. How much air quality problem is there associated with it? How big is the plant? Take all that into consideration and see if that helps you with the uncertainty.

I do not think you go out and you say, yeah, the model shows real bad air quality problems; we have got to do something; let us put a scrubber on a 50-year-old plant.

You do not make decisions like that.

What we probably cannot do is make some of the -- look at that one individual plant with the uncertainty associated with it and project way down when 300 or 400 miles and talk about that one plant and its air quality impact. But in the vicinity of the plant, you could probably make a judgment; you could factor in some of the real world constraints and I guess I submit people are making judgments like that now, around the immediate vicinity of plants.

MR. WALKER: Well, without addressing, you know, what actually happens, because I think we are all familiar with that, I was trying to isolate the very important factor that the workshop came back to time and again and that was the use of uncertainty or the fact of uncertainty. There is no doubt that in that case, a decision-maker has to make a decision and that he or she looks to many other factors.

My question was given the hypothetical that I posed, is the decision-maker justified in being more confident in his or her decision by the fact that he or she has these model results? Do the model results, absent quantification of uncertainty associated with them, provide any basis for that decision at all?

MR. HELMS: Modeling results will tell you the

order of magnitude of the problem from a very bad air quality problem to no problem at all type and there are probably ranges of control that could be associated with the modeling results. I do not think -- I think sometimes we get hung up too much on whether an emission limit should be 3.51 pounds per million BTU or 3.0. There probably is not that much difference, frankly.

I think you could look for ranges of control as a result of what your model tells you. You do not have to be so absolute.

MR. WALKER: So you are entitled to rely on the model, even though you cannot quantify the uncertainty so long as you do it on a sliding scale?

MR. HELMS: I do not think I said that. I think -- I was trying to say use a little common sense with what you have got. If you do not have the uncertainty associated with it, you have to do the best you can.

MR. WALKER: Thank you.

DR. BURTON: Is there anyone else -- hold on a minute, Vern. There might be someone else who would want to fell that one. Does anyone? Bruce?

DR. EGAN: Yes. I would just comment that I think the hypothetical may be a little unreal in the sense that you can get some other information. You have got some predictions from, say, a single model and you

have got a very specific series of numbers that come out, but there are other things that you can look at to gain some confidence about that value. Look at another -- at somebody else's assumptions in the model, for example, if there is a dispute about deposition rates or whatever.

I think you can look at the assumptions within the model; try to trace that back in terms of a scientific validity.

There are a number of things that I think you could examine to gain some confidence about what sort of certainty you would have with the predictions.

DR. BURTON: Another thing that -- there must be some information available about whether the model has a tendency for a bias or not. You can -- there are ways in which -- in the workshop various work groups suggested ways of doing this -- looking at sensitivity analyses, for example. So, that you can get a handle on the quantification and then, even if -- pardon me? -- and even if there are -- if you do not choose to do this, there are other ways that you can get a handle on the uncertainty through simulation of the model so that where you might not have an explicit specification of the uncertainty for your hypothetical plant, you could come up with the range on uncertanties and whether or not there is the potential for bias and then look at what the effect of or the implication

of that range of uncertainties would be on a decision. Once again, bringing to bear a fair amount of common sense on the consequences of that uncertainty and how you might respond to it.

MR. WALKER: But you think that that collateral information should be on the record in a particular case?

DR. BURTON: I do.

MR. WALKER: Thank you.

MR. WOOD: My name is John Wood with Central and Southwest Services. Was there any consensus at the Airlie House meeting that a new model need not necessarily give the exact same results as a previously approved EPA model?

DR. BURTON: Would you repeat it again? I do not know if you are --

MR. WOOD: Was there any consensus at the Airlie House meeting that a new model need not necessarily give the exact same results as a previously approved EPA model?

DR. BURTON: Describing the same phenomena?

MR. WOOD: Yes.

DR. BURTON: I do not think that question was addressed, but if no one else -- it was?

DR. EGAN: I think that was implicit. There was a lot of discussion about developing new models. I think people understood that they would produce different numbers, in that sense, yes.

I know we are not supposed to discuss the guidebook, but --

DR. BURTON: Oh, that is up to Joe.

MR. TIKVART: Okay. Let me ask you a question now. Let us say -- because this ties into the process question. Let us say that there is a standard EPA model that is suggested for an analysis and the source has a model that it would prefer to use and they do not give the same answer. Considering the process question of let us sit down and discuss it and establish a protocol, etcetera, etcetera, how do you resolve the problem of having models that give conflicting or estimates or estimates that are not within a same reasonable range of values? How do you deal with that?

MR. WOOD: Of course, I would not go to EPA if I did not feel the range of the values were reasonable, and yours were not. So, I guess we would have to have some arbitrator.

DR. BURTON: The context of your question then is one -- it is not abstract? In other words, it is not the introduction of a new model which when applied to some facility somewhere would produce probably a different result, but rather one source has a model, EPA has a model, and they do give different results, and how do we reconcile the differences?

MR. WOOD: Yes.

DR. BURTON: That particular question was very much addressed and contained in the recommendation for the protocol and to get that up -- you know, we tried to get that resolved at the beginning and without arbitration. I mean those -- there are differences -- the differences are real.

MR. TIKVART: I guess my question is given that that needs to be taken care of as a process, how would you approach EPA or some other regulatory agency, a state agency, with that issue? And try and resolve it in the process mode that Steve Connolly was talking about?

MR. WOOD: I have no response for that, sir.

MR. CONNOLLY: One of the things I think that you can do is come to the regulatory agency and say here is the model we propose to use. As best as we can determine, here are the major differences between the way we treat certain data or certain assumptions in our model and the way you treat them in yours. Here is why we think our way is superior. Agency please respond on the record why you think your way of doing things is preferable or more reasonable or why ours is unreasonable. I think that the more you can get into those processes early and force the process to be explicit and force one side or the other to demonstrate conclusively, if that is possible, why one

approach is reasonable and one is unreasonable. I do not think you have any mechanism other than to come with a model and shake it in somebody's face and say, hey, our model is better than yours. Unless you get down to those very explicit details early on, and unless the source assumes some of the responsibility for forcing those issues, then I do not think that the source is in a position to come back, which I think it should be, come back to the agency and say, okay, agency, here is ours. Why is yours better? You show us why yours is better.

I have not been directly involved in, but have observed processes where neither side has adequately demonstrated why its approach is preferable.

DR. BURTON: That could be done through theoretical or performance evaluations and I think the workshop definitely suggested that when new models were being introduced in the regulatory process that the new model bear a -- the burden of being evaluated and I know that not all models being used at this point have borne similar costs or exercises, but it seems that to introduce new models, that that is something that has to be done.

The next question?

MR. VAN VLICK: I wanted to take issue with a statement that you made that you think that modeling has come a long way in the past ten years. It seems to

me we are at about the same place we were in about '63. We still have Delta T or Sigma Theta and we still look up in a cookbook what the dispersion rate is going to be and there is plenty of evidence around that you cannot even specify the dispersion rate from measured meteorologial variables. We are talking here about we are going to find new models; we are going to come up and develop these new models and what we need is a meteorological variable that will, in fact, type the turbulence and specify the diffusion rate. We do not need a new model; we just need to find a way to tie these together better.

DR. BURTON: I want to clarify something. I certainly did not mean to imply that all of the recommendations were based upon the existence and introduction of new models. As a matter of fact, I intended to say that regardless of whether we had a new model or not, they would still be misused or likely to be misused or misapplied.

I think in terms of whether or not there have been advances or not depends on whether you are looking at the fraction of the glass that is filled with water or the fraction of the glass that is still empty and I do think that there has still been no specification of the turbulence, the parameters that you indicate, and that is a need. We still will make decisions using models and the

purpose of the workshop was to improve the quality of those decisions in light of those kinds of uncertainties.

MR. VAN VLICK: Of the major meteorological variables that are going to be new and different from what we are using; is that right?

DR. BURTON: I think that there are plans to introduce new and improved ones and I am not certain that those -- in fact, I am confident that those will not reduce a lot of the uncertainties that you might be alluding to or that others have alluded to.

MR. VAN VLICK: Another point -- well, this morning they presented that the frequency distribution resembled each other between CRSTER and what was measured in the field, but they failed to specify that did this so-called highest values occur in the same meteorological class? They may get a high value from Type B or Type A or something that is wrongly typed and really ended up with strong, steady winds. I would hardly call that a validation of a model.

DR. BURTON: I know the problem.

MR. TIKVART: Would you give your name and affiliation, please?

MR. VAN VLICK: I am Lowell Van Vlick with Tuscon Electric.

MR. TIKVART: Thank you.

DR. BURTON: Yes, sir?

MR. McGUIRE: I am Kenneth McGuire of Kentucky Air Pollution Control. It is nice to hear all of this reasoning and people willing to sit down together and try to get together over these differences; however, we have a more contentious situation and I was wondering if I could get some reflections from you all on that.

I can remember a few cases -- one of them is with an oil refinery in the eastern part of the state where we have 200 days a year of stagnations and no matter what we could do, the oil company would never recognize modeling. They put monitors at most any place we would suggest, in addition to their own places. They would always get compliance and we never would.

(Laughter.)

MR. McGUIRE: Now, this got settled in Federal Court. Let us put it that way. That is where it always goes.

DR. BURTON: I would like to comment since I was personally involved, I think, with that particular refinery.

(Laughter.)

DR. BURTON: And with that particular circumstance and I believe the way it did come out was that, yes, there was no agreement on modeling and a monitoring program

was adopted and, yes, there were exceedances of every standard that was on the books -- three-hours, 24-hours, and annual averages -- and the refinery was just as surprised as anyone else about those and quite embarrassed about them, as a matter of fact, and did institute a fair amount of control to assure that those standards -- the exceedances of those standards would not be threatened, but I still have to say that the controls that were implemented and agreed upon would, I think -- at least from my own point of view -- and I think the EPA and the state people concurred -- protect against exceedance of the standard.

The model still showed tremendously more additional controls would be required. I think that was just a way in which to deal with the uncertainty in that situation which was to spend a lot of money to go out and get the observations because it was for an existing source.

MR. McGUIRE: Yes, that is right. But when we have an adversarial type situation, it is very difficult for us to get a situation of agreement on EPA models because they seem always to show higher level of emissions than can be obtained by monitors. Our own monitors or others.

DR. BURTON: It does seem in this situation that both sides were right.

MR. McGUIRE: Thank you.

DR. BURTON: Lou?

MR. TOSIE: Hi, Shep. My name is Lou Tosie, not as Tikvart tried to turn me into Lew Kontnik back there.

I was at the Airlie House Conference. I am with the law firm of Fuller Henry and in response to the question -- I think it was the second question ago: what do you do when you have competing models like dualing models, dualing banjos? Our group -- and the only reason I am up here is to pass on what our group talked about a little bit and we had some ideas -- I do not think they were reduced to writing, so I will just report them as a bard; if I had a banjo, I would sing it to you.

The point was that someone in our group felt that if EPA had one model and another group had another model, the best way to look at those competing models was on some type of equal comparison and if EPA had either an empirical or theoretical base methodology to support its model, the agency should be willing to accept an alternative model on an equally sound or better empirical base.

For instance, if the model they had had a set of dispersion curves based on 20 tracer measurements, they should be willing to accept another set of dispersion curves based on a like kind study.

One of the problems I have experienced personally is that you have the agency developing guideline models on one set of information and to come forward with a competing model, you have to go through a continual analysis and reanalysis to prove what the agency did not prove in the first place.

So, in our working group, there was at least that thought. If the agency had a model that was purely theoretical, some physicist drew it up, and you had a better physicist, your model should win. If the agency had a model based on two measurements, empirical testing and you had one based on three, and yours looked better, you should win. That was a very crass idea, but at heart, I think a rather important one of consistency of criteria and, of course, it is always good to have advance agreement because when you get into the courts on this, -- I mean half the people in this room do not understand what we are saying and I do not think we do. In courts themselves, it is much harder.

I thought I would just pass that on.

DR. BURTON: I appreciate it, Lou. One comment. There are examples where that has actually been implemented in practice in the PSD permitting arena and it seems to work in introducing new models, for example.

Yes?

Steve would like to respond to that.

MR. CONNOLLY: Lou, I would add to what you said that to the extent possible, not only should the criteria be consistent, but that they are to be made explicit and in advance and as many of these as possible can be committed to paper as general guidelines for how like situations will be treated and how unlike situations will be treated. Then that facilitates cooperative processes, rather than having to invent the wheel every time you start a new process.

DR. BURTON: And I think that the things that Doug Fox talked about this morning probably provides a basis for reaching some of that agreement.

Yes?

MR. NOCHEMSON: David Nochemson, Los Alamos National Laboratory. What measures would you recommend to use for characterizing undertainty in models and how would you explain these measures to the lay public and, third, how would you validate the methods for estimating these measures?

DR. BURTON: Anyone? Bruce, do you want to -- I have an answer, but I do not want to answer.

DR. EGAN: Well, I missed the morning session, but I assume Doug Fox may have described some of the results of an AMS-sponsored workshop held last -- a year ago

September, which described some measures in terms of at least looking at airobers and so forth on models.

I am not sure if you are looking for a specific list of statistics to be used or what. Maybe you could clarify that.

MR. NOCHEMSON: The statistics that AMS recommended in terms of characterizing uncertainty and things like error estimates and correlation coefficients?

DR. EGAN: Yes. There is a table in this report -- I do not have it with me, so I cannot read it off to you -- but it talks about bias and gross error and other measures which then have some interpretation associated with them so you can relate these to correlation coefficients and so forth. But more generally, your question really does not have a single answer in the sense that depending upon whether you are looking at one hourly or 24-hourly. You may want to quantify the uncertainty in different ways.

The first approach would be to think of using standard statistical means -- conference limits and normal curves and so forth -- to describe the uncertainty. That would be the way people would approach it first off. It gets to be more complicated when you look at, in my view, when you look at real meteorological events which may have some persistence associated with them and things, so you

may want to adopt other statistical measures to describe uncertainty.

DR. BURTON: I think there was one other component to your question and the validation of those measures with -- that report has not been written on how to explain them to the lay public yet. But in terms of validation, I think that that is an issue that the Electric Power Research Institute Study is dealing with, in particular, the influence of auto-correlation in various measures on assumptions about independence contained in those measures and whether or not the influence of auto-correlation on those measures is important and so on. So, I think that that is also a report not yet written and the importance of it, although one can speculate on what the effect would be.

Yes?

MR. HAYNES: Eldewins Haynes, North Carolina Division of Environmental Management. I hope I am asking the right questions of the right people. I may should have asked this before. It occurred to me that the air quality modeling that a modeler does, if he uses statistical techniques such as a percentile ranking or whatever, you are forced to assume that accuracy of your output; otherwise, your statistics that you develop are more or less meaningless.

Now, if the model employed always over predicted -- its values were always shown to over predict, it would be sensible to use the -- a 95th percentile or take its tenth highest value or whatever, whatever you have to do for your regulatory decision, but if the model may just as often underpredict as it may overpredict, there is no -- there may not be an inherent advantage replacing this statistical technique to high, second high other than that perhaps you will get lower numbers, which would help industry pretty good.

If we accept the possibility of model overestimates in instances of model non-attainment, we as scientists should also be willing to accept the possibility of model underestimates in instances of model attainment. Let us say ten or twenty percent below a standard.

What do we do statistically or is there any plan to statistically adjust the decision-making or model impacts if we accept the possibility of underestimates in the modeling and what percentile rate might we apply to the maximum concentration which may -- that we get which may itself be an underestimate?

DR. BURTON: Mike? Bruce?

DR. WILLIAMS: Well, my inclination right now is to say that what you do is you go with your best estimate. In other words, we know something about the

uncertainty, but I do not think it guides the decision in a more complete situation which I would argue might happen to be -- might require other people in the current set of air quality managers. You might look at things like suppose -- you have a decision that it is critical within the range of confidence and you find that the control is not much more expensive than any part of that range. Well, then you might go to the higher end of the range in that case. The next time you came back and found that you were at a critical control level and costs jumped up dramatically, you might stay -- you might accept that one as giving the benefit of doubt to the facility.

I think that is the kind of thing you would have to do if you really were going to incorporate uncertainty into your decision. Right now, I think what you do is you make your best estimate and you live with it.

DR. EGAN: Let me just comment on one thing you mentioned, that by changing to a 95th percentile or something, you necessarily make it more lenient or whatever towards industry. You do not have to do it that way. If you right now took a standard and then changed the frequency recurrence so you allow it to occur more often, you would be doing what you are saying, but you have the alternative of resetting the standard to another number if you wanted to maintain the same stringency, whatever that means

And the notion is that some tighter value could be exceeded more often. The advantage from a modelers point of view is that that, if you allowed a value to occur more often, you might be able to predict it with more certainty and you do not necessarily have to give up environmental goals in so doing that.

Now, there is a difference. Mike Williams pointed out earlier that it may not be a single scale factor that you can apply to do that. It is not necessarily a simple matter to define an equivalent to the highest, second highest value and it might even be site specific for situations of complex terrain or whatever, but the notion would be that you would gain some certainty in being able to predict the values, hopefully.

MR. HAYNES: Yes. I asked this question because I am anticipating that whatever we decide in the future, we will probably have some resistance from environmental groups. They will need to be fully informed about what this really means.

DR. BURTON: In the practical sense, I can tell you what happened when I experienced in a model evaluation activity, we were trying to introduce a new model in a PSD permitting situation and the evaluation of the model was made separate from the application of the model to determine the impact of the proposed facility.

In the course of doing the model evaluation, the high, second high value was, in fact, observed to be lower -- the predicted value was observed to be lower than the observed value and the protocol in this case stipulated that if that happened, all values would be raised in proportion to that value.

I would not -- I cannot defend that from a scientific point of view, but that, in fact, is what was agreed upon. Then -- but that was done in the context of model evaluation; it had nothing to do with the application to the particular facility. It was based upon tracer data.

Then with that sort of fudge factor or that factor for bias, in mind, the plant, the model was actually applied to the facility and then that factor applied to all of the calculated concentrations of that facility and then comparisons with the standards were made. It is a procedure, but it is -- which everyone could agree to in advance, but, as I said, it is not one that you would want to advance for all situations.

Doug?

MR. FOX: My name is Doug Fox. I would like to ask a question of the panel and it is really addressed to some comments that Mike Williams made with regard to the Class I PSD increment and the air quality related values

test and if I am quoting you or paraphrasing you correctly, Mike, -- correct me if I am wrong -- you said to the effect that some of the reliance on the specific numerical value was reduced and diffused because of the air quality related values test that is implied and required in the Clean Air Act for permitting in Class I areas. You were the only one that made any mention of that. I would like to address the panel in general to see if they agree that such an added complexity to the problem reduces the pressure on air quality models.

I am not sure it reduces it. In fact, I think maybe it increases it because it increases certain degrees of uncertainty associated with what you are talking about in the whole realm of air quality related values.

So, the question really is would anybody else on the panel like to address that business of the ACRV test and whether that is a suitable surrogate for using specific numerical values.

DR. BURTON: Does anyone? Bruce?

DR. EGAN: Maybe I could just comment on -- in terms of some of the discussion that our panel had -- in fact, I think it one day when Mike was absent, so that maybe he will hear this for the first time also.

But we identified that fact, that there is a variance procedure with Class I and, in fact, thought about

it in terms of opening the door to adding -- to bringing other information into the decision-making process. In other words, here in the law already was a case where you do not have to depend upon making a decision basically or have a decision usurped, in fact, by a value of the highest, second highest value. There were some ways to go around that. We talked about, in that context, and thinking that perhaps that sort of thinking could be extended in other areas.

Now, most of all, I think, as you recall, our group addressed the PSD issues rather than non-attainment issues and we felt that there was at least some precedent for allowing other supplemental information to be provided to decision-makers in the PSD program which might be less acceptable, if you will, for looking at compliance and non-compliance questions.

MR. CONNOLLY: I would like to add that there is an interesting history to the creation by Congress of the air quality related values test. The time that that was created, I was working for the House Committee on -- now, Energy and Commerce -- on the Public Health and Environment Subcommittee and it is an interesting story of the struggle between values: one between certainty and the -- or between certainty on one hand and flexibility on the other.

The House said let us set up these increments. They may be silly; they may be arbitrary, but at least if -- at least you know what the game is: it is modeling. Up or down. Yea or nay. If you are over, you do not build; if you are under, fine, you can build.

The Senate created the air quality related values test and it was actually the creation of Senator Howard Baker. What they were looking for was flexibility. That they wanted a subjective case-by-case analysis in the Class I areas to determine whether or not there was actually going to be some problem caused to something they called air quality related values in a National park or National wilderness area and that that decision as to whether the permit ought to be granted ought to be based upon that, not upon some arbitrary measure of air quality or modeling of air quality.

They thought we were nuts creating this fantasy world of modeling and we thought they were nuts creating this fantasy world of flexibility where somebody would be making a decision somewhere on what basis God only knew and perhaps he or she could not get involved enough in it to find out.

The deal that was made that we also had the Bro Amendment along on the variance and we slapped the variance on the air quality related values test and that

is the base that walked out of the room. But it was a struggle again between flexibility and uncertainty. The values -- the flexibility uncertainty, the values that we are talking about here.

DR. WILLIAMS: I certainly do not think it reduces the burden on modeling. It gives them a whole series of, in many cases, more difficult questions: ozone and other things.

On the other hand, it gets away from reliance on a single number and probably a number that in many cases is not very directly related to impacts and gets away from that kind of thing. So, I think it is in the line that we are talking about here, improving the decision-making.

I do not think it -- that that kind of improved decision-making is any easier; it is more cumbersome, I would say. Every time you get away from that simple number, it is going to be more cumbersome.

DR. BURTON: Jim, would you care to comment because I think you touched on this when you -- in your presentation?

MR. SALVAGGIO: I agree with what Mike said. Once you get away from the number, you are opening up the ball game for a whole new set of judgments and there is no basis for those judgments. Any decision on that matter

you are going to have questions of equity and consistency between regions of the country, between Class I areas, between state agencies, between companies applying for permits, what have you.

I think it makes the process much more difficult. I think the issue you mentioned initially about attainment and non-attainment areas, that problem is going to come up much more pointedly in the future as we get uncertainty into the decision-making process. There is not this relief valve of other related values that you can bring up to answer questions when you are sitting right on the border of a permit causing a violation or not causing a violation and you are not sure where it comes.

DR. BURTON: There is one more question and I think this will be the last one before we ask Joe how he intends to implement the recommendations in the workshop reports.

MR. PRESTON: My name is James Preston. I am employed by TENNECO, Incorporated. My question is one of the classical solutions in modeling to solving the problems we are discussing is reducing the uncertainty limits to the point they are arbitrarily small. On the basis of the data that is available for us to use in modeling being classically Weather Bureau data, etcetera, has any thought been given to the consideration of the cost benefit to the

end consumer or the American public in general in terms of funding an adequate data acquisition program nationwide to reduce this thing to a better foundation in the modeling industry, which would probably make them -- the whole business of modeling a more viable profession in the long-run?

Then it would -- if everyone recognized it as a tool that gives a reliable result, I would say it would probably be a better recognized instrument for everyone to use. What I am talking about is Congress said that the EPA would do this and EPA officials said that is nice, but they did not give us the money to accomplish it with. Has anybody been looking at the potential cost to do a good job in this area?

DR. BURTON: Tom, you are the only one from EPA next to Joe up here.

MR. HELMS: I certainly do not think any new major program -- I would hate to calculate the cost benefit of that. Now, I do not see us undertaking any major new program to collect that data.

DR. BURTON: I do not even -- I am not aware -- it would be a hard study to do and it looks to me like the EPRI Study offers that possibility because it has both from which to make the comparison.

MR. PRESTON: Okay. Let me clarify my statement

in this regard. I am a member of various organizations that have been entering into contracts with environmental consultants all over the United States to acquire necessary data. We are spending millions -- and I am not talking about one and two millions of dollars -- every day. This data may or may not enter the public domain. In lieu of that, is there any cost benefit nationwide to doing this in a public manner for -- accessible to everyone to save individual organizations or groups of organizations from doing this on a regional or small basis here, there and yonder? There might be some larger benefit out of it.

MR. TIKVART: Jim, I do not think that anybody has done a cost benefit of the sort that you talk about. I am not sure how you would do such a cost benefit, but in response to your question perhaps everybody here is not aware that every EPA and DOE are actively discussing a joint complex terrain study to take place in a time frame of a couple years from now.

I do not know that it is necessary to elaborate on that. Somebody here -- somebody else here -- Norm Bowne -- may want to comment on it, but those discussions are taking place.

DR. BURTON: We will fill the available time, I can assure you.

MR. CONNOLLY: While I was on Capitol Hill

working for the Committee staff, we heard week after week an enormous amount of and numbers of criticisms about modeling, air quality modeling and the use of air quality modeling.

I was trying to recall as you asked your question whether we ever heard about any suggestions on how the basic data that you put into the models could be improved and whether there was a national interest in doing so and how you would go about doing that and how you would fund it.

I do not recall anybody every having -- from industry, from EPA -- and that was in two Administrations -- the late Ford and early Carter -- or the environmentalists ever proposing anything like that, or even proposing that it be looked at to determine whether it would be cost-effective to do it.

I think that is the sort of thing that in terms of research that is a sound suggestion and is one that demonstrates good faith. We want to build a system that works and to build a system with good works, we have to have reliable, credible data that people are willing to live with and we do not have that right now. We do not have that at all.

DR. WILLIAMS: There have been a lot of suggestions of various panels like the NCAQ, air dispersion

panel, about a list of what additional measurements ought to be made and made suggestions to NOAA that things be done, but as far as I know, nothing was done beyond that.

Lots of people are not aware and are not talking about it. Maybe it did not get any audience.

DR. BURTON: Are there any other questions or comments that people would have, especially the panel?

(No response.)

DR. BURTON: Mike, do you?

DR. WILLIAMS: Just one comment I would like to make. I do not want to bring up -- it is obviously not the time and the place and -- I do not think it is the time and the place at least to debate the 1977 Clean Air Act Amendments and if anyone is interested in the basis of the PSD, there is an extensive record in both Houses of Congress that deal with it and ones that wants to deal with Lewis' discussions, I think should refer to that material.

MR. TIKVART: Okay. Thank you.

Shep, I would like to express my appreciation to you for organizing and running this panel. I would like to thank all the panel members for their participation here today and I would like to thank the audience for bearing with us.

The ball is in your court now. Our goal today

was to present you with a lot of information on the status of model accuracy and uncertainty in decision-making and we wanted to present you with some of the concepts that are running around and the necessary information. It is now up to you to feed back to us your ideas on this difficult issue.

I think a lot of us are suffering from information overload right now, but perhaps tonight after a couple of drinks, that overload can be short-circuited and some ideas will spring out. In any case, I am looking forward to be hearing from you tomorrow with your ideas, your thoughts. We will start promptly at 9:00 o'clock a.m. with presentations by the Governmental agencies I mentioned earlier and we will probably not get to the individual presentations until after lunch.

I would like to close the meeting with that and thank you.

(Whereupon,

at 4:57 p.m. the conference was adjourned, to reconvene at 9:00 o'clock a.m. the following morning.)

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CERTIFICATE OF REPORTER

This is to certify that the attached proceedings before the Government of the United States, Environmental Protection Agency, Second Conference on Air Quality Modeling, Afternoon Session, held on Monday, August 10, 1981, in the Thomas Jefferson Auditorium, South Agriculture Building, 14th Street and Independence Avenue, S.W., Washington, D.C., were held as herein appears and that this is the original transcript thereof.

Neal R. Gross

NEAL R. GROSS
Official Reporter

TUESDAY

GOVERNMENT OF THE UNITED STATES

ENVIRONMENTAL PROTECTION AGENCY

SECOND CONFERENCE ON

AIR QUALITY MODELING

TUESDAY, AUGUST 11, 1981

The conference was held in the Thomas Jefferson Auditorium, South Agriculture Building, 14th Street and Independence Avenue, S.W., Washington, D. C., Mr. Joseph Tikvart, Chief, Source Receptor Analysis Branch, Conference Chairman, Presiding.

PRESENT:

Joseph Tikvart	Chief, Source Receptor Analysis Branch EPA
Richard Rhoads	Director, Monitoring & Data Analysis Division, EPA
G. Thomas Helms	Chief, Control Program Operations Branch, EPA
James Dicke	Chief, Techniques Evaluation Section EPA

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P R O C E E D I N G S

(9:00 a.m.)

MR. TIKVART: Good morning, and welcome to the second day of the Conference on Air Quality Modeling, the second conference.

Since some of you came in late yesterday or did not arrive until yesterday afternoon, I plan to excerpt my opening remarks of yesterday to make sure that all of you are aware of the ground rules under which we're operating today.

I am Joseph Tikvart, Chief of the Source Receptor Analysis Branch of the Office of Air Quality Planning and Standards. I will be your chairman for this conference.

Participating with me on the hearing panel to solicit your views and take your comments are Richard Rhoads, starting from my right, Richard Rhoads, Director of the Monitoring and Data Analysis Division; Tom Helms, Chief of the Control Programs Operations Branch; and James Dicke, Chief of the Techniques Evaluation Section.

I'd like to thank all of you for accepting our invitation to participate in this conference. The conference is being held in response to requirements of Section 320 of the Clean Air Act. A conference

on air quality modeling is required at three-year intervals to help standardized and improve modeling practices within air pollution control programs such as prevention of significant deterioration.

The conference is designed to encourage an information exchange. Topics to be addressed are the use of models in regulatory processes and the accuracy and reliability of those models. We are interested in recommendations for improvements both in modeling procedures and in regulatory processes with the goal of insuring optimal use of air quality models in all programs which require their use.

As an aside, I'd like to mention that my impression of our discussions and the various presentations yesterday were there were a lot of shoulds -- we should do this, decision makers should do that, modelers should do the other things, et cetera.

There weren't very many hows, how to do those things, so the individual presenters today, I will be looking for and asking questions about hows, so I'm not badgering any one individual. I'm trying to get as much information out of you as I can, and I hope that the other panelists will assist me in asking questions about hows.

I'd like to note that we have specifically

invited those governmental agencies identified in Section 320 of the Clean Air Act to participate in the conference. Those agencies that responded to our invitation were listed yesterday, and their representatives will speak this morning.

If there are any governmental representatives or members of the public who wish to make a presentation and are not on the list outside the door, or your name wasn't read yesterday, and you wish to speak, please make arrangements with Ann Asbill in back or Charlotte Hopper, and we will get you on the list of speakers.

As required by the Clean Air Act, a verbatim transcript of these proceedings is being maintained. The recorder is Miles Anderson of Neal R. Gross and Company, Incorporated. Speakers are encouraged to provide extra copies of their presentation for the convenience of the recorder and the chairman. Interested persons will be permitted to enter into the record any written comments they do not present orally. The record will remain open for written statements and comments until September 14, 1981.

The transcript and all written statements will be maintained in EPA Docket No. A-80-46. If you would like a copy of the transcript of this conference, please see Mr. Anderson, and he'll make arrangements

for you to obtain a copy of the transcript.

The comments and discussions during this conference will be informal and nonadjudicatory. We will try to allow time for questions after each presentation if those in the audience desire to ask questions, and, as I've indicated, I and other members of the panel will try to obtain clarifications and ask questions to bring out your ideas as we go along.

Individual presentations should generally be limited to ten to 15 minutes with time for questions, so the total presentation by each individual should be about 15 minutes. If somebody needs longer time, we will try to accommodate you.

When making a presentation, please give your written statements to the recorder and summarize your remarks if they are very lengthy, but, as I said, we will try to accommodate you.

Come to the podium for your presentation. Projection equipment, namely a 35 millimeter slide projector and an overhead projector are available. If you have 35 millimeter slides, please give them to Charlotte Hopper in back and she'll see that they get into the projector and are projected for you.

When making a presentation, identify yourself, your organization and your address both verbally and

on any written statement. We plan to have a 15-minute break this morning approximately about 10:30, depending upon how that time falls relative to individual speakers, so we will have a 15-minute break this morning. We're not going to run you like we did yesterday.

If at an appropriate time you have a question or a brief observation, and we will try to allow for such at the end of each presentation, go to the nearest microphone, either one of the two on the floor or here at the podium and clearly state your name and affiliation for the recorder. I have to emphasize that. It really helps the recorder if you state your name and your affiliation before you proceed.

Before I introduce the first speaker, someone left a yellow tablet with notes written in it in the back of the auditorium yesterday. Ann Asbill has it outside. I don't know how important the notes were, -- but if you left a tablet, she has it for you.

Now I'd like to proceed and introduce the first of a series of presentations by various government agencies. The first speaker speaking for the Federal Aviation Administration is Mr. Sundataraman. I didn't say that right, I'll try it again.

MR. SUNDATARAMAN: Thank you, Mr. Chairman. Regarding my name, it is Sundataraman. Nobody pays

attention to that, so I would expect all of you to forget it immediately. The nickname is Ram, spelled R A M. I don't know how that happened. I assumed as I landed on Ellis Island a few tens of years ago, the immigration officer would have assigned that name to be.

I am the Chief of the Air Quality Division in the FAA, and my main purpose here today is to tell you something about what we have done with regard to the uncertainties that we see in the models that are used in calculating the aircraft related air quality problems.

I begin by referring to the fourth and fifth recommendations of the EPA-sponsored workshop on the use of mathematical models as management tools held during May 3-7 of this year.

To quote them partially, "(4) The explicit stipulation of uncertainties through the best available means . . ." and then it goes on -- I don't want to read all of it and take up your time, "(5) The selection and application of new or modified modeling approaches rather than insisting that existing guideline models be used for all circumstances . . ." These are taken verbatim from the workshop summary report Role of Atmospheric Models in Regulatory Decision-Making: EPA Contract No. 68-01-5845.

Even though we did not participate in that workshop, the operating philosophy of the FAA in air quality studies has been (1) to emphasize the need to identify and quantify, where possible, and you all know that is almost impossible, the inherent uncertainties, and (2) thereby or otherwise to continue to refine, improve and verify aircraft-related air quality assessment models.

One of the objections of the air quality studies in FAA is to develop techniques to determine quantitatively the contributions from aircraft engines and other airport-related sources to ambient air quality, so really two different kinds of sources we are looking at, one is aircraft itself, and the other one would be everything that is non-aircraft, but airport related activities.

Such determinations are for purposes of (1) assisting your agency, namely the EPA, Mr. Chairman, and the International Civil Aviation Organization in evaluating the need for and type and timing of aircraft engine emissions standards, and (2) environmental assessment of Federal actions at airports and/or airway facilities as required by the National Environmental Policy Act of 1969.

The meaning of the term federal actions is somewhat vague at the moment. May I be permitted to

insert at this juncture, Mr. Chairman, our expressions of genuine satisfaction at the very excellent, continuing and responsible cooperation and help that we have received from the EPA in the general area of airport related air quality assessments.

With regards to the uncertainties in the air quality models we employ, we have begun by identifying the sources of uncertainty, and today we can list about six of those. These six, I am simply going to read them all to you. They are not unique particularly. They will start fairly general.

The non-steady or the impulse nature of the airplane as a source. The emissions take place maybe for a minute or so while the plane is taxiing and taking off. In terms of taxi, it will be a little bit longer than that, but certainly take-off doesn't take very long, so in a sense it is like an impulse source.

Number two, determination of background levels because you want to find out enhancement of the background levels, how do we determine the background levels.

Number three, the four operational modes for the aircraft. These are somewhat unique to the aircraft because the emissions defer at different modes. For example, the carbon monoxide emissions are the highest, for example, the idle on the taxi, on the queuing

situation, while take-off, it is noxious which is important. The four modes are the engine start, taxi in and out, queuing and take-off.

The fourth source of uncertainty is the high engine exhaust temperature and velocity which affect the vertical and horizontal extent of the plume. I will have a little bit more to say about this later on.

Five, substantial changes in idle emissions rates, for carbon monoxide and hydrocarbond, for relatively small changes in power setting.

Six, other non-aircraft sources associated with airport operations.

We have undertaken oftentime jointly with other federal agencies, including yours, monitoring studies as needed at selected airports across the country to address these uncertainties.

In regard to the non-steady or the impulse nature of the source and the determination of background levels, we have used high time resolution measurements of concentrations and associated meteorological parameters close to the taxiway and/or runway to study the emissions from essentially each aircraft passage. To such monitoring, we have assigned the name single event monitoring.

By co-locating noise monitors, there is one

good use of noise. We can differentiate between airplane and other sources. Such measurements give more objective measurements of both time average values and background levels. I am not going to go into more details than this, because it will take up much too much of the time.

With regard to the different operational modes, we have developed specific models for taxi in and out, queuing and take-off. These models lend themselves more readily to verification. Also, we exercise these models in a diagnostic sense in order to formulate and refine the multi-mode models.

The hydrocarbon emission rate as a function of power setting is being studied in an engine test bed in a cooperative effort with the EPA and the Air Force. This effort will include an assessment of alternative fuels as well.

Further, the emissions will be directly fed into a portable smog chamber for studies on photochemical oxidant formation.

With regard to the plum problem. I feel the FAA has made some contribution in this area. In the other areas, we are making attempts and we have some results, but it's a continuing process.

We did conduct at Dulles International Airport a monitoring program in connection with the decision

made by the Secretary of Transportation on the Concorde landing, and this program was undertaken in 1976, and we did make some observations at that time at the airport, Dulles International Airport, which have clarified the characteristics of aircraft plume rise.

I would like to show you two viewgraphs at this stage, and I apologize for one of them. It is -- you obviously cannot see it. I can't from here.

You may feel why does this idiot show such viewgraphs, but I can assure it is not intentional. The point I want to make here is you can see the taxi path, and there are three spikes sticking out. I don't know that you can see the third, but certainly there is a line joining those. They are the locations of the three towers.

The three towers were not varying in height. They were all 84 feet in height actually, but this was to show simply the location with respect to the center-line of the taxiway, the location of those towers.

We had five stations on the first which is about 200 feet from the center taxi line, and the second tower had also five stations which was another 300 feet away, and then we had a third tower which was an equally tall tower which had only three stations on it, and that was about 200 feet away from the second tower,

so in all it was about 600 feet, 200, 300 and some fraction of whatever is left over.

And what we did was to measure really carbon monoxide. We did measure the temperature gradient from the vertical gradient of compressed air, and we had concurrent wind observations.

Can I have the next viewgraph please? This gives you the result of what we found. This is on one occasion. Obviously the results depend upon the wind speed and direction and the other characteristics.

As you can see, the first panel on the left shows the measurements taken at the first tower. The measurements were at 6 feet, 26 feet, 41 feet, 56 feet and 80 feet, and the middle panel shows the measurements in the second tower, and the measurements on the third tower are shown on the third panel, and you can see the plume rise visually here.

What we have done is we have collected quite a bit of this data, and going through certain analysis, we did come up with values of plume rise that would be applicable to aircraft in a certain general sense, and now we do use this plume rise characteristic routinely in our models.

And the inclusion of this plume rise render a discrepancy between model calculations and observations

much less severe than otherwise.

That concludes my remarks. I thank you for the opportunity to tell you about the FAA program. My associate, Mr. Segal, who is with me or I will be happy to answer any questions you may have. Thank you.

MR. TIKVART: As promised, I would like to ask you a question. That is, you've listed areas of uncertainty in the model estimates, and you've shown what the measurement program showed. How do you plan to take this uncertainty into account in judging what the emission limits from the aircraft should be?

MR. SUNDATARAMAN: The best thing I can do is to say that first of all, you use the measurements in a sense to really check your model, and the two don't agree. I'd be surprised if they agree. And then what you do is to go back and see where you can really make improvements, and one area which we specifically has is plume rise, and the plume rise, we can give you the numbers.

In fact, it appears in our report that was published jointly with the EPA. It was a study, and we do incorporate the plume rise in our models these days routinely, and from the same study, we also had determination of the box size parameters which we also include, and this gives you a technique of perhaps coming

closer to assessing the future impact of airport operations as against what we have today.

MR. TIKVART: Okay, so as I understand it, basically you're talking about going through a model evaluation and improvement exercise until the model conforms acceptably well for making a decision, is that correct?

MR. SUNDATARAMAN: Yes, maybe what you are trying to get at is can we really quantify these uncertainties. Is that the thrust of your question?

MR. TIKVART: Well, okay, that certainly is an aspect, can we quantify the uncertainty, but more importantly perhaps the difficult question is once the uncertainty is quantified, what do you do with that? How do you factor that into your decisionmaking process?

MR. SUNDATARAMAN: I don't think I have answered any of those questions, but let me just throw some thoughts out.

It is not impossible, it's very expensive, but it's not impossible to quantify the uncertainties to the extent that we know the sources of uncertainties. You can do an exercise, like a Monte Carlo model, for example. We have done this in connection with aircraft emissions in the stratosphere. That program is also under me in the FAA, and you can come up with certain

results. You can come up with certain measures of uncertainty, but what you do with those uncertainties especially in a regulatory context is something that is not frankly upto me at the moment.

MR. TIKVART: Thank you. Does anybody else have a question? Any questions from the audience? Okay, thank you very much.

MR. SUNDATARAMAN: Thank you.

MR. TIKVART: The second speaker this morning -- there are two speakers. Is this a joint presentation? No, okay. There will be two speakers for the Federal Highway Administration. The first is Dr. Howard Jongedyk.

DR. JONGEDYK: To clarify this as we go along, the speech about how this all fits together. The Federal Highway Administration has a role play in air pollution inasmuch as highway sources have been identified as a major source of air quality problems.

In our investigations, we have tried and are trying to identify and clarify the problems, to see how we may make measurements, models to more clearly quantify particular situations.

As we identify these problems more thoroughly, we have inputs into other types of modeling efforts, automobile emission control strategies, transportation control measures and highway designs and operations

in general. We are not alone in this regard. The Federal Highway Administration really has a fourth full wave of operations besides the overall cooperation of other federal agencies.

There is actually the work done by the staff. Then we have what's called administrative contracts, and we work with the NCHRP program of the National Academy of Science, and we have cooperative relationships with various agencies who may do the work themselves or work with state universities or other places in the state.

At various times, we've had work in many states, New York, California, Colorado, Texas, Virginia and several others which I won't name right now. Now, in this regard, Dr. Carpenter who is with us this morning has been at the University of Virginia, and he has participated heavily in work which Virginia has done in cooperation with us, and rather than saying anything more right now about that, I'll let him talk about that.

Mr. Moe from Texas Department of Highways and Traffic will discuss some of the work they are doing, so I will try to avoid saying anything about that as well.

Now, our overall role in what we call our federally coordinated program is to look at the role of highways as sources and as places from which emissions

go from, in other words, the dispersion. In our efforts to do this, we've tried to look at what happens to vehicle movements, the unsteady flow, the non-uniform flow of the traffic, and trying to do that, we look at the emissions at sites specifically.

For example, currently we have a major study in cooperation with the Transportation Systems Center in Cambridge, Massachusetts where we are trying to quantify what actually happens as vehicles operate various modes.

Vehicles are put on the dynamometer, a test similar to the ordinary test vehicle from the automobile manufacturers, but also they are tested under a wide variety of modal operations and various applied external loads, meantime collecting the emissions put out by those vehicles under those various loads.

Then these vehicles are taken out in the field, and we find out in the field why we have these loads. Loads come from acceleration, lift, a vehicle going up a grade, aerodynamic drag and road resistance. We are finding out that the road resistance is not a constant factor. It varies with velocity of vehicle. It varies with the extremities of a vehicle going around a curve, it varies with the roughness of the pavement and so forth.

Interestingly enough, as you look at the

variations of source, what we have is a highly variable number for the emissions factor, per se, of a given vehicle, never mind what happens when you have a whole big assemblage of vehicles going down a particular highway or a highway network.

For example, we have found out the emission rates for a highway going up a hill at grade are much, much higher than the stated FTP test cycle value. At the same time, a vehicle operating in a cruise mode at a moderate rate of speed had much lower emissions than highway Federal Test Procedure would give.

Much of our work in the past has been done, however, looking at the quas steady uniform line sources. The first efforts were really done primarily in the New York City area looking at how highway configurations affect the air pollution pattern around a given higway site.

This is followed by a major effort with the California Department of Transportation in Los Angeles. A caline 2 model was evolved from that. That work in California has continued. In most recent years, they have produced a caline 3 model. The caline 2 model was a line source, the caline 3 source is assemblage of area sources near the point of consideration.

The caline 3 which is currently widely

distributed considers such factors of dispersion in the wide direction with the parallel wind and dispension in the Z direction with perpendicular wind, and both factors are considerably greater than we had originally thought.

In the meantime we had a major effort done by SRI international in which they tried to look at some of the basic mechanics with the help of wind tunnel measurements and field measurements of the highway dispersion from again a quasi-uniform steady line source.

We looked at numerous configuration, varying roughness parameters, we considered a highway on a side of a hill, fill, viaduct and so forth.

Many of these evaluations have been incorporated into what we call roadmap models. SRI efforts were contributory to the work which was done by General Motors in Michigan and work which was done in New York and work which was done in Texas, and then all these efforts have formed a data base which is now being looked at against the various models by SRI under an NCHRP program to try to look at the validity of these various models.

One of the areas which we think we're weak in is where we have a great deal of uncertainty or unsteadiness especially near complex sites, complex sites which have large geometric variations due to the buildings,

due to the highway cross sections themselves, interchanges, intersections;where the sources are harder to quantify, dispersion is harder to quantify.

We are looking at in many ways the degrees of uncertainty. We realize our models are not exact. They give us just ballpark numbers frequently; however, until we have something better, we do the best we can.

Now obviously what we have done is interacted with working with what EPA has done with the highway model and other work as well.

A major part of our work, for example, has been done in cooperation with Texas Department of Highways and Public Transportation, and Mr. Moe will talk about that later. If there are any questions, I will entertain them at this time.

MR. TIKVART: You haven't said too much about uncertainty. How do you deal with the problems that you mentioned in the models and how you use those model estimates? How do you deal with the uncertain factors?

DR. JONGEDYK: Well, first off, I would like to defer to Dr. Carpenter who will follow me where he will address some of the probablistic aspects of modeling. As far as the uncertainties are concerned, we have tried to look at the effect of stability on the models and realizing how these factors can come into play, we have

tried to even incorporate hot and cold starts in some models, but that's just a big guess too.

I don't know. I think the answer to your question in large part is largely in the users' hands to have them have enough understanding of what the models look like or what they consist of and treat the models accordingly.

MR. TIKVART: Tom?

MR. HELMS: You mentioned these models give you ballpark numbers. Could you maybe comment on the role the models played in your decision to build or not to build a highway?

We heard yesterday, they were talking about point sources. It seems that models give a go/no-go type approval to construction of say a new power plant. Again, what is the role of these models that give the ballpark numbers in your decision?

DR. JONGEDYK: Well, basically, we have usually taken a very conservative aspect. Most all of the models, when they are applied, consider worst case situations, and with some exceptions where we have sometimes insisted for a higher degree of dispersion than sometimes the models were earlier calling for, we usually go with high volumes of traffic, traffic situations which are conducive to higher rates of emission per unit of distance

or per unit of time, and maybe on usually adverse meteorological conditions, and then we hope everything's fine, and then if it is underneath considering background, underneath the standard, then we have no more problems.

Understand, we have already taken the worst situation. Now we've more finely tuned this, and there have sometimes been situations where I believe the highway agencies had to do some tough negotiations or maybe in a realistic way, we have said, all right, here is what the situation is, and there's nothing we can do about it.

Let me just give you one quick example. Frankly speaking, any tunnel portal with heavy traffic in the mediary of that runnel portal has violated the ambient air quality standard, but somehow or another, we're not called on that one, whether it's a grandfather situation or whatever.

MR. TIKVART: Thank you. Why don't we proceed with Dr. Carpenter? Go ahead, ask your question now.

MR. HALBERSTART: I'm Marcel Halberstart from the Motor Vehicles Manufacturers Association. You mentioned rather serious departures during portions of your testing from the emission patterns shown in the Federal Test Procedure, and I was wondering if you were perhaps working on development of alternative test

procedures. These are serious implications, of course, for fuel economy testing as well as emissions testing.

DR. JONGEDYK: Let me add that fuel economy is being measured in parallel to this, and one of the ways which we have tried to respond to air quality situations or violations in the past has been definitely to show either a new facility being constructed or a new traffic pattern. We will try to have more cruise mode situations occurring because of timing of traffic lights and so forth.

In answer to your question about the legal implications about the federal test procedure, I think the regulatory agencies and the vehicle manufacturers both agree they do not want to change legal status of that current federal test procedure, because they have so much already invested in that particular procedure, to meet the requirements and so forth.

MR. TIKVART: Okay. Why don't we proceed with Dr. Carpenter then?

DR. CARPENTER: Good morning. My name is Bill Carpenter. I am today representing the Federal Highway Administration. We have a need to predict future air quality impacts for either existing or proposed facilities. There's a basic problem. The problem is we cannot predict the certainty.

This uncertainty arises from two places. There are imperfect air quality prediction models, and there's also the random behavior of the inputs to these models which is meteorology, source emissions, background concentrations.

Two obvious methods for dealing with uncertaintly one is averaging. When you take a bunch of numbers and average them together, you reduce errors or uncertainty resulting from unbiased errors. You cannot remove bias, but the averaging process will reduce unbiased uncertainties.

Another approach is simply a probablistic approach which is simply to quantify the erros, try to measure them.

Looking at air quality standards with respect to uncertainty, you could take air quality standards and write a standard in terms of say a long-time average pollution level, such as a yearly average level. The uncertainty present in this long-term average will be significantly less than the uncertainty in any one hourly pollution level.

For instance, if there is no bias and if, assuming for a moment, that the errors were somewhat normally distributed, the error in an annual average would beon the order of one percent of the error in

a one-hour value. Another approach would be to look at average pollution levels and a probability of exceeding some long term average as an annual average.

You can also look at pollution standards as a lot of them are written today which specify maximum allowable pollution levels not to be exceeded more than say once per year, and you can look at the probability of violating that standard.

The two different kind of standards, the long-term average and the maximum level, have different kinds of applicability. Annual average type standards are applicable to chronic exposure health effects. The annual maximum are applicable to acute exposure, and the two do not overlap.

With annual maximum type standards, you really cannot address problems due to chronic exposure, and with annual average type standards, you cannot address problems due to acute exposure.

There are three basic approach to the annual maximum type standards. I'm going to address the maximum standards, because those are the ones where the uncertainty plays the biggest role.

We have the worst case type approaches which Howard Jongedyk addressed a little earlier. In the worst case type approach, subjectively someone says

we will use a given emissions factor, we will use a given background concentration, we will use a given set of meteorological conditions, input these into some model and come up with a pollution level, a single number.

If that single number exceeds a value specified by a standard, you can have a no-go situation. If it does not exceed, you have a go situation. There are the lognormal type models which are also worst case in nature where a lognormal probability function is fitted to a set of data. An order statistic from this lognormal probability function is obtained to estimate an expected annual maximum concentration. This expected annual maximum concentration is treated as a worst case again. If it exceeds the value specified in the standard, again you have a no-go situation.

A last approach is a probabilistic approach where one might try to estimate the probability of violating the standard. On to the next slide please.

The worst case type approach, first off, compounds uncertainty by its subjective choice of the parameters to be input to a model. Also, it's a deterministic approach to what is basically a stochastic phenomenon, a random phenomenon.

The lognormal approach does eliminate subjectivity. It assumes universal lognormality which

is really a pretty hefty assumption that in itself introduces some uncertainty. The lognormal type approach does employ averaging. The parameters of the lognormal distribution are obtained by an averaging type process, so they turn out to be relatively stable, given the assumption of lognormality.

The lognormal type approach does ignore the sequential dependence of hourly pollution levels, and they do depend on each other sequentially.

Given the lognormal assumption and given the assumption of independence, the lognormal approach can be used to estimate a probability of violating a standard. In general, however, the lognormal type models are used to estimate worst case.

The probablistic approach also eliminates subjectivity. It employs averaging and the determination of parameters. It does address sequential dependence. It does not assume any give distribution. There are no uncertainties introduced by assumption of a type of probabilistic model, and the probabilistic method obvious does yield a direct estimate of the probability of violation.

We've done some simulation studies, and what we do here is we input a meteorological history, generally something like a ten-year history. We input deterministic

emissions patterns. These are the predictable patterns from hour to hour, day to day, weekday to weekend, month to month, whatnot, of emissions patterns. These are deterministic.

Then you general random perterbations around these emissions levels. You also input deterministic background patterns. Again, these are hourly fluctuations, and you generate random perterbations around the backbone level.

You employ some sort of an air quality model, such as, for instance, highway. You could, we haven't done this, generate perterbations around these predicted values, in other words, enter in the uncertainty in the predicting model itself.

You have to be able to quantify that uncertainty. That's why we haven't done it. From the result you estimate the probability of violation. We did some work with NO2, and the example that I have on the next slide I will show you the results, but let me describe the example.

We had a highway with six four-meter lanes, a 12-meter median, the receptor was 15 meters from the nearest lane, the roadway extended about two kilometers in either direction away from the receptor. The ADT was 100,000 vehicles per day, speed limit was 55 miles

an hour. The average emission rate was 3.6 grams of NOx per vehicle per mile, and that was at the standard 19.6 miles per hour.

The average annual background concentrations were 2/100ths of a part per million of NO2 and 3/100ths of a part per million of NO3. We did a ten-year simulation and we addressed three possible standards. At the time this was done, there was no standard for NO2, and we looked at three different possibilities, The next slide please.

Looking at these results, we looked at standards not to be exceeded more than once per year of .25, .35 and .45 ppm of NO2. At a .25 standard, all of the methods gave a 100 percent probability of violation. Let me just quickly describe these methods.

The P(V) method is a method that we've been working on for the Federal Highway Administration, and it attempts to address the true probability of violating a standard.

What we call the simply binomial probability of violation is you look at, we had a ten year simulation, you look at each of the ten years, and each year either violated or did not violate a standard, so you're going to end up with one to ten over ten, or 10, 20, 30, 40 possibles of violation. The one above P(V) is actually

a combination of the simple binomial overall possible one year periods.

The lognormal is, as I've discussed before, the lognormal worst case is either a go/no-go depending on whether or not the worst case predicted by the lognormal exceeds the value in the standard, and the worst case is either a go/no-go. It will be zero or 100 percent depending on whether or not it would exceed the value given by the standard.

At .35 part per million standard, again, we have virtually 100 percent for all the methods. At a standard of .45 parts per million, you'll notice what I'll call the true probability of violation was at about 40 percent. The simple binomial does give a very good approximation to this and is quite a bit cheaper to actually implement, and that gives about 30 percent.

The others though, the lognormal probability of violation, the worst case lognormal and the true worst case all still give a true 100 percent probability of violation.

The last three methods don't seem to agree as well with the data as the first two methods do, and I'll go on now to an example using carbon monoxide.

The scenario here is very similar. We had again six four-meter lanes, a 12-meter media, the

receptor was 15 meters from the nearest lane, roadway extend two kilometers in either direction, and the ADT was 100 vehicles per day.

Here the vehicle speed was actually 55 miles an hour, not the speed limit. The vehicle speed and the previous problem varied around the 55 miles. The average emission rate was a little bit over 30 grams of CO per vehicle per mile at 55 miles per hour. The annual average background was 3.2 PPM of carbon monoxide and we did six ten-year simulations.

The first one used only deterministic source and background terms, no random perterbations. The next four used ten percent random perterbations around the initial deterministic source of background concentrations, and the last one was deterministic. It was the same as the first, but it was a five percent overall increase in all source and all background terms.

Next slide please. For reference, the CO standard was 35 parts per million. That's one hour not to be exceeded more than once per year.

We have across the top the six different runs that we did and the same probability terms we had on the previous example. We see that, for instance, look at run number one, the true probability of violation was about 26 percent. The binomial again is a good

approximation to the nearest ten percent which is all it will give.

Using the lognormal probability of violation and the lognormal worst case, both of these yielded a zero percent probability of violation. In other words, the lognormal ended up with a much shorter tail than the true distribution of the data.

The worst case method, however, came out to be again 100 percent probability of violation. The worst case method is really not stable because of its subjective inputs. Two different people can do a worst case and get to drastically different results.

The next four runs two through five again involved random perterbations around the data that was used to run number one. Actually if you'll look at the averages over two through five they approximate what happened in run number one, and the results are basically the same, same source of patterns as you see in run number one.

In run number six, we had the true five percent increase in all background and source, and we notice a significant jump in the true probability of violation. The binomial matches it again to the nearest ten percent.

The lognormal probability of violation in lognormal worst case again, the tail on the lognormal

did not match the true data, and it comes up showing a zero percent probability of violation.

The true worst case, again it's subjective. It does not match the data, consistently doesn't match the data, and it shows an overestimate probability of 100 percent violation.

The last slide please. It's always a pleasant thing to hear, the last slide. Just to briefly summarize the probabilistic method, it does require no subjective input. That's a true benefit for the user. He doesn't have to sit down and scratch his head trying to figure out what sorts of numbers he's going to put in.

It is distribution independent as opposed to say the lognormal, and there are other approaches other than the lognormal. I've used that simply because it's the most common, but this probabilistic method we've been working on does not assume any one probability distribution function.

The probabilistic method does quantify uncertainty. It gives a measure. That's a lot of what we've been looking for. It has the potential for addressing model uncertainty. If you can quantify the uncertainty in a prediction, then that can be entered into the probabilistic method and will, in turn, affect probability

that you would violate a given standard.

The probabilistic method is more realistic. It does match the data better than the other methods considered. As a final point, it is really quite cost effective. There was a total of around $300 in computer time spent to do almost a million simulation, 700,000 or so simulations for the two examples that I showed you.

That's all I have. If there are any questions, I'd be glad to answer them.

MR. TIKVART: Thank you. That was a very interesting presentation. One question, how do you define the probability of a violation? Does that mean two excursions in one year of the series of years that you looked at?

I'm not sure what you mean by that.

DR. CARPENTER: With reference to the standards that we were looking at, like, for instance the CO standard, the standard says not to be exceeded more than once per year, we looked at all possible one year periods, and you can start on any hour of the year.

So you take all possible one-year periods, and you look at the one-year period, and you ask the question, was the standard exceeded more than once.

MR. TIKVART: When you say all possible years,

what does that mean?

DR. CARPENTER: For instance, a year can start on January 1 at midnight, it can start on January 1 at 1 a.m., 2 a.m., 3 a.m. There are 8760 possible starting points in every year for a one-year period.

The standards do not specify calendar year, fiscal year, a year starting on April 30 at 3 in the morning. Standards only specify a one-year period. By implication, therefore, the standards must address all one-year periods. That's what the probabilistic method that we've employed does, as contrasted with what was called the simple binomial which simply looked at ten calendar year periods.

There are many, many more possible one-year periods, and what you do is you average them altogether.

MR. TIKVART: One other question. Do I understand from your last slide that you did not consider model uncertainty in your results, but were only looking at the uncertainty of a violation probability?

DR. CARPENTER: Yes, what we were looking at was the uncertainty in source terms, source emissions, background concentrations and meteorology. We did not specifically consider model uncertainty. The reason we didn't was we weren't able to quantify it. The technique is able to address model uncertainty if someone

can sit down and say what the bounds are or how much variability there is around any one prediction. That could then be entered into the technique and which show up in the final result which says the probability of violation is X percent.

MR. TIKVART: Have you written some reports that we could see and study?

DR. CARPENTER: Yes, I'm not sure when they're going to be out. Do you know?.

DR. JONGEDYK: The draft copies have been fairly well distributed, including your office.

MR. TIKVART: Okay, I'll have to go back and look.

DR. JONGEDYK: But I might introduce one little topic here, and that is the basic probabilistic approach that he is dealing with, the probabilities of the various inputs, could obviously be applied to any type of a highway or any other type of model. It's sort of a conceptual idea involved here.

DR. CARPENTER: That is a good point. The concept is appropriate for any type of source. We've simply applied it to highway sources, because Federal Highways was paying for the research.

MR. TIKVART: Go ahead.

MR. ARTICOLA: I'm Bill Articola of REOTEC.

You appear to be using a Monte Carlo method in this kind of approach that's assuming that the model is basically okay and that you're going to come up with an area distribution for your various inputs, your background concentration or your emission levels.

What kind of thought have you given to the type of area distribution these various inputs would have? For instance, would they be flat, Caussian distributed, and I guess the corollary to that question is why in particular did you choose a 10 percent index for your random perterbations? Why not 15 percent, for instance, or five percent?

DR. CARPENTER: That was not to reflect reality. That was simply to make an example, something that because we would know exactly what we were putting in, and then know what we were getting back out again.

The ways that we address the uncertainties are as follows. The meteorological uncertainty, we assume is taken care of, by an actual ten-year history of meteorology, so we don't perturb the meteorology. We assume that the ten-year history is sufficient to represent its population.

We assume again that the FTP emission factors are appropriate, largely because they are virtually legislated, so we don't touch them. We then take for

every hour of the year, we have an expected traffic volume, expected. From traffic theory, you can go to the Poisson distribution which can be represented for large numbers by a normal distribution, and what we say is that expected value, there's a distribution about that expected value, with a variance equal to the expected value.

So we calculate a true traffic volume. Then we look at the speed traffic volume relationship to calculate a true speed from the initial speed limit of 55 miles-per hour. This then gives us a final emissions factor based on traffic volume and the traffic speed for the different vehicle types.

As far as background is concerned, what we do -- I can't remember what it's called, SAROAD -- thanks, somebody knows what I mean -- has data on various sites around the country giving means and variances of pollution levels, and we simply use these in the model, so the plus or minus ten percent uniform distribution that I used in the examples was simply for the purpose of example only.

MR. ARTICOLA: So essentially you're finding your nearest meteorological station, taking ten years of history, compress it into one year, some up with your distribution from there, and you're using whatever

data you've got for CO monitoring stations where your particular site is a background.

DR. CARPENTER: There is no compressing into one year unless I misunderstood you.

MR. ARTICOLA: I have the impression that you're taking a ten-year history, and you're looking at the probability of say Class F, one meter per second, et cetera, et cetera, over that ten-year history and then come up with a distribution that should be applied for one year?

DR. CARPENTER: No, there is no distribution. It's a straight simulation, and what you do is you end up with in the neighborhood of 100,000 one year -- one-hour observations. It's a string of zeroes and ones and you look at this string of zeroes and ones, and you look at each set of 8760 of them, and you measure your probabilities that way.

MR. ARTICOLA: Thank you.

MR. TIKVART: Please identify yourself.

MR. WITTEN: My name is Alan Witten. I'm from Oak Ridge National Laboratory. Now, to generate your random source terms, you're taking source terms that are randomly but uniformly distributed within plus or minus ten percent of the mean?

DR. CARPENTER: No, this has apparently led

to some confusion. In the example that I presented for CO, that was done simply for the sake of simplicity. In practice, what you do is you have an -- for any hour of the year, the traffic engineers in the Highway Department should be able to give you the expected traffic volume on a link. What you do is you make what's the standard transportation Poisson assumption, and that is that the distribution of the number of vehicles on the road during that one-hour period will have the average given by the numbers that they gave you, and the standard deviation equal to the square root of that average.

The Poisson can be approximately by the normal distribution for large values, hundreds, two hundreds, a thousand vehicles or so, and so what you do is use the normal approximations to the Poisson distribution to come up with a true -- a predicted traffic volume for that length for that time.

Then you take the traffic volume speed relationship to calculate the true speed relative to the posted speed limit, you take that true speed, your true traffic volume and go and find an emission factors.

MR. WITTEN: Okay, so that's not really distribution independent. It's dependent on the distribution you assume back at the beginning?

DR. CARPENTER: That is true.

MR. WITTEN: Now, have you looked at auto correlation in emissions? Is there any assumption there? Are the emission uncorrelated from one instance to the next?

DR. CARPENTER: Oh, no, in fact, they turn out to be highly correlated. That auto correlation is taken into account by the deterministic pattern that the traffic engineer has given you which shows the traffic pattern or the expected traffic pattern on an hour by hour basis. They're highly correlated.

MR. RHOADS: When you evaluate a highway segment, you consider not only its impact on air quality, but also on safety, traffic flow, the effect on the entire network.

Have you attempted or do you intend to apply these concepts for evaluation of some of those other factors, or do these concepts at the moment apply only to air quality impacts?

DR. CARPENTER: I haven't, and I haven't found somebody who would like to sponsor the research.

MR. RHOADS: Understood.

MR. TIKVART: Okay, thank you. Next speaking for the National Oceanic and Atmospheric Administration, we have two speakers, Roland Draxler and Jerome Heffter. Dr. Draxler?

DR. DRAXLER: My name is Roland Draxler. I work for the Air Resources Laboratories which is part of the Environmental Research Laboratories which is part of National Oceanic Atmospheric Administration. It used to be called the Weather Bureau.

Our research is funded by the Department of Energy, at least our group. Our group in the Air Resources Laboratories in Silver Spring, Maryland is conducting research in long-range transport of atmospheric pollutants.

Research efforts are divided between the development of numerical models and the design and execution of atmospheric transport and dispersion experiments to verify the modeling studies. The transport distances over which these models are applied range from the hundreds to thousands of kilometers.

A long-range transport and dispersion model that was developed by ARL and is frequently used by various researchers, calculates the pollutant transport from archived meterological data. The pollutant is assumed to be uniformly mixed in the layer above the ground that may vary from hundreds to thousands of meters.

The pollutant is transported in this layer following the average wind flow. The pollutant grows in horizontal extent with increasing distance from the source.

From some limited experimental data collected during the past years, we have found that this model may overpredict pollutant air concentrations. One particular experiment where an inert gas was released from Idaho and sampled 2000 kilometers downwind in the mid-western United States, the dispersion model calculated concentrations five times greater than what was measured.

These model overpredictions are attributed to much larger horizontal growth of the pollutant due to wind shear and mixing of the pollutant out of the layer near the ground into the upper troposphere. The wind speed and direction change rapidly with height near the ground and the assumption that the pollutant transport can be represented by a single average wind in this layer may not be appropriate.

Model calculations with the pollutant distributed through several layers reduced the excess calculated concentrations to only twice the measured.

Further reductions in air concentrations are expected from pollutant mixing futher aloft into layers not simulated in the model. Both of these processes are probably enhanced over areas of mountainous terrain. These problems are currently under investigation and a revised versions of the long-range dispersion model will account for the effects of enhanced mixing by

permitting multilayer transport and dispersion calculations. Thank you.

MR. TIKVART: Thank you. Why don't we proceed with Mr. Heffter before we take any questions, and then we can ask questions of both of you?

MR. HEFFTER: I'm Nick Heffter from the Air Resources Laboratories, NOAA, and I'll be speaking as the second half of this presentation on tracer experiments for verifying long-range air pollution models.

Concern over air pollution on regional and international scales has led to the development of long-range atmospheric transport and dispersion models such as those just discussed by Dr. Draxler.

Experimental verification of these models is essential to determine the accuracy of assessments based on the model calculations.

To meet this need, several long-range model verification experiments have been completed or are now being planned. A 2½ year experiment was conducted at the Savannah River Plan, South Carolina, starting In march 1975.

The experiment was a joint Air REsources Lab-Savannah River Lab project in which weekly and twice daily concentrations of a noble gas emitted during normal operations were measured at 13 sampling sites (the red

dots on the diagram) from 30 to 140 kilometers surrounding the Savannah River Plant.

A model verification workshop sponsored by the Department of Energy was held in November 1980 in which models developed by several national laboratories and other government and private agencies were tested against these data.

A continuation of this experiment is now being planned which will extend the range of sampling to 600 kilometers, ACURATE (an acronym for Atlantic Coast Unique Atmospheric Tracer Experiment) will take place over a 12-month period starting January of 1982.

Four sampling sites on an arc 600 kilometers northeast of the Savannah River Plant will take twice daily samples. During the experiment period, routine and special meteorological data over the entire ACURATE area will be archived, and about 3000 twice-daily samples will be collected and analyzed.

Now the Air Resources Lab has recently developed a new system for long-range verification studies using perfluorocarbon tracers and utilizing automatic sequential samplers which provide rapid, inexpensive tracer analyses down to parts per ten to the 15th.

The capabilities of the systems were successfully demonstrated in a 600 kilometer experiment during

July 1980 in which two perfluorocarbon tracers were released simultaneously in Oklahoma. Samplers were located along arcs at 100 and 600 kilometers to the north and northeast of the release point at Norman.

Deployment of many samplers over a large area in a long-range experiment can be very costly and present difficult logistics problems. One of the objectives of this experiment was to test the feasibility of using the National Weather Service substation network numbering over 12,000 sites.

Samplers were operated at 38 selected sites on a 600 kilometer arc by cooperative observers who take routine temperature and precipitation measurements. The observers carried out their assigned role with competence and enthusiasm. With the cooperation of the National Weather Service, future long-range tracer experiments will take advantage of the sampling capability inherent in this substation network.

We have also begun planning for a major long-range dispersion study called CAPTEX (Bross-Appalachian Perfluorocarbon Tracer Experiment) involving six separate tracer releases in the Ohio Valley during August and September of 1982.

This location was chosen because it's a major pollutant source area affecting air quality in the

Northeast. It is also suspected that effluents from the Ohio Valley contribute to acid rain affecting many lakes in the U.S. and Canada.

Tracer concentrations will be measured from Ohio to the East Coast at 80 sites located from 300 to 1200 kilometers from the release point. The influence of the Appalachians on transport and dispersion of pollutants is of particular interest.

In conclusion all of the experiments just described should provide tracer data to improve and verify long-range pollution models currently used in environmental studies.

MR. TIKVART: Do either you or Dr. Draxler have any comments on what sort of statement of the accuracy of models is most appropriate. It seems like at this time most researchers have their own preferred method of stating accuracy, and there is not a great deal of consistency in doing that.

Do you have any recommendations on how to go about stating how accurate a model is?

MR. HEFFTER: I don't personally. I think the workshop, the DOE workshop that took place in November will go into the answer to that. The paper is in draft form now, and I really don't think that I'd want to say anything more until that comes out. It does deal

exactly with this problem of how to make statements about accuracy.

MR. TIKVART: Okay, the workshop you're referring to is that that was conducted at Hilton Head in November of 1980?

MR. HEFFTER: Yes, that's right.

MR. TIKVART: Anybody else, any questions of Dr. Draxler or Mr. Heffter? Thank you.

The next speak is John Goll for the U.S. Geological Survey.

MR. GOLL: Good morning. My name is John Goll, and I'm with the U.S. Geological Survey's Conservation Division, National Center Mailstop, 640 Reston, Virginia, 22092.

Before I begin, I just want to mention, we have a small hand-out that we left on the table in the lobby that has references to a few of the sites that I'll be giving, and also names, addresses and phone numbers if any of you are interested in contacting us.

To begin with, the U.S. Geological Survey is within the Department of Interior and is responsible for reglating the mineral activities on the outer continental shelves of the United States. Primarily in the past, this has been oil and gas development, and the Survey has been doing this for several decades, but

fairly recently we received a new responsibility to regulate area missions from OCS facilities. Our rules on this became effective just a year ago, June 1980, and, again, are referenced in the hand-out.

For those of you who are not familiar with the OCS, the outer continental shelf, it's essentially the area of the oceans three miles and beyond the coastlines of the U.S.

How far this goes out depends upon who you talk to. Roughly, you might call it the zone of influence of the United States out to 200 miles.

Briefly the rules we developed for our air quality program are roughly patterned after those of EPA. We were not trying to reinvent the wheel or to go off in a totally different direction, and our sort of mandate from Congress implied that we should contact EPA and again not totally go off in different directions, so roughly we have screening procedures to separate large and small sources and then, of course, controls.

The screening procedures do differ a little bit from those of EPA in that we have a first screen which is an admissions screen based on the facilities emissions, versus its distance from shore. Roughly, the further you go out from shore, the more you are allowed to emit.

Due to the location and size of most facilities, most OCS sources over the first year of regulation have been exempt from controls. Again, we're talking things out beyond 20-30 miles from shore in most cases.

The second screen in the program is through use air quality modeling to compare on-shore impacts to what we term the significant on-shore concentrations.

The significant concentrations, we adopted the significance levels of EPA's emission offset policy. In the intent, Congress implied that we should not control a facility until it caused some significant impact on shore.

Finally, if a facility is not exempt by either the first or the second screen, then it would require controls, again similar to those required by EPA.

The first reference on the sheet, again, fully explains our regulatory program for those of you who are more interested in looking into it a little further. The second screen as we noted required air quality modeling, and as we were developing the rules, and as we were implementing the rules, of course, we were faced with two major problems.

One, there really is no suitable air quality model for over water applications, that is in the regulatory context, and, second of all, where many of

these facilities are data, meterological data can be a problem to collect, especially existing data to find. So to solve this problem, the Department took a three-step approach. First of all, for an interim period only, until better models could be developed, we allowed the use of crster with some minor modifications to the program.

Now the second site on our hand-out explains the modifications and our views on the limitations of using crster for overwater flow. We also encouraged in this notice that any parties that were interested in developing models to contact us and also to submit any overwater models for future consideration, because we realized, of course, that crster was not the answer.

Second of all, to get better data and to find out really what was going on for over-water dispersion and transport, the Department through the Bureau of Land Management has been sponsoring over-water tracer tests, primarily now off the coast of California.

The results of this first series of tests should be available later this year. In addition, independently, the American Petroleum Institute has also been conducting tests in the Gulf of Mexico, so hopefully these and other tests that have been done beforehand will give a little more light on how good or bad we

are with regards to modeling, and also to provide data for any future modeling efforts.

The third point of the Department's program was that the Geological Survey would help to develop a suitable over-water model, again, for OCS facility applications. Now, first of all, we wanted to make sure that we were not duplicating anything that EPA was doing, so we have been in contact with a Dr. Hank Cole of EPA who has been working on coastal problems and also is developing a coastal version of crster.

The second point is that later this year we hope to issue a contract to begin development of an acceptable overwater model. Our first intent, if we are able to do it, depending upon the data from the tracer studies and also the availability of data on the OCS would be to try to stay with something similar to crster.

Now whether we take Dr. Cole's coastline version of crster or have to go to another version, this would be decided. Some of the main points in considering on the final approach would be -- well, through the contractor we would hope to summarize available overwater tracer data and the type of meteorological and oceanographic data available today as input into the model.

Second of all, to determine the meteorological

parameters of prime importance to describe on-shore concentrations. Again, our facilities are three miles and beyond from shore, and we're supposed to measure impacts at the coastline, not over water.

Third, we would be providing guidance to this contract on using the available data to describe the meteorological parameters that we do deem to be important to describe these on-shore concentrations, and, finally, we hope to provide guidance on the meteorological and oceanographic data that should be collected off-shore for use as input into air quality models.

Now as part of the model development through this contract, we do plan to subject a draft of the model to peer review, so one purpose of my being up here today is to ask any of you who are interested in taking part in this review or wishing to receive copies of the draft to please contact us, and we'll try to maintain a list and send copies of the model out when it is available, the draft of the model.

Likely, this will not be until 1982. Again, so that we're not duplicating EPA efforts, we would hope that this model, if it is acceptable and of good quality, will be included in EPA's guidelines and air quality models in the future.

That's my formal presentation, if there are

any questions?

MR. TIKVART: Yes, does USGS have any plans or programs to consider uncertainty in the model estimates?

MR. GOLL: Through the contractor, we would hope, right now for the crster use, we looked at different conditions that you might get comparing what crster would produce versus situations where you may be overpredicting or underpredicting. Again, it was explained in the Federal Register notice that I referenced to, that certain conditions you may be overpredicting than others.

However, we would probably be playing a game that we would wait until we received some of the tracer data and that through the contractors really take a look at where really we are. That would be considered in the contract.

MR. TIKVART: Any questions?

MR. GOODIN: Bill Goodin, Deems and Moore in Los Angeles. A photochemical oxidant, I suspect, may be of importance, considering the hydrocarbon emissions from some of the sources. Will you address that, or strictly the pollutants that EPA is concerned with?

MR. GOLL: For this model at this time, we were just going to be considering non-reactive pollutants.

We hope to again follow EPA's lead as far as reactive modeling.

MR. GOODIN: As far as what model you mean you choose or --

MR. GOLL: Well, there are several approaches that we can take to reactive modeling. One, the Bureau of Land Management in their environmental impact statements do include reactive modeling, and through that, we can get an idea of where problem areas may exist in the future.

We can -- depending on the scenario or the data they've considered in that modeling, we can watch in the future as new facilities go in. If that scenario appears to be coming true, then we could take action.

Right now, for specific facility by facility review, again, the regulatory programs considers VOC's in a certain way, but it does not consider air quality modeling for them at this time.

MR. SKLAREW: Ralph Sklarew, Form and Substance. We're doing the photochemical modeling for the PLM, and one thing really strikes me is that the photochemical modeling is very worst case oriented, very trajectory oriented. By its nature, it has to be a gridded model, and the results and methodology is very inconsistent with the crster methodology. I've been wrestling with

that for a number of years, and I'm wondering if there's any recognition and comments from the USGS point of view?

MR. GOLL: There is a provision in the rules for cumulative impacts, and by cumulative impacts, what is the effect of multiple facilities on a shoreline, especially with regards to ozone.

The words in the regulations are not that specific on what the GS would or would not do. We would consider information, again, for example, from the BLM studies plus information from the affected state and from the affected leasees, the oil facility operators. I guess what I'm saying is that there is not a clearcut yes or no way we would deal with that. It would have to depend on the information that was available at that time.

Again, for example, if we took looking at the BLM scenarios, looking at that, knowing that photochemical modeling only gives indications, it is not an exact science, that it indicates things are getting worse or indicates that things are not so bad. We would compare what went into that modeling with the reality at the time, with how many facilities are actually out there, and versus, also, what the situation is on-shore, but we would get a relative, are our facilities causing

a problem or not causing a problem?

MR. SKLAREW: Speaking of multiple impacts, are you planning to go on to an MPTER type approach for looking at the number of sites at one time?

MR. GOLL: For this modeling?

MR. SKLAREW: Yeah.

MR. GOLL: For the purpose of this contract, we would be looking at a single source; however, we are asking that the final model come out in modules that hopefully could be applied to other models also.

MR. TIKVART: Anybody else?

MR. DESCAMPS: Val DesCamps, Region I, EPA. Could you describe the sources? I had no idea they were that kind of a problem.

MR. GOLL: The sources?

MR. DESCAMPS: Yes.

MR. GOLL: Primarily you're talking about power generators from most facilities. Gas turbines, diesels, in some cases.

MR. DESCAMPS: That are on oil rigs?

MR. GOLL: Yes, they are, yes, perform exploration activities, but primarily from production and development activities.

Depending upon the size of oil or whatever you're looking at, ycu're looking for, you run from

very small sources, very small facilities with a few wells to some very large facilities with upto perhaps 50 wells or so on, with quite a lot of equipment.

If you are not pipelining the production on shore, you might run into tanker emissions which could cause a problem again with the VOC and the ozone problem. Areas of sour gas, that is gas with a high sulphur content, you might run into SO2 problems, but for the most part you're talking about NOx.

MR. DESCAMPS: So you're not concerned with say plumes? Are you considering the plume model or anything of that nature? You don't figure that your emissions will cause any sort of a plume reaching shore?

MR. GOLL: Yes, we would. Essentially I would think the models would be applied mainly to facilities within 20 miles of shore. It depends on again your meteorological conditions. Some areas of the Atlantic perhaps, a plume may never get on shore. Other areas of the OCS, it may go a majority of the time. It really would depend on where you are.

MR. DESCAMPS: USGS is also involved in acid rain and monitoring and so forth, so what sort of an organization is it developing to take care of all these atmospheric problems?

MR. GOLL: Okay. The acid rain is being done

at a different division, in the Water Resources Division, and to my understanding, they're more working on the deposition side as part of the interagency cooperative workgroup.

It's independent of the group that we are with. Our division is sort of a wierd part of the Geological Survey in that we're a regulatory division whereas the rest of the survey is more research oriented.

MR. TIKVART: Thank you. One more question, and then we'll take a break.

MR. VIERATH: Doug Vierath, General Electric Company, Schenectady, New York. A follow-up to the question concerning perhaps a major installation off-shore, the plume impact upon shore. To what extent would terrain impacts be part of your model? Perhaps you might elaborate a little bit on the coastal model as well.

MR. GOLL: Okay. Primarily the terrain should not be too much of a problem for what we have looked at so far. Most releases are roughly 50 meters to 60 to 70 meters, somewhere in that area. You may get plume rise under varying conditions of 50 to maybe 200 meters or so on.

Again, remember, we're three miles off-shore and beyond. Most of the plume will be down to ground

level by the time we reach the shore line. There will not be -- we're also concerned about, you know, as soon as we hit the coastline.

Now most areas of the U.S., well, in the eastern U.S., of course, do not have, you know, high topography. Areas on the west coast and Alaska would.

MR. VIERATH: May I interject perhaps also some Class I areas in the west? Does that get a little sticky?

MR. GOLL: Again, the Class I area would have to be along the coastline.

MR. TIKVART: Okay, thank you. We'll take a break now and begin at 11:45 with Earl Markee from the Nuclear Regulatory Commission.

(Whereupon, a brief recess was called.)

MR. TIKVART: The next speaker will be Earl Markee speaking for the Nuclear Regulatory Commission. Earl, would you come on up, and while Earl is coming up, I would also like to mention that on the list outside we had indicated Don Henderson would be speaking for the Park Service. That is an error. Don will not speak. So the first speaker now will be Earl Markee.

MR. MARKEE: I'm Earl Markee. I'm principal meteorologist with the Nuclear Regulatory Commission in the area of licensing. Today, I will present some

NRC licensing experience which may be of some use to the EPA in meeting their air quality modeling objectives.

Since EPA regulated source characteristics are usually quite different from NRC regulated sources, I would like to focus on one round which I feel we have in common, and this is the acquisition of meteorological data to provide inputs to the models.

The models and the data inputted to the models in my opinion are of equal importance in providing assessments. If a model demands data which are not readily obtainable, the model is of little value in the regulatory process.

In research, the data requirements are stringent so that fundamental processes can be documented and defined. In the regulatory process, the capability to paramaterize these fundamental processes by data is needed to be feasible and cost effective.

Basically, what is needed to drive air quality models are parameters which describe atmospheric transport direction, atmopsheric dilution and stmospheric diffusion rates.

The basic questions which must be answered on data requirements are what kind of measurements should be made, whre should I make these measurements, and how many measurements should I make.

The kind of measurements to be made usually boils down to measurements of wind direction, wind speed and an estimator of atmospheric stability.

Sometimes there is a measure of turbulence or atmospheric fluctuations as in the case of the lateral fluctuations of wind direction; however, the other questions are not answered as easily, namely where and how many measurements should be made.

The where and how many questions are functions of things such as source characteristics, complexity of terrain and the models. For example, if we wish to describe flow around an obstacle like was presented here yesterday in a presentation, is there any way that we can parameterize from single point measurements where the air will flow over a hill or around the hill.

Another example might be coastal terrain. Can we make a reasonable assessment of the penetration of sea breeze or lake breeze effects by a single measurement source, or do we have to have several sources for these measurements.

There is a reasonable concensus that on-site meteorological measurements neart the source are needed as was concluded in the workshop conducted by EPA in January 1980 at Raleigh, and also is described in NRC regulatory guide 1.23.

If meteorological conditions are very radical -- temporally and spacially, we may have a problem. If the site is located in homogeneous terrain where we don't expect these variations, a simple model, and a single set of onsite measurements may suffice.

Therefore, we must consider that -- we must question ourselves as to whether temporal and spacial and homogeneities affect the licensing and the regulatory process.

This can be accomplished either by increasing the data requirements or increasing the modeling requirements. In some instances that we have in the NRC, which are for annual average dispersion models, and so forth, the relative importance of these homogeneities is much less than the consideration in emergency situation where we have an accidental release of effluent from a nuclear facility and are trying to protect the public against the conditions existing at that point in time.

Another factor to be considered is as the modeling and/or data requirements become more complex, the cost goes up. Now cost effectiveness becomes a consideration in the regulatory process. For example, a typical nuclear power reactor with a meteorological program which complies with regulatory guide 1.23 requires a capital outlay somewhere between $300 and $500,000

with annual costs for maintaining the equipment in the range of $50 to $100,000.

However, the total cost for a single nuclear power plant is approximately $1 billion, and there can be severe consequences although remote from a nuclear power plant. The money spent on the meteorological program amounts to much less than a tenth of a percent of the entire cost of the facility, and, therefore, has been considered as diminimus.

However, if we take the other extreme in the nuclear area which is a pharmaceutical firm which handles radioisotopes, the cost of a regulatory guide 1.23 program would represent more than 10 percent of the operating costs of the plant.

However, with a pharmaceutical firm, the consequences are much less, and the requirement for the rigorousness of meteorological definition can be less. Therefore, in the Nuclear Regulatory Commission, we have taken the position that compliance with regulatory guide 1.23 is only expected where it is cost effective and needed to insure safety.

Other procedures can be used to insure safety at the smaller facilities. For example, taking the pharmaceutical firm again, we have made estimates of what might be the worst relative dilution factor that

would occur anywhere outside of that facility and have used a screening technique whereby we have found that well over 90 percent of these facilities can meet those criteria described by the Commission.

It's only in this very small fraction that we would need to do additional studies to make that definition and to make the finding of acceptability.

I'd just like to close with these thoughts. Data requirements should be optimized so that they require only the minimum data needed to perform the regulatory evaluation. Direct measurements rather than parameterization of atmospheric conditions should be used with caution since they are -- the cost of measurement is high in relation to the effectiveness of the measurements.

And, finally, there should be a balance between the modeling and data requirements. Thank you for allowing me to present these comments.

MR. RHOADS: Sir, we share another thing in common. We both represent regulatory agencies. We've been talking about uncertainty in air quality analysis; however, ou regulate for a lot of other criteria, tremors, accidental release of radioactive material, et cetera. Under the other criteria which you used for regulatory purposes, have your people found any better way of handling uncertainty than we have in EPA? That is, your data

for earth tremors, for example, for accidental release of radioactive material must have uncertainty in it also. From that standpoint, do you have any advice to us?

MR. MARKEE: Well, in the NRC, if the consequences are very severe, we usually take the -- what we call the conservative approach. If we were, for instance, to consider, say, that we would have an incident at a facility, we would take the measurements of the entire radioactivity in the containment at that time, and predict the consequences of that, although we know that only a very small fraction will become airborne and get into the ambient atmosphere.

So this is the procedure that we use. I might add that the 95 percentile criterion was mentioned. We use this for analysis of what we call design-basis accidents for a nuclear facility, which are the more severe class of accidents, and using that criterion, this has a reasonable basis in regulatory policy.

If we compare to those objectives that are in our regulations a value based on the 95 percentile level of meteorological conditions, we have conservatism built into the source condition. As I mentioned before, we've considered the entire build-up of inventory and so forth.

However, the thing that we do have in our back pocket is that we know that if we use the 95 percentile criterion, it is deminimis to assume that concentrations in the air would be greater than an order of magnitude greater than the predicted value, and the reason we are using the order of magnitude is because our regulations are not based on -- they are based on obvious injury to the public. They are not based on fatalities.

If you were to increase those by an order of magnitude, you would approach the LD50 levels for one criterion which is the only body dose, so, therefore, we have a safety factor, and we also have the conservatism built into the analysis, and this is how we arrived at that as being the reasonable factor by which to design the plan.

MR. RHOADS: I see. You had mentioned the control of the pharmaceutical industry and that approximately 90 percent of the industry could comply with, I believe you said regulatory guideline 1.23. The remaining 10 percent became questionable, and you evaluate those on a case by case basis as a criteria?

MR. MARKEE: No, we use a maximum value, an arbitrary maximum value based on experimental data. The maximum relative concentration that could exist anywhere outside of the facility. We did an evaluation

to see whether the doses met the applicable guidelines. We have short-term guidelines and we have annual average guidelines.

If they could meet that, then we felt it was acceptable, because this was the worst case consideration. However, those 10 percent which failed the screening test would be re-examined and incrementally, we would have to apply more meteorological information, more site specific meteorological information to that evaluation to see if the maximum value was, indeed, reasonable or whether the maximum value was not reasonable.

MR. RHOADS: So essentially instead of explicitly considering uncertainty in those remaining 10 percent, you're trying to reduce the uncertainty?

MR. MARKEE: That's correct.

MR. TIKVART: Anyone have questions for Mr. Markee? No? Thank you, Earl.

The next speaker will be Dr. Shull for the Department of Energy.

DR. SHULL: Thank you. Good morning. Roger Shull, Office of Environmental Programs. U.S. Department of Energy, Washington, D.C.

First of all, I'd like to compliment Joe here on a well planned and well run program. I know it's required by law, but you people acted like you really

wanted to do it. I think it's a useful meeting. I concur with nearly everything that's gone on so far. I'll give you a little thumbnail sketch of 20,000 people called the Department of Energy and some of the concerns we have with modeling and uncertainty.

DOE has several different functions, one of which is general energy policy development. An example of that is the recent energy policy plan that was released. Regulation, if you've been reading the papers, is a declining function at DOE, because the current administration believes that the market can handled many of the problems that we've been trying to handle in a regulatory mode beforehand.

We also do a large amount of energy research and development at the national labs across the country. We have representatives from those sites here, and you've heard mention of some of those facilities from some of the earlier representatives.

What some people may not realize and what accounts for a lot of the number of employees in DOE is that we are a very large manufacturing organization. All of the fissionable uranium that's burned in civilian nuclear reactors is concentrated in DOE gaseous diffusion plants in Tennessee and Kentucky.

All of the plutonium that's used in military

weapons is all produced at DOE facilities at the Savannah River and Hanford, Washington.

Finally, we are the nation's largest producer of hydropower. We supervise five power marketing administrations around the country and actually generate several billion dollars worth of federal revenue by selling that hydropower.

Why is an agency like that interested in air quality modeling? I guess it becomes fairly obviously. First of all, we have these several large facilities around the country, several dealing with nuclear facilities, but they all have their own power facilities, and other types of activities that release air pollutants, so we are, in fact, subject to EPA and state regulations, and any kind of a tighter regulation translates, of course, into a higher capital and operating budget for our people which is a higher federal budget and on and on.

Secondly, in our environmental policy role, we'd like to be assured that if there are air quality constraints on developing the nation's energy supplies that those are, indeed, valid constraints and not just an artifact of a mathematical model, conservatism.

Why are we interested in uncertainty? The basic reason that even the best mathematical models

that we have now have ranges of uncertainty that translate into millions of dollars of control and compliance costs variations.

These variations can be retrofits on existing facilities, suboptimal siting patterns, requirements for changing fuel sources, decreased reliability of an energy system because of delays im building new facilities, forcing facilities to go to the very highly efficient air cleaning methods, and dispersal of the facilities which has a lot of diseconomies from other points of view.

Some examples, we have been experimenting with the use of long-range transport models in possibly developing national control policies for long-range transport effects, and we coupled some cost estimating algorithms with some of these long-range transport models and looked for optimal control cost solutions at using different values of transport, and we used the matrix method for this.

If we simply use July meteorology as opposed to January meteorology, we can see tremendous shifts for responsibility for the burden of controlling such things as fine particulate sulphates or SO2 precursors, great shifts between regions of the country depending on which particular set of meteorology we use.

So we would argue that before any kind of thought be given to using models for long-range transport control policies that we increase the range of certainty on those.

Other reasons for concern here is that there are many other factors affecting the siting of energy facilities besides just air quality. Any of you who have written EIS's know that the regulations you have to consider just go on and on, dozens and dozens, water quality, waste disposal, water resource laws, land use, wildlife, archeological, those sorts of thiogs.

A simple example, the Intermountain Power Project in Utah, back in the mid-seventies, spent over $2 million checking out groundwater sources in a likely site area because there wasn't enough surface water there, and they did, indeed, find adequate ground water, but that site was subsequently ruled out because of a PSD judgment.

And, I don't believe the company considered it necessary to go back and challenge that judgment, but let me just say, supposing that the shift 30 miles down the mountain to a different site was a result of the model overpredicting on concentration. Just consider the amount of increased cost that was placed on that utility, and, of course, that comes out in your and

my electric bill sooner or later. Another thing we have to consider is that there are many other factors besides clean air that are of interest to states, industries and citizens, and, of course, those are economic growth, jobs, moderate energy costs. We used to think of low energy costs. I think we're convinced that those days are gone forever, but we still would like to hold those down where possible, and, of course, economies of scale in putting togeter energy supply facilities.

An erroneous forecast or overprediction of an air quality concentration which would result in either increased costs or preclusion of establishing a certain facility there could easily deny these other benefits to the citizens for no measurable benefit that would have occurred otherwise, so we want to be aware of these.

I heard talk yesterday that there were high stakes involved in these modeling activities. I hadn't heard to date too many examples of that.

We believe if the air quality modeler properly expresses the uncertainty involved in his forecast that the state or regional policy makers can appropriately consider these other state goals in the political context which is his prerogative.

For example, we may say that our prediction is accurate within a factor of two. That means times

are divided by two, and that can be a fairly large range.

For example, if a state administrator is in a state that really has a bad unemployment problem, he might assume, well, let me guess that the model was really overpredicting, that it really did plus two of what the real value is, so I'll allow the facility to site there.

On the other hand, if he's in a state that advertises its clean air and long vistas and is into tourism, he may assume I'll apply my own safety factor, I'll assume the model underpredicted so we won't let the facility site there.

In that way, the burden and the judgment are on his shoulders as a representative of the people, not on the burden of the air quality modeler who is far removed from responsibility and accountability to the people.

So that's another reason why I'm glad that we're addressing this topic here. That gets us into safety factors, and uncertainty is one of the things that brings us in the engineering profession to use safety factors.

The Clean Air Act actually specifies, and we said the ambient air quality standards that we have an adequate margin of safety. I'm not intimately

familiar with how the PSD increments were set, but I would assume that there were some either explicit or implicit safety margins there also.

The question is how many safety factors do we need, especially if they're all compounding and acting in the same direction, and the next question is, who should set those safety factors. Should it be the state administrator, the Congress or the air quality modeler. I won't answer that question.

Finally, how can uncertainty be reduced in air quality modeling. The simple answer is money, just more money will giveyou lots of improved certainty. But what do we do with the money.

Based upon my experience both in modeling and in dealing with policy matters, I would argue we spend more money on site specific data rather than on extensive improvements in the modeling mathematics, because we alwrys have to demonstrate these things to people who aren't air quality modelers, and I've heard other people argue for simplicity, but we need to have easily recognizable factors that people can help to educate themselves.

So calibration of the model with the tracer studies that our NOAA colleagues have talked about, I think is something we should do a lot more of.

Something else we need to do is utilize along with the air quality modelers who may be mathematicians or computer specialists or whatever actual field trained meteorologists or climatologists. If the air quality modeler says, well, I have a model that pretends that the plume will do this when it hits the mountain, but I'm not sure that it really does that, we need to find somebody who knows what happens to wind patterns in that part of the country, and he can say, well, it does do this, it doesn't do that, and those kind of people working along with the modelers always tend to add credibility to the results.

That concludes the DOE remarks. I m open to questions.

MR. TIKVART: No questions from the panel. Any from the floor?

MR. SKLAREW: Ralph Sklarew, Form and Substance. One of the big arguments against control is the long-range transport, and since you people would perhaps be interested in the cost effectiveness of control, perhaps you've looked at the trade-off of control cost effectiveness versus long-range transport, cumulative impacts.

DR. SHULL: I can't speak in detail on that. That is one strategy that we have begun looking at again

in the past couple of years, but I don't know the details of that.

MR. SKLAREW: Is there anyone in your organization that may be the individual?

DR. SHULL: Yes, if you'll see me after the meeting, I'll put you in contact with somebody that would know.

MR. GOLL: John Goll, Geological Survey. Your statement about leaving it upto the political types, you know, when you have a situation where you might be plus or minus two from what the modeling shows, that leaves me a little uncomfortable, in that I would think that we would loose credibility as modelers and as the person giving that information if we didn't along in these cases where you might be approaching a standard or you might be approaching a situation where it was a tough decision for the political person in charge.

I think in our position one thing we should do is at least give that decisionmaker our best guess of whether in this circumstance the model is overpredicting or under predicting rather than letting him really be faced with the option, well, it could be, you know, a factor of two under or a factor of two over.

Did I misunderstand what you had said?

DR. SHULL: No, I think I agree. My position

was that the modelers should explain the uncertainty and his best guess. I assume the number, if you do present a number as your best guess, that you present that and you present the uncertainty in your professional judgment as best you can, but what I'm arguing is that you don't argue that your best guess is the exact answer and thereby playing a stronger role in a decision than you should be.

MR. GOLL: As long as we let him know what went into the decision, the numbers that we gave him.

DR. SHULL: Right, right, and you may also present him with what the likely effects would be at the upper and lower bounds, but the comprehensive evaluation of what that means in a societal sense is his decision and not yours.

MR. GOLL: I was just worried about situations where you're very close to a cut-off rather than, well, if you can call anything a normal modeling situation.

DR. SHULL: Right. Well, the ones that are way above and way below are never a problem. They're sort of a priori, and the model works, but you hardly even need the model. It's the ones that are close to the line that are always going to be controversial no matter how good the model is.

MR. STRAITIFF: Dan Straitiff, Standard Oil

of Ohio. DOE is a major participant in the shale oil development in Region 8 which is an area of very complex terrain. Given the undertainties of complex terrain modeling, can you offer any suggestions to Region 8 as to how they should overcome these uncertainties in allowing these projects to go through.

Your suggestion of perhaps more money is not maybe an immediate enough solution. Permits are going forward now.

DR. SHULL: I guess I would have to think on that, before I make a pronouncement too much, but I think the tracer studies, of course, that would take time. Perhaps tetroons, the radiosondes, those kinds of things so that we actually learn more about the wind patterns in those areas, or perhaps taught some local hunters to know how the winds blow around there.

I mean get the real facts rather than trying to build a better model of air to flow around those mountains.

MR. STRAITIFF: But other factors, socio-economic, you're going to weigh those as important or not important. I know this isn't your decision. You're kind of on the other side of the fence, but I'm just looking for information.

DR. SHULL: Well, I believe those factors

are being weighed in the decisions, and they're brought up in the public hearings, and if the uncertainty in the models are brought out, and the uncertainties are quite high there, the decisions may fall in the correct place, wherever that is.

MR. TIKVART: Thank you very much. Next we have Mr. Moe for the State of Texas, State Department of Highways.

DR. MOE: Thank you. I'm Rod Moe, State Department of Highways and Public Transportation D-8, Austin, Texas, 78701.

Well, after sitting here for a day and a half and listening to everybody say that wind speed and wind directions, stochastic process, it's random in nature, and that's all that ever occurs, I'm going to try to show that it's not always random.

Would you turn on the slide projector please, someone, and get the lights please? Now, for about seven years, we've been working on model validation studies on line source models at the Texas A&M University in College Station, Texas, and we've established a data base with about 400 hours of data, screened data, and we have monitored at six different sites in four major cities in Texas.

When we examined this data base, we noticed

that any time we had negative wind shear, we had high CO levels. The models predicted well about what our carbon dioxide levels would be, at least reasonably well, ballpark estimates, except when we had negative wind shear. By negative wind shear, I mean a decrease of wind with height somewhere within our tower structure, so we have a tall tower here at 30 meters, and we have four different levels of weather instruments on the way up.

We found that if there was a decrease in wind between any of these sets of instruments that we had fairly high carbon monoxide levels. When we tried to plot these carbon monoxide, wind speed and wind direction, we'll show you what kind of graphs we came up with. May I have the next slide please?

Thos shows our monitoring tracer. Next slide please, and this is the view toward the east, next slide, a view toward the west. It gives you an idea of what kind of obstructions.

This is an at grade site about one mile west of I-45 on north loop 610 in Houston, Texas. Traffic volume about 60,000. Next slide please. This is an overall view of the experimental site. It shows a tall tower, and we have instruments mountedon telephone poles and some of the instruments up the sidestreeet there,

and we also have instruments on both sides of the street for monitoring carbon monoxide at 12 different locations.

Next slide please. We plotted this -- this is an example of several carbon monoxide tracers. This is five-minute average data in the afternoon. Notice, we have almost 8 parts per million of CO under fairly high windspeed conditions. Next slide please.

What we did, we plotted a five-minute average windspeed data, and you notice that only at one of these particular data sets where we have it cross hatched do we have a negative windshear.

Next slide please. This shows the traffic, traffic volume and speed. You notice that at about the same time we had the negative windshear we also had a cessation of traffic in the westbound direction. Next slide please.

This overlays several of these at the same time, and you'll note that the high concentration of carbon monoxide occurs at the same time as a negative windshear and low volume of traffic. Next slide please.

This is a plot of the carbon monoxide on 30 second time increments. It's a plot of several of the carbon monoxide monitors. Notice that the peak CO concentration was 16 parts per million, a modeling meteorologically the same conditions, you get about 1.5 parts

per million using standard models, so we get an order of magnitude increase in peak CO for a short time period as compared with what the model would say for one hour. Next slide please.

Now this is a plot of the windspeed, so what I've done here is shade a negative windshear, so the red areas are areas of negative windshear. You notice the periodic pattern. Also notice that it's 16:15. There seems to be an interruption of the pattern. Next slide please.

This shows a temperature trace for the same time period. You'll notice the yellow areas are inversions. The green vertical lines are strong downdrafts. By strong downdrafts, I mean say more than about .4 meters per second.

Note that the downdraft at a 9 meter height at 16:14:34 is five miles an hour which is a pretty strong vertical wind. Next slide please.

Okay. This matches the windspeed with the temperature trace. You notice the wind inversion plume occurs at 16:14½ or so, well, about 16:15, shortly after the strongest downdraft in this period.

Now this is a short period. We're talking about five or ten minutes here, but notice periodic pattern and also the updrafts and the change in

temperature. Next slide please. This matches the carbon monoxide with the temperature in the wind. You notice that where the negative shear is heaviest is where the carbon monoxide is highest, and that at the time you get the downdraft and the warm temperature plume, you also get a sudden decrease in carbon monoxide. Next slide please.

Now we looked at what happened to the wind direction at the tower, the same time as we got the strong downdraft, and these are the changes we noted. What you've got is a stronger change, greater change in the wind direction towards the bottom of the tower, the smaller change as you go up. But this is a very sudden change and could possibly be caused by a plume that came off the roadway, and this was air that was filling in behind the plume.

Remember, this tower is about 70 feet to the side of the roadway. Next slide please.

Okay. This shows acoustic sounder trace with some strong shear echoes on the lefthand side there. Next slide please. These are from aero environment when they're catalogued, but you notice the shear echoes and their closeness to the ground on the lefthand side of the top trace and the ways on the lower trace there on the lefthand side. These are early morning areas

that we're talking about. Next slide please. This shows the gradual increase in the inversion height, and the ways forming along that inversion with top of fog, shear echoes on the lefthand side.

Next slide please. This shows way forms aloft or representations of that on an acoustics sounder. Next slide.

Now this is an acoustics sounds trace superimpsoed microbarograph traces. Microbarograph trace superimposed on the righthand side of the diagram is the dark line, and then there are a couple of other traces of microbarograph complex that show different time periods.

Notice that evidently at the same time these waves are occurring, there's a big fluctuation in barometric pressure. Next slide please. Now this is an interesting slide. It's the only slide I have of radar traces, of gravity waves and Kelvin Helmholtz instabilities.

You'll notice the top trace up there with two sets of regular waves. They're identical as far as period frequency goes. The middle slide shows the same picture at a lower gain setting. You notice that the lower gain setting you see there, pattern of Kelvin-Helmholtz instabilities that are obscured in the top

photo. The bottom trace shows a K-H instability with breaking waves. The vertical streak could be what they call turnover, you know, in a gravity wave or K-H instability.

Now, these waves can have the cusps up or the cusps down, and this may be a matter of what kind of shear you have at the time with a positive or negative shear. What you can get for that bottom trace is possibly what they call turnover or intermittent turbulence. You get wave formation, very stable conditions, form, and then every few minutes, you get turnover and you get intermittent turbulence, and under these peculiar conditions, this is the sort of thing that seems to occur.

Now, a lot of these is not well understood. The energetics exactly what happens when you have turnover or whether it really is turnover, it's very difficult to measure these phenomena. You can take pictures of them pretty well with radar and things like that, but try to understand some of the relationships is something the meteorologists haven't been able to fathom too well so far, but these may be features that may need to be taken into consideration. Next slide please.

Okay. Here's the second case. Now this is an early morning case. I think this shows the pattern

a little clearer. You notice that you have high concentrations of CO in the early period there, and gradual decrease, very slow decrease in carbon dioxide with time. Next slide please.

Okay. This is the windspeed again showing the shear. The shaded areas are shear. Notice that until 8:00 you've got heavy shear. From about 8 to 8:06, you have what you might call moderate shear starting to break up, and then after 8:06, the shear virtually disappears.

The strongest downdraft is 2.7 miles per hour at 8:00, and the downdrafts seem to be related to the weakness in the shear. Next slide please.

This shows the thermal pattern for the same time period. Notice that temperatures changes at three or four levels at the same time. Notice that the downdrafts seem to be related to the colder temperatures. Next slide please.

This is an overlay of the two, windspeed and temperature. Notice you get a thermal plume at 8:00. Next slide please. Now this is an attempt to integrate the negative shear, to try to quantify it in terms of each minute of negative shear, and that's the solid line. The dashed line is the 3 point moving average of that. Next slide please.

This overlays the carbon monoxide levels on the integrated negative shear, and there does seem to be some relationship. If you average the CO levels, it would pretty close to the integrated shear. Next slide please. Okay. You may say what does five minutes, ten minutes or 20 minutes even make in terms of modeling ifyou're trying to model for aone-hour average or something like that.

Okay. Here's a case where the same sort of thing happened for about an hour and a half, early morning case. It lasted most of the morning. The highest concentration of carbon monoxide if about 26 parts per million which is about 10 times the predicted caline 2 concentration for one hour.

Order of magnitude increase. Notice that the highest concentration of carbon monoxide occurred at three different CO levels at the same instant of time almost, even at the 30 meter height, and notice the relationship. The two are plotted there all the way through. It's a pretty close relationship in terms of peaks. Next slide please.

Okay. This shows a relationship of carbon dioxide at the different locations. You take 4H, that's the top of the 30 meter tower. You compare the solid line with the dashed line in the 4-H, and I think

you'll see at 9:00 or between 8:00 and 9:00, the average is 16 times as high as what the model predicts. That's a one-hour average, and an hour later, it's still perhaps six times as high.

That's a lot of carbon monoxide at 30 meters. Next slide please. This is the plot of the negative shear for four different wind measurements along the tower for about two hours.

You notice the negative shear stays pretty strong until about 9:23, and then at 9:23, you get several sharp downdrafts, then a big increase in windspeed and evidently a completely different regime.

Now that doesn't mean that the negative shear has cmopletely gone away. You've still got some negative shear in that, but you've got a big increase in windspeed and something different happened. Now we think what happened at this time was that we developed longitudinal roles, and that's why you've got the increase in wind-speed there. There's been some work with pulsed doppler radars that shows this, but that's about an hour and a half of solid negative shear. Next slide please.

This shows the temperature structure for the same period of time. Now this was a day in the winter-time in Houston in early December after three days of freezing temperatures, after cold front passage. This

is the third day after cold front passage when the high cell was over the Appalachians, and we were in the southwest corner of the high, you might say, at an east wind blowing parallel to the roadway, and you start out with freezing temperatures and increase quite a bit as you can see here.

Now what we did, we related the carbon monoxide, the wind shear, weakness in the wind shear, the downdrafts and the cold temperature stucture to eight sets of ways all within a 15-minute period.

Now, lights please. I think that study of conditions like this admittedly we don't know how certain they are, how often they happen, because we haven't observed that many cases.

In our data base, you've probably got 200 short-term cases, and this is probably the only long-term case. We monitored this sort of thing about one percent of the time, and we monitored about one percent of the total year, you might say 100th of one percent. We did no monitoring at night. We monitored only in the daytim. We don't know how often this thing happens at night.

There's been a lot of work with radars, sonars, lidars, weather balloons in an effort to try to measure these things, but a great deal more needs to be done.

I think there needs to be a lot more research. I think it's important to consider in terms of modeling as a factor, to my knowledge, it's not been incorporated into the models. I think it's important for monitoring, that when we monitoring, we don't just monitor at ten meters as far as windspeed goes.

If we need to check our windspeed at at least two levels, we need to incorporate vertical windspeed measurements, and I think AMS is considering a Bulk Richardson modulus where you monitor at 1.5 and ten meters.

I think if you're going to do a model validation study, you need to monitor windspeed at higher levels as well, and you should measure temperature probably more often than once a minute, because I think the temperature structure is probably a good bit more details than what our measurements show.

I think we need, like Ned Meyers said, to study the fine scale data. I think it's extremely important. Now we save all our raw data, and I think in studies like this it's important not to average our the data to maintain that, but try to make enough detailed measurements so you can go back and reconstruct these things for your data base.

Any questions?

MR. TIKVART: Questions from the audience?

MR. HAYNES: Eldewins Haynes, North Carolina Division of Environmental Management. Was there any relation, did you check these stability classes during these periods?

DR. MOE: The stability classes were C and D.

MR. HAYNES: C and D?

DR. MOE: C and D, so we're indicating neutral or unstable, slightly unstable classes where the actual stability was about F, you know, in terms of modeling.

MR. HAYNES: It was F.

DR. MOE: The Pasquill Gifford class about C or D.

MR. HAYNES: Okay, and this was also during the periods of the negative wind shear?

DR. MOE: Right, so the Pasquiill Gifford stability class doesn't work for this situation. In fact, a similarity theory didn't work, the wave structure and all that.

MR. HAYNES: Sort of going through all the detailed measurements that you had to go through, do you have any suggestion of how we could make this operational?

DR. MOE: I don't know for sure on the negative

windshear. You know, that's pretty easy to check for. What we did, we took a raw data, and we averaged five minutes, 15 minute and 60 minute averages. If you look at five minute averages, and you see you have negative windshear by having a plot that shows what the windspeed is for the different heights, then you can narrow this thing down.

I look, of course, mainly for areas of high carbon monoxide, the same time as you're looking for that negative windshear. That narrows it down, and then once you narrow it down, you can use a fairly versatile plotter, like say versatex or something like that and try to plot your raw data, instead of doing it by hand like I did. That one graph took several thousand points, and it's tedious, you know, but you can do it with computers too.

The trouble with computers is you crunch so many numbers, you crunch all the good information out of it sometimes.

MR. TIKVART: One more question, Fred, and then we'll have to move on.

MR. HAMBURG: My name is Fred Hamburg. I'm with Radiation Management Corporation. I just wanted to get straight in my mind the purpose behind this presentation.

It is obviously in the nature of a professional paper, and as member of the TT3 committee of APCA, I would welcome a paper of this sort at our forthcoming meeting whether it be in New Orleans next year or the one precedigint that at San Antonio.

Nevertheless, the question that came up in my mind is why are we seeing this, and I thought about it, and perhaps I have the answer, and I'd like you to comment on this.

DR. MOE: Okay. I wanted to show if you think you know all about what's going on out in the atmosphere, you're wrong. I also wanted to show that you can't just summarize your data by applying statistics to a mass of numbers and come up with all the answers. I think these are critical points. I think it's important to go back and look at the detailed data.

MR. TIKVART: Thank you very much. Next we have Mr. Misra representing the Ontario Ministry of Environment.

MR. MISRA: Thank you, Mr. Chairman. My name is P. K. Misra, and I'm representing the Ontario Ministry of Environment in Canada. My talk will deal with what air quality models predicts and what we measure, what the observations are, and why there are uncertainties in models.

I'll also talk briefly about how to quantify some of these uncertainties that exist in models. Dr. Venkatram -- a quarter of this paper that I am presenting. May I have the first viewgraph please?

Some of the things that I want to describe right now are probably familiar to you, but nonetheless I want to work through it just for completeness. What I have here is to show you the difference between performing an experiment on laminar flow and a program flow.

If you have two flow experiments under identical conditions, and the laminar flow or variable flow will be the same for both experiments. You do not expect them to -- they will be different. Next slide.

Therefore, in doing an experiment in program flow, we do not really work with just a single measurement, but rather an ensemble of measurements, An ensemble is defined by values of a valuable, say, for instance, concentrations of any gas or pollutants, of all possible experiments under given conditions.

Now, the ensemble is defined by probability mean of this ensemble, and I've given you a formala there like if N stands for a number of experiments, when N is very large, you expect a steady (Unintelligible0.

Now in the atmosphere, and keep in mind that

the experiment must be repeated under given external conditions, and this is not possible in the atmosphere, and it's also (unintelligible) to use time averaging; however, time averages approach ensemble average only for a restricted state of conditions.

Now if you do assume that these conditions are (unintelligible) we can perhaps replace ensemble average by time averaging, and I have given you a formal for the time average where we now have defined the average (unintelligible) to infinity. May I have the next slide please?

Now if you can define a time scale, for instance (unintelligible) then if the sampling is sufficiently larger than the time scale we can in principal (unintelligible). However, in the atmosphere, the way we perform the experiments, I do not believe that we can get this time scale, because the time scale by definitions is defined by that, the spectrum at zero frequency and when you do an averaging, you take a finite averaging here and then you don't (unintelligible) zero frequency scales.

And so the time scale is not finite to atmosphere and, therefore, in principle, we do not get an ensemble average in atmospheric experiments regardless of the averaging period or the sampling period. May I have

the next slide please? Now, the best we can do in any mortal is to predict the ensemble average whereas an observation is just a member of this ensemble chosen at random. Therefore, the model prediction is not expected to exactly equal (unintelligible), and this is a sort of (unintelligible) atmosphere.

Therefore, uncertainties in predictions are inevitable. I do not believe regardless of how much you try that we can get through this uncertainty. May I have the next slide please?

The next question is given this, how do we quantify the uncertainty models? The one way of doing it is to compare the model results with the experimental data. Now, keep in mind that when you do an experiment, your ensemble is defined by the input parameters. In other words, if you have the first measurements, like we get one data point, that belongs to one ensemble, the second measurements belongs to a different ensemble. It's a very important point, and, therefore, there's no actually (unintelligible) ensembles that are parallel.

Now if the model predicts the ensemble mean, then the data points that is of (unintelligible) atmosphere are distributed about the model predictions in some distributions which (unintelligible) of time. Then if we assume how these data points are distributed

about model predictions, we can perhaps predict the probability with which an observation should exceed a given value, say CM which may be a standard. It can be, like I said, (unintelligible) if we know what the distribution is. Can I have the next slide please?

I'll show you as an example for normal distributions if you want to predict the set of probability, (unintelligible), you can in principle, and you know what the (unintelligible) are. Like I said before, it's very difficult to obtain these parameters from atmospheric data, because (unintelligible) are different for each data point.

But you can't make an assumption like perhaps these symbols are close to each other; therefore, you can (unintelligible). If you assume that the concentrations are lognormally distributed, it can express your observed concentrations as the predicted concentrations (unintelligible) and ideally these should be an average of (unintelligible).

Therefore, the average of log absolute square is the measure of uncertainty in models. Incidentally this is the geometric standard deviation. Could I have the next slide please?

With that introduction, I shall show you how we obtained the uncertainties of the modeling study

carried out in Ontario Environment. Now this is a model for the prediction of ground level concentrations of (unintelligible) particles inside a (unintelligible). The model is based on the idea that most of the dispersion is controlled by the large scale eddies, the updrafts and downdrafts in the (unintelligible), and where the entire plume is divided into an updraft plume or a downdraft plume, and the (unintelligible) to give the final results of the ground level.

The observations, the model predictions are compared with observations of Willis and Deardorff in their Watertown experiments. Now when you do those statistics, assuming a lognormal distribution on the concentrations, you get a geometric of the predicted (unintelligible) is 1.12 (unintelligible)which is not too bad, and the geometric standard deviations is 1.25.

So this givens an idea of what uncertainties are. May I have the next slide please? This is a model developed by Venkatram for dispersion of pollutants for particles in the (unintelligible) atmosphere. It gives you the ratio of the predicted observations, and, again, if you do the statistics, you'v0 got a geometric mean of 1.14, where value is 1, and a geometric (unintelligible) of 1.69 (unintelligible). This is a large number of data points. Next slide?

And this is a model for long-range transfer to air pollutants, a statistical model where statistical, long-term, long-range plume is assumed for each source and applied to all the sources or most of the sources (unintelligible) in the United States and Canda, and the model also assumes the scavenging in a statistical manner by assuming that (unintelligible). You can see the rightmost column gives the ratio of observed predicted concentrations, I'm sorry, the (unintelligible) of sulphur in rain, and (unintelligible), the geometry mean and (unintelligible) deviations are 1.16 and 1.64. Again, the value of the (unintelligible) reasonably well.

Okay. Given the fact that (unintelligible) are close together, and we can get a handle on the model uncertainties, how to use this in the decisionmaking. Generally, (unintelligible) controls are based on impact on the environment of sources. Its effects by (unintelligible) averaging over several years, then our decision on the control of the source can be based on model prediction for long-term averages, and I say that with reservations, because (unintelligible) but probably now the uncertainties are smaller, so it enables you to make a decision on the long-term average.

However, (unintelligible) short-term concentrations, the model predictions can not only be

used in the absolute sense, uncertainties would have to be associated with model predictions to affect our inability to predict the short-term concentrations exactly. Even with uncertainty and with suitable assumptions on the distribution of those (unintelligible) about the predicted mean, we can only determine the probability of which a given concentration of (unintelligible) measurements.

Decisions would then have to be made on the basis of this probability along with economic constraints if the -- may have I have the last slide please? The last view graph shows if you are not comfortable with a normal distribution or lognormal distribution or any distributions, you can use these expressions which is colored (unintelligible) regardless of the distribution which says that absolutely (unintellgibile) the probability that it will be greater than K is less than sigma square of the variance, so given sigma square, we can get an (unintelligible) of this probability.

Thank you.

MR. TIKVART: Are there any questions from the floor for Mr. Misra? Okay. It doesn't appear that there are any questions; however, can you supply the recorder with a copy -- you have. Mr. Moe, have you also supplied a copy of slides? Can you send those

to the recorder please? It's right after 12:00 so I think it's appropriate to break for lunch now, so let me give you some additional information before we do break.

There are four more governmental agencies who wish to speak, and they'll be first up this afternoon. Those are Mssrs. Trout, Bonta, Raol and Meguire. After then, we'll move to the individual presentations and the following individuals are those I have listed, Sklerew, Witten -- excuse me, and they'll speak in this order, Sklerew, Witten Hanson, Kohm, Moon, Pell, Wright, Fein and Wurmbrand. I hope I've pronounced those names correctly.

It will be tight, but we probably will be able to get through all those this afternoon. Those are all we have as of now with one exception. I've been informed that the National Academy of Sciences wishes to make a statement tomorrow morning, so whatever happens this afternoon, we will be here tomorrow morning to continue to take comments as appropriate.

We will reconvene this afternoon at 1:30.

(Whereupon, the hearing adjourned for luncheon to reconvene at 1:30 p.m., this same day.)

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

(1:28 p.m.)

MR. TIKVART: After further reconsideration of the list of speakers for this afternoon and negotiations with them, et cetera, I'd like to quickly just revise the list of speakers and their order of presentation.

One of the government agency speakers, Mr. Raol, somebody scratched his name off the list in back, I assume he no longer wants to speak. If that's incorrect, please let me know.

That leaves three agency speakers, Trout, Bonta and Mequire. The individual presenters following them will appear in the following order, Sklarew, Pell, Hanson, Fein, Witten, Moon, Wright, Maxwell, Kohm and Wurmbrand. The last couple of names are in jeopardy as far as completing today's schedule. If you want to speak today, we will accommodate you, the last several names, if you could appear tomorrow morning, that might be helpful all the way around. We'll see where we are after the break at about 3:30.

The first speaker then will be Dennis Trout, the third last of the agency speakers.

MR. TROUT: Thank you. My name is Dennis Trout. I'm the regional meteorologist with USEPA in

V. The address is 230 South Dearborn, Chicago, Illinois, 60604.

First, I'd thank all the participants here, and I'm particularly glad to see some of the state and local agencies represented. I think we'd be well advised to hear their comments and suggestions as well.

The last day and a half, we've heard numerous talks on motherhood and apple pie, and I took a look at what I had prepared and found it to be very similar. I had originally intended to present a summary of comparison criterias suggested for evaluating the adequacy of air quality models for regulatory purposes.

I propose particularly after luncheon, my attention span, if your attention span is anything like mine, maybe some of you won't be listening after three minutes, so I'll try to hold my comments to as brief as possible.

We've heard very much on the should's as to where you should go, but haven't heard too much on how we can get there. In order to improve model estimates and account for and, where possible, reduce modeling uncertainty, we have been constantly and consistently advised in meetings such as these by statements such as, one, models should be developed and revised following scientific methodology and adequate representative air

quality, meteorological and source monitoring data should be developed and relied upon.

Two, models and analyses should make use of proper input data. Three, model analyses should be judged by their conformance to accepted scientific principlaes.

Four, potential investigators meaning industry and their consultants, for purposes here, should meet with the agency personnel to develop a protocol to be followed, and, five, frequent communications should be maintained between the investigators and the agency.

I would like to note that I wholeheartedly agree with the objectives of these statements. However, what is routinely preached and generally accepted by all of us attending these meetings today, this does not appear to be what is routinely practiced.

Many of the reasons given for departures from these proposed objectives are well understood. For example, costs and time constraints which are probably two of the most important concerns. Other reasons that may bear on the situation include the objectives of those funding the study.

For example, if the study is to develop an alternative model that would in hope allow for greater emissions by estimating lesser impacts than a reference

model, those funding the investigation may not be desirous to begin funding or continue to fund a study that might actually result in the development of a model that would estimate greater impacts than the reference model.

Considering the facts that, one, most of the specific recommendations of how to account for uncertainty do not diminish resource requirements, either on the part of industry or the agency, but actually require greater amounts of time and money to accomplish these specifics, and, two, that the historically listed constraints, time, money and objectives may be expected to continue or remain constraints that we find ourselves faced with in the future.

In view of these, I would like to solicit the specific recommendations from industry, consultant community and the state and local agencies for what you might view as minimum proposed requirements for the agency to follow regarding the five objectives previously stated.

The specifics on the how's or the means of accomplishing the five should's previously indiciated, if these are not resolved or provided, we should not expect that the problems associated with uncertainty will be readily resolved or that we will likely account for the historically known resource constraints, so at

this point, I'll either let you comment to me on what I have said or I would suggest as this is opened up for public comment that you feel free to elaborate on those concerns, because I think they are of particular relevance and importance to us, in exepditing this process. Thank you.

MR. TIKVART: Any takers on Dennis' question? I think it was a good question. Why don't you go ahead and state the five should's and then your question about it. Give them a chance to think about it.

MR. TROUT: Okay. One, models should be developed and revised following the scientific methodology and adequate representative, air quality, meteorological and source monitoring data should be developed and relief upon.

Two,models and analyses should make use of proper input data. Three, model analyses should be judged by their conformance to accepted scientific principles. Four, potential investigators, meaning industry and consultants particularly for this meeting, should meet with agency personnel to develop a protocol to be followed in the course of their investigations. And, five, frequent communications should be maintain betwen the investigators and the agency, and I'll just note that we standarly hear these. In fact, comments have

been made before the Senate Committee recently towards these ends, but we find that they are very frequently due to alleged time and resource constraints unable to be complied with.

If they can't be complied with, when you come in to talk to the region, and that's my particular problem, then we're making excuses from the start. We have to build in the caveats, let's build them in now, and not wait for later which is going to cause you and all of us greater delays and dissatisfaction.

MR. TIKVART: And what was your one or two sentence question?

MR. TROUT: Okay. My question in regard to the specifics that have been suggested during this conference are obviously complex, involved and cause greater resource requirements, and, two, I don't think that what we see historically is that greater funding is going to be available or we're going to have greater amounts of time or objectives in providing these studies are going to change.

In light of those given's, we would like to, or I would like to solicit specific recommendations, particularly from industry, their consultants and state and local regulatory agencies for any specifics on the how's as opposed to the should's in accomplishing these

objectives which are the motherhood and apple pie statements that we typically hear.

MR. TIKVART: Good, we have a taker.

DR. MOE: Rod Moe, Texas Highway Department. There's a highway research project called NCHRP 2018, and this project is well funded by the Transportation Research Board, funded actually by a lot of highway departments all over the nation.

The idea behind the project is to develop a data base of line source modeling, and what they've developed actually is a five data base. They haven't developed these, but adapted five data bases so they can be used to test models.

They're right now in the throws of publishing a final report. They've also developed what they call a figure of merit. The figure of merit has five sets of statistical tests to test different aspects statistically, the different test methods, different aspects, like one might look at say how well it predicts all kinds of values, you know, for any model you pick.

How well it predicts say the extreme values, you know, the highest to the second highest and so forth, that may be another set of tets, but with the figure of merit, the standard data base, you've got a testing ground based on this research project where you can

actually go out and if you have a new model, test it against the data base and against different data bases is what you should do. You shouldn't ever test it against the data base you develop it with, and use the standard set of statistical methods that handles different aspects of the thing.

When you wind up, you can get actually something that people can look at as kind of an objective judgment to how well that model does. Now I think that same sort of thing might be done for point sources, or you might even use an entry data base.

That's not a question, that's a statement of one approach.

MR. TROUT: There are five different data bases?

DR. MOE: There are very few data bases that are good enought to --

MR. TROUT: Did i misunderstand you that you said that there were five different data bases developed for this study?

DR. MOE: Right, they weren't developed for that particular study. They're data bases that were adapted for this research study. They're existing data bases.

Data bases with enough detail, you know, that

they had good enough measurements to be able to be used to validate models.

MR. TROUT: I would suggest if you feel that these have utility towards these objectives that you forward a copy of this report or those proposals or whatever you may have that you feel appropriate towards the subject of this conference.

DR. MOE: The research study hasn't been published yet. I'm working on a panel that chose a subject and chose a research and will be published shortly. When it is published, it might be made a part of the minutes of this meeting possibly, but it's not been published yet.

MR. TIKVART: Thank you very much. Any other takers? I think Dennis has asked a very challenging question, and I would encourage members of any consultant groups to take that question to heart, and perhaps after you have some time to think about it, submit written comments to us, because we're essentially asking you, how would you prefer the agency to conduct business in this area. That's the question we've asked you, so we'd like some response on that. Thank you Dennis.

MR. TROUT: I'd just like to say that it would be much more efficient for you as industry or consultants, when you meet with your regional offices, if you take

the time and effort now to save yourself the additional cost and time.

MR. ANSIS: I have one question. I'm Dick Ansis from ITT. You asked a good question. Would it be possible that you'd have a written statement? It was hard to write everything you had down that we could take with us on that?

MR. TROUT: I'd be glad to give you a list of what I have. Otherwise, there will be copies of this in the record which is available from the gentleman sitting up front here.

MR. TIKVART: Speak in the microphone please.

MR. ANSIS: That would be after you have a chance to make a comment on it.

MR. TROUT: After all the comments had been received, you'rse saying?

MR. ANSIS: If we got a list of your question now, we could have a chance to make a comment before the report was closed. If we wait to get the transcript, we may not.

MR. TROUT: If anybody has particular, I'll be glad to type them out if you give me a business card, I'll have my secretary send you a copy of my version.

MR. TIKVART: Yes?

MR. VAN VLECK: Lowell Van Vleck from Tuscon

Electric. A comment on your five criteria here, one of which was the model should be scientifically defensible or something of that sort, and presumably this means something like taking into the curvature of the wind height and maybe a large scale oscillations or some other such things as this, and you want an adequate, you say, meteorological base, and you want measured source terms and measured meteorology.

MR. TROUT: Let me correct you a second. It's the community that wants. I am repeating what has standardly been requested of us. You can hand be a black box, and that's what you propose for what's necessary for regulatory purposes, and that will meet with the public comment and scrutiny, I'm just repeating at this point what we standardly hear as the motherhood and apple pie statements.

That's why I said I wouldn't give my paper, because it follows in some of these natures, very resource constraining.

MR. VAN VLECK: Sut I think though and at least my experience is that this is beyond the abilities of the meteorological community. They have been unable to find those variables through many, many, many field trials that have really any proven ability to predict dispersion. The first thing we had was from Mr. Moe

today where he talks about the Helmholtz and gravity waves being significant factors in perterbating our general conceptions of controlling a dispersion process.

His was all short time, and it's hard to see how we can apply this for 240hour periods, but it's the first thing that's come forward in about 20 years now that may offer some chance for improvement, but I bet you you can spend $140 million and take ten years to make that work.

It may work, I don't know, but it will take that kind of an effort to prove it. First, you've got to go with his process. He's got windshear at every level.

MR. TROUT: This is exactly the point. It is a complex situation. Scientists recognize this. However, this is standardly what is recommended. We all recognize that, and you said as an excuse why we fail to do our jobs later, and if you can provide an objective or a scenario that we can accomplish our objectives in the regulatory sense of protecting ambient air quality and not being overly restrictive at the same time, I would like to hear it as much as you would, I feel.

MR. VAN VLECK: I think you just have to rearrange a track that will bypass this modeling business

and yet provide the atmosphere with substantially the same amount of protection and illuminate it. It's an administrative or procedure technique, but it could be done, and you don't have to go through all this modeling and meteorological measurements and tall towers and all these things.

MR. TROUT: I'm not disagreeing with you. That is one alternative.

MR. VAN VLECK: But I don't think we're going to get there, from what I know about all the things that have been tried to predict dispersion.

MR. TROUT: I would suggest, and I would welcome any scenario that you can provide to us, if you want to provide the detail that can stand up so that we can try to implement this type of policy so that we can evaluate.

MR. VAN VLECK: You mean the second approach I mentioned?

MR. TROUT: Any approach. That second approach is, I think, will be evaluated in the same context as any other suggestion that is made, and I would open this up to some free thinking, because I think it hasn't been stressed sufficiently before, that the cost of such studies are exorbitant.

We're talking millions of dollars for a source

study.

MR. TIKVART: Excuse me. I have a feeling we're going around in circles. What I'd like to request is the following. Dennis, if you and Mr. Van Vleck could clarify between you the issue and then if Mr. Van Vleck could submit a written suggestion as to how to approach the process of dealing with what model, what sort of data to use the specific application, I'd appreciate that very much. Okay? Thank you. Thanks, Dennis. I'd like to move on.

The next speaker is William Bonta, speaking for the State of Maryland Department of Health.

MR. BONTA: Thank you. It looks like you've closed nup your ranks there. Maybe if I turn this this way, I can hide behind it. Okay.

Good afternoon, ladies and gentlemen, and panel members. My name is William Bonta, and I am the Director of the Technical Services Programs in the Maryland Air Management Administration. I would like to first thank you, Joe, for this opportunity to take a few potshots at EPA. It isn't very often that we have that chance at the state level. I hope you guys remain calm in your seats until the end of my talk, because my aim isn't so good sometimes, and you've got to be still.

Seriously, however, I find the theme of this second conference very refreshing, and I would like to congratulate whoever it is that's responsible within EPA for regrouping the effort around a more macroscopic look toward the problem of modeling and especially its interrelationship with air quality program management.

I've been sort of distressed in the past about what seemed to be an inexorable slide toward uniformity with the federal government establishing all of the rules. After all, the objective in modeling has always been to mode accurately w enever it's used. Uniformity is a laudable goal, but it's not the primary goal.

For example, I don't see any reason why a model can't be tuned specifically to a local condition and applied only there and nowhere else. Why, after all, does a model have to work in Cleveland, Ohio and Nome, Alaska, if it was empirically tuned to work on the coastal plain next to the Chesapeake Bay.

We all realize that there are many parameters that are obtained empirically. Theoretical diffusion only goes so far in this game, and after that point in time, there are many black box coefficients and variables that are built into the programs that you and we both developed.

If we include explicit climatological terms

in the theoretical diffusion equations and then say that these equations can be utilized throughout the rest of the country, in other words, pick out these as independent variables, then I think that's a large mistake, because it's quite possible that some of these black box coefficients that you've tuned into the models that you've generated also contain climatologically related factors.

It might vary between say Nome, Alaska and Cleveland, Ohio and the coastal plain next to the Chesapeake Bay. I've had a number of conversations with Al Cimorelli about this difference of opinion, and sometimes quite frankly, I wonder if I'm the only one that has the difference, and everyone else thinks the same way you guys do, but that point aside, I'd like to mention several things about the background document which was sent to me to prepare for this meeting.

My first comments that I would like to talk would be about my dislikes, I guess, and then I'll follow those items with the things that impressed me favorably, and that wa maybe you'll be my friend again when I sit down. Okay.

My first problem with the statements made, they assume that everybody realizes that modeling is necessary to all situations. Well, I don't agree with

that necessarily. I'm an administrator now after many, many years, and not a modeler any more, but I just note with cryptic interests that it was a conference of modelers that sort of made that determination, and I'm going to let that point rest where it sits.

There are many situations where I suppose you could say analogies could be drawn from operating plants with adequate surveillance in the area surrounding the plant over many, many years would easily substitute for modeling analysis in the event the plant proposed to change emission rates.

The second point I noticed was that when taken in context of a widespread urban area with many sources, it seems to me as relationships between far reaching program design and air quality levels can quite often be drawn accurately utilizing simple proportioning techniques rather than diffusion modeling especially when ambient air quality levels are directly proportional to emission levels, and I know that may sound a little bit reactionary, and I'm on the right side of the stage here, but I still think that from the state level, some program opinions should properly be drawn that way, especially long range estimates of what's going to happen with air quality.

The third point, and this is the one that

I would like to dwell on a little bit more than the rest is that I think I'm strongly opposed to the conferees recommendations about introducing uncertainty, explicitly into the air management decisionmaking process. I'm uncertain why I even came here today to fight with you guys but that's beside the point.

Most of the problems dealt with in a regulatory agency deal with continuous variables, and we know that. We do not very often relate to exact situations except for maybe when the light switches on or off in the men's room when you walk in the door.

Why dhould uncertainty be explicitly considered only in the modeling aspects of the program when it is not considered in such a fashion anywhere else. That doesn't make sense to me.

For example, why should not uncertainty be included in an engineering evaluation of a piece of control equipment? That was mentioned yesterday. After all, we know that equipment does fail and these failures sometimes in a stochastic manner. Also, the failure rates and variability could probably be predicted during the design of a facility.

Such a failure, if it occurred on the worst possible dispersion day could just as easily cause an ambient air quality standard violation as the situation

where a high sulphur level in a load of coal is burned on the worst possible day. In general, these factors are considered implicitly in the permitting section's review of a plant in question.

In a similar fashion, the uncertainty over modeling is also known and is considered in the deliberations over permitting new facilities, at least in Maryland I can say that with safe assurance that I'm correct.

It is just as unrealistic to say that equipment never breaks down, and it is unrealistic to say that the worst-case day is the worst that we shall ever see. Why, therefore, is EPA focused on ExEx methodology and other such discussions for uncertainty in meteorological calculations and not emphasize the same sort of uncertainty in plant operational problems.

If a Monte Carlo sensitivity analysis is to be performed on a source that is proposing to build, why shouldn't the analysis include all of the factors that could lead to high ambient air quality levels?

Indeed, I find it curious that ork Group I at the Airlie House Conference and EPA, in general, have been sniffing about this uncertainty issue for the past several years. I see it in the discussions of the CASAC meetings with their discussions of risk assessment, and I see it in EPA's proposal to use

expected exceedences to evaluate remote power plants, and I notice it in the Work Group recommendations to consider factors like, quote, "location and extent of the geographic area where standards/increments are most likely threatened", unquote, and also where they recommended looking at, quote, "exposure or dosage estimate," unquote.

I think that what I'm seeing here is what I saw 15 years ago in the radiological health area where certain groups within the Atomic Energy Commission, then the Atomic Energy Commission, were basing decisions for aerospace hazard evaluation on total risk assessment, and other groups, the groups that were dealing with reactor safety problems were dealing with maximum credible accidents and basing the design of plants on those kinds of criteria.

I don't think it will be too long before somebody in EPA gets the general idea that total expectations can be used as the measure of a new plant being built. That is to say that from the time you sign the permit to the time the thing has been operating for a number of years, how many cases of emphysema do you expect to see, or whatever, and if the total expectation is less than one, somebody would say, well, we don't expect to see anybody get it, and that's all right, and design

the plant so it's down to .01 to give a factor of a hundred uncertainty measure.

This is what was done in the aerospace nuclear field. The situation was different, but it's analagous. I don't want to get into any detailed discussion of these issues in the small time available, but I would like to point out that the single value index is in my opinion the number that modelers should be calculating and that explicit expressions of uncertainty will serve no other purpose than to generate further uncertainty over those procedures and numbers.

I know from talking with program decision-makers that they get very upset with me when I try to express uncertainty about a calculated result from a model or even a measured result from an ambient instrument.

There are many instances where regulatory standards at the state and local level are based on arbitrarily fixed standards, highway speed limit, for example, a frostline for building a water line, a color form content in food.

The law recognizes that in dealing with continuous variables, the standard is always determined arbitrarily; however, as long as there are sound and sufficient reasons for choosing a particular number, the

regulatory program can defend it in court. What EPA seems to be suggesting is that not only is that number an uncertainty to be documented in the original standard setting process, but all of the subsequent uses of that number must consider similar uncertainties as well.

Imagine what problems would unfold if the state police were to treat a speed limit the same way. It's been my experience that the pressure to form finer and more technically sophisticated standards, regulations, et cetera, have stemmed from enforcement personnel who oft times get mixed up in discussions where they do not belong.

For example, enforcement personnel try to answer questions posed to them by industry regarding program design. The answer delivered in cases like that should be, "That's not my concern, I don't write the laws, I just enforce them."

Many times, however, the complications that I've seen dreamed up arise through enforcement people, particularly the lawyers who imagine that they can't possibly defend themselves in court against the myriad of questions that they are being asked by industrial people.

Many times the lawyers don't have the courage or the courtroom experience to make these judgments.

I assure you that by designing a more complicated system to answer all of these questions will in no way reduce the numbers of attempts that environmental lawyers will make to circumvent compliance.

Bernie Steigerwald mentioned some of the problems over uncertainty issues yesterday, like autocorrelation and plant failures, for example. I have the feeling that he has just uncovered the tip of an iceberg.

When one starts looking at the beginnings of new program element, the element normally increases its complexity very rapidly as the program is implemented.

I need only cite the significant deterioration program as an example that proves my point.

And yet here, with the uncertainty proposal the EPA is proposing to complicate things in yet another matter. I know when a Program Administrator is forced to make a decision in Maryland, he doesn't want to hear about the uncertainty. All he wants to hear about is whether or not his best people have done the best job that they know how to provide an accurate number and whether or not that accurate number violates the law. Adding an error band about a modeling result could very well increase the legal problems rather than decrease

them. On the other hand, if there is an -- well, environmental groups, you know, the pressure from industry to say plus or minus, you could be high by 30 percent, and you say, well, yes, we could be high by 30 percent. If you take that error band, what are you going to do with it then.

If you say yes, indeed, the calculated value could be lower by 30 percent, and the guy says, ah-hah, I'm in compliance, and then he puts a little bit of pressure on the politicians, and they twist your arm and you give them the permit, and then the environmental group looking over your shoulder goes to court, because he says no, it wasn't 365 down to 330, it was 365 to 390, because it's plus four minus.

So I think what you're really looking at is the mean value anyway, and I think we're all going to target into that, and that's the best thing we ought to be looking at.

Just maybe a cryptic comment, my impression in the modelers dilemma was expressed in the sentence from the support document, "Furthermore, for air quality management and regulatory process to ignore modeling uncertainty and to continue to base decisions on past estimates, single value measure, such as the high, second high concentration, places an unduly heavy burden on

modelers," unquote, and my overall sentiment is, Aw, gee, that's tough, fellas.

It seems to me that the fellows in the programs at the firing level, and I'm sure you're there in many situations who have been looking at hardware are doing this all the time. The registered professional engineers see equipment design and they know there's uncertainty with that. If they approve a lemon, boy, they're going to eat it, because it's going to be out there putting out the smoke, and the thing doesn't work right.

We all know that, and yet they aren't talking about uncertainty, so I think you ought to consider this all around.

Let me get to the positive reactions. Now that I've dug you enough, I might as well get a little kinder. On the matters of those positive reactions, I was very impressed with the protocol approach. I like that. It sounds good, I hope it will work.

I think that the establishing of protocols will protect both the company and the state from a great deal of delay due to misunderstandings which routinely occur. Guideline documents should contain examples of pitfalls and traps, such that the public agency can have ready guidance on other things to look for in circumstances where maybe a streetwise consultant knows

more about the modeling than the individual and the public agencies dealing with it, so it can work both ways. It can protect industry and the control agency from a lot of circumstances that you know of exist, and we may not know it in the state agency.

Okay. With regard to the central modeling operation, I guess Joe is the central modeling operation. The question is are they well enough insulated from a political standpoint. I don't know that question. Perhaps it's good enough for the present time.

I think that I agree that a technical oversight committee should be established, but I believe it should be advisory in capacity and, therefore, not have any approval rights, or else the advisory council may be tainted somehow.

Oh, another thing about the recommendation, the modeling center should be a suitable group to provide protocol guidance. I also noticed that in Egan's group there was a recommendation that, quote, "a suitable form (up sup.)" whatever that means, "should be identified and established to resolve anticipated or unanticipated issues," close quote.

This wording appeared kind of odd to me since it seemed to defeat the whole purpose of what you tried to do in coming up with a protocol document. The wording

seems to be so broad as to allow anyone to escape anything agreed to in the protocol document, and a smart environmental lawyer, consultant could play a tune on that one, so maybe you need a lot more thought on that concept.

Overall, I'm very impressed, and this is my summary here, I'm very impressed by EPA's approach toward this modeling conference. I like the open minded attitude exhibited by the turn around from the first proposed guideline/revision, and I hope that going with the protocol and the modeling center, technical oversight committee that the modeling chore can be simplified.

Perhaps the pendulum that we see that is about ready to dismember the PSD program will not also chop and seriously affect our modeling activities as well. State of the art modeling has progressed so well in the 12 years I've been in the air pollution programs. I'd hate to lose it at this time, because the trend is towards simplicity in government, as you know, and I hope that you can successfully cast the die towards that end. Thank you.

MR. TIKVART: Any questions from the audience? No? Bill, thank you, as always, for a thought-provoking presentation.

The last government agency presentation I have is Kenneth Mequire for the Kentucky Division of

Air Pollution Control.

MR. MEQUIRE: Good afternoon, ladies and gentlemen, and gentlemen of the panel. I've had to rewrite half of this in the past few minutes. I hope it won't suffer too badly.

Actually it fits in pretty much with what Mr. Bonta has just said in that the matter of statistical scatter has not only administrative problems, but it also is only part of the picture and it's the remaining part doesn't seem to have been dealt with very much here so far, and that is the fundamental accuracy of Gaussian equation and modeling which is contained in practically all of EPA models.

There are problems with that model, becuase it requires a finite velocity of the wind on the one hand and on the other, there are three fairly important areas in the United States where there's an awful lot of no wind at all, areas that the wind power people will never make any progress with, and this has to do with the area of long calms.

The problem there is all of our weather data that we use in Kentucky comes from the EPA. It has no velocities reported under one meter a second, and usually everything we get is where you have a period like that. They'll report one meter a second for each

hour which is what we use hourly, and you know good and well if you get six or 12 hours in a row reporting one meter a second, and that's the lowest it'll go, you know that it went below that, but you don't know by how much, so that's what goes into the computer modeling, and we do get an artificial result as to our modeling.

What this tends to do in an area of calms, it tends to understate the actual concentration, because under a condition of no wind you're going to have over a duration of time, and that's also something that's not directly taken into account in the EPA models, you have a build-up, and usually the heaviest calms occur when you have a period lasting through the night and into the second day, this type of thing. It can go on for days.

So these cause understatements of the actual concentration in areas where you have maybe 200 periods of calm, 200 days with these calms out of 365. Those are in the southern Appalachians, in the Salt Lake Basin, in coastal California, according to the EPA's own publication on the subject.

There's another problem we have, and that is since we do have only EPA weather data, that comes to us with the input as to the velocity with its

limitation, and also there's a problem of the stability. It comes to us that way.

Now if we try to do any monitoring or get weather data for modeling from a local area in the Appalachia Valley, we are up against it, because we don't have a system for converting various meteorological factors into one figure, the Pacquill stability, and we could very well need that for operating on our own, if we were going to use the Gaussian model.

So my feeling is that this type of weakness needs to have a better priority than a consideration of the scatter, statistical scatter that prevails where you're just using a standard procedure to actually do the computing in the computer.

So that was my comment, and I feel that it's an addition to the last statement, and it also represents a comment on the first presentation asking -- Mr. Mr. Dennis Trout asked for some comments on what's needed in order to improve the meteorological data that's used in modeling. Thank you.

MR. TIKVART: Are there any questions?

MR. HAMBURG: Fred Hamburg speaking, from RMC. I don't understand the statement about the non-reporting of calms. It seems to me if you get your weather data from the National Weather Service, I presume

you meant EPA?

MR. MEQUIRE: Absolutely. I've called down to NOAA in Asheville to try to get some weather data for some additional pounds that I know has weather stations, but we don't have it, and when I started asking about stability numbers, they never heard of them. They wondered what on eart I was talking about.

MR. TIKVART: For the record, I should clarify that the data you're referring to is undoubtedly National Weather Service data processed through a special program by EPA to generate stability classes.

MR. MEQUIRE: Right, that's what I found out subsequently, it is like that, so the way we operate now, please excuse me if this sounds too harsh, but we are completely at the mercy of EPA when it comes to getting basic weather data to put in our computers.

MR. HAMBURG: I'm just at a loss to understand this lack of meteorological data because it seems to be available everywhere. If you want to get synoptic data, if you want to get timely data, you can get it from Louisville Airport. Well, the only thing I'm trying to bring out as an old-time weather bureau hand myself, I don't know of any situation where we were forced to write one mile per hour or one meter per second when it was a dead calm. There is circular uncertainty

provides for zero direction with zero wind. Now, of course, modeling has a problem with that, and particularly when you get into CRSTER modeling where you make the assumption that when you have that, you have to look way, way back to get the last one that happened, assume it's continuing to happen, and that, of course, gives you three hours in a row with extremely high concentration. Now I understand your point, but with the criticism of the meteorological data, per se, I think it is unwarranted.

It makes the computers work, although in a certain way, and I'm not sure exactly. The only thing I will say is this. We have never, ever been able to get a concordance between monitored data and modeling data. Now, the modeling data comes from some previous year or years, hourly, 8760 hours worth for a year or for the average or 64 to 70, that's six years or seven years, whatever.

That's the way it comes to us, and that's the way it's processed. It comes to us through the EPA. We right now don't have any facilities that I know of for handling information that would come directly from the MWS.

Let me cite you a little problem we had recently down in a certain industrial area known as Calvert City,

Kentucky by the Tennessee River near its mouth.

MR. TIKVART: Can you keep your example short please?

MR. HAMBURG: We found out that if we used Paduka weather, Salem, Indiana weather or Nashville, Tennessee weather, each one of us gave absolutely different reports on the compliance or non-compliance of this plant there, and I forget the choice that was finally made, but we had a hot old time there. We were all laughing the whole time.

MR. TIKVART: Thank you very much. Are there any government -- representatives of government agencies in the audience who wanted to speak but did not have the opportunity to do so? No? Okay.

I will remind you that a representative of the National Academy of Sciences has expressed a desire to speak tomorrow morning. That will happen right at 9:00 tomorrow morning.

I'd like then to move into the next phase of the conference whereby individuals have expressed the desire to make presentations. The first of these will be Ralph Sklarew from Form and Substance. Where did Ralph go? He was here. Charlotte Hopper, is Ralph Sklarew out in back?

Out to lunch, did you say? Well, we'll give

Ralph a chance later on. Next we have Jerry Pell. Jerry, as I understand it will be speaking for the Meteorology Committee, TT-3 of APCA.

MR. PELL: Thank you. Mr. Chairman, Members of the Panel, I am pleased to have the opportunity to participate in this conference on air quality modeling. I am Dr. Jerry Pell, immediate past chairman of the TT-3 meteorology committee of the Air Pollution Control Association.

I am also a certified consulting meteorologist of the American Meteorological Society, and I'm here today on behalf of the APCA meteorology committee. There are 41 members subject to revision, because we have people joining us in leaps and bounds presently on the committee all of whom are meteorologists and members of APCA, and of the 41 at last county, 21 were also certified consulting meteorologists.

Together, we represent the views of government, utilities, industry, consultants and academia. All of us are concerned with air pollution problems and with air quality modeling. The following statement is the statement of the committee.

Undoubtedly air quality prediction modeling is an important tool used by regulatory agencies in the permitting process. However, a model must remain

a tool and not itself become a decision making. Air quality models are currently being used as tools in two distinct types of regulatory applications. The difference between these two types of applications and their relationship to the history of air quality models are not commonly recognized. Nevertheless, they have a strong bearing on the cost of regulatory policy.

Several years ago, the only air quality standards were health related. These standards were mandated by the Clean Air Act and promulgated by EPA. The standards were held as a goal to be achieved at virtually any price. It was felt that protection of the public health made any violation of the standards impermissible.

Air quality models were developed and approved by EPA eo ensure eompliance with these health-related air quality standards, and, as a result, these models necessarily acquired a built-in conservatism to reflect their historical purpose of protecting public health.

Now, however, the Congress and EPA have set "arbitrary" standards -- arbitrary, as we've heard from previous speakers, in the sense that they do not have a clear relationshp to a known deleterious effect such as mortality or morbidity. Included among these standards are de minimis levels and Prevention of Significant Deterioration increments; these are arbitrary numbers

set by regulatory fiat. Nevertheless, we are still using the same conservative "thou shalt never" set of air quality models for assessing cmopliance with these arbitrary standards.

Models, as we all well know, are simply mathematical constructs of environmental processes. Early in the history of model development, model performance was related to reality on an average basis. Some predictions were higher and some predictions were lower than actual field measurements. The average of the predictions matched the average of the measurements.

To meet is mandate for health protection, EPA conservatively chose, as one of the criteria for success in model performance, to emphasize model prediction of higher concentrations than observed, thereby ensuring protection of public health.

Purely on the basis of this tradition, we have begun using these historically conservative models for other well intentioned regulatory purposes, but without a full and clear understanding of the cost impact to society.

Requiring the use of these highly conservative, health-based, models for assessing compliance with arbitrary regulatory standards can be very expensive and probably is not cost effective.

Since the numerical values involved in these arbitrary standards are relatively small to begin with, the use of a model with a strong conservative bias imposes a very onerous compliance burden. The Committee, therefore, urges that, for these arbitrary standards, models with more realistic performance criteria be used.

Despite these reservations, modeling is the only tool available to estimate the potential impact of proposed new sources or of proposed major modifications to existing sources on ambient air quality. It is also the only available way to estimate the degree to which emission standards for existing facilities, and hence the costs of air pollution controls, could be reduced and still comply with the applicable air quality standards This is becoming increasingly important as we seek ways to achieve air quality standards in the most economical manner.

Nevertheless, air quality prediction modeling is not an exact science and there are inherent uncertainties in the predicted maximum concentrations. The results of an air quality modeling study and the regulatory consequences of these results depends very importantly on the particularly model chose, the input data provided to the model, and how the results from this model ultimately are used to develop air quality control regulations.

This assessment of the importance of air quality models is consistent with the conclusions of the National Commission on Air Quality, and for those of you who have the written statement, the recommendations of the NCAQ panel on atmospheric dispersion models is provided as an attachment to this written statement.

Accordingly, in recognize of the importance of air quality models, the APCA Meteorology Committee fully supports the continued reliance on models as basic planning and evaluation tools, but the Committee hopes that the uncertainties of these planning tools will be recognize within the context of specific applications.

Turning to the discussion of the accuracy of these models, the accuracy of an air quality model depend on the purpose for which the model is being applied. A model can be expected to be more accurate in predicting the maximum pollution concentration to be found in the area surrounding the source than it is in predicting the exact location of that maximum or the exact time of that occurrence.

Studies comparing predictions with measurements repeatedly have shown this to be the case. This is probably due to the fact that the pattern of the wind is not known sufficiently precisely. For example, a two percent change in wind direction can lead to a 1000

percent increase in the predicted concentration at a given fixed location using Pasquill Gifford dispersion coefficients for E type stability, or moderately stable atmopshere.

By carrying out predictions over a large number of observations and for a large number of locations, the inaccuracies due to the uncertainties of the wind field are partially compensated for.

In most regulatory applications, it is not necessary for a predicted model to be able to determine the exact time at which the highest concentration will occur. In many applications, it is also not necessary for the model to be able to determine the exact location of highest concentration.

Thus, in the evaluation of how accurate a prediction model is, one must take into account the purpose for which the model is being used, and on the subject of purpose, we heard some earlier discussion of the kinds of purposes used in the nuclear industry as opposed to more conventional air quality control considerations.

Several issues arise in taking model accuracy into account in regulatory decision making. These include one, should generic ranges of uncertainty for models be provided, and used explicitly in the regulatory

process? Two, should site specific ranges of uncertainty be developed and used in the regulatory process, and, three, should uncertainty, once it is quantified, be used explicity, quantitatively or qualitatively in regulatory decision making.

Concerning the first issue, the accuracy of a prediction model is highly site-specific depending on the source characteristics, meteorology, and topography of the surrounding area. Thus it is very doubtful that a generic range of uncertainty can be specified for a given model. The Committee therefore recommends that generic ranges of uncertainty not be developed or used explicitly in regulatory decision making.

On the other hand, the accuracy of a particular prediction model at a given site can be determined by careful comparisons of predictions with measurements at that location, such that a reasonable estimate of the accuracy of the model in predicting the maximum concentration can be made.

If such an estimate is known, it can play a useful role in regulatory decision making.

The third issue, whether to quantitatively incorporate uncertainty in model predictions into the regulatory decision making process, has been considered by EPA. The written text quotes an internal piece of

correspondence at EPA. The evaluation concluded that it is redundant and improper to include modeling uncertainty as an explicit variable in the Expected Exceedances method. This conclusion was based on the fact that modeling errors will almost always result in an overprediction of maximum concentrations.

We support that conclusion. There are a number of issues that arise in regulatory decision making that cannot be quantified to the point of leaving no discretion to the decision maker.

The Committee recommends that estimates of modeling uncertainty be accounted for qualitatively in decision making by regulatory authorities rather than explicitly in the results of the prediction modeling analyses.

In conclusion, I would like to note that the TT-3 meteorology committee of the Air Pollution Control Association would be pleased to assist EPA with establishing model uncertainties by drawing on the experience of our large and diverse membership. We furthermore offer to serve as members of a peer review board if such a board were to be formed to assess model accuracy and uncertainty.

Now I'll be pleased to answer any questions you or members of the panel may have, Joe.

MR. TIKVART: Yes, I have an observation and a question. The observation deals with you're referring to the Steigerwalt-Burton memo. It should be noted for the record that that report discussed conclusions based on interim results.

I find that people are too anxious to grab anything that comes out of EPA as interim progress reports and run with it. As a matter of fact, that work is still ongoing, not completed, and I'm not sure that when we get through with this, we will find that models as they have been used in the past have been overly conservative.

I don't know. That's an iffy proposition, but to draw the conclusion that the way we have used models in the past definitely is conservative is only an interim conclusion, and the work is not done on that, and from what I've seen it could well go the other way. That's the observation.

The question is, in the early part of your presentation, you seemed to indicate that the models used have been overly conservative. That doesn't seem to square with the presentations that were made yesterday morning whereby the standardly used EPA model was shown to be unbiased and in some cases underestimated concentrations. Would you care to comment on that?

MR. PELL: Well, on the question of the internal memorandum from Bernie Steigerwalt, I take note of your comment, and I appreciate the input. The testimony properly should have included the notation of the status of that work.

With regard to the conservatism of models, the concept has been, and I think it's been reiterated in some of the remarks that I heard today that when the public health is in question, if you're going to err, you err on the side of conservatism and if a model design is to be approved for regulatory purposes, then the best option would be when uncertainty is present is to opt for a model which tends to overestimate the concentrations rather than under.

So as a matter of procedural policy, my impression and the impression of the committee has been that the working concept has been when you're not sure, go for the higher end of the scale rather than the lower end, and if the plant turns out cleaner than you had intended, so much the better. If it were to turn out dirtier, you'd have a problem on your hands.

MR. TIKVART: Thank you. Any other questions or comments? Dennis gets there first.

MR. TROUT: I would like to make some comments on the word conservatism because we --

MR. TIKVART: Excuse me. I don't know that you identified yourself.

MR. TROUT: Dennis Trout, EPA Region V. And we're frequently accused of putting in all conservative assumptions, and this is something that we're particularly sensitive to, and, as a result, we put together lists of conservative, liberal and case by case dependent factors, and at a recent meeting of the APCA in St. Louis back I think in April, I went through this list.

Of that 25, 11 were classified as liberal, seven as conservative, and seven case by case dependent. At that time, I also solicited from that conference which was heavily attended by industry lists of any additional factors that they might consider liberal or conservative.

To this day, I have not received one additional addition to that list, so I think the statement that our models err intentionally or on the side of conservatism has not been founded, and from studies that have been shown including some of the EPRI studies and all the others, I don't think there is technical support to arrive at the conclusion that our models are conservative.

MR. PELL: Perhaps we can chat about that over a drink some evening, Dennis.

MR. TROUT: Okay.

MR. TIKVART: Doug?

MR. FOX: My name is Doug Fox. Jerry, I'm pleased to see you've made mention of the National Commission on Air Quality, Dispersion Modeling Panel policy statement, and my comment is just simply for the record to point out that actually the statements of that panel are contained in about a ten-page article that was published recently in a 200-page report.

I didn't want anybody to have the impression that this was, in fact, the only output of the NCAQ, and I'd like to recommend to Mr. Tikvart that somehow or other that NCAQ report be made part of the record for this proceeding.

MR. TIKVART: Would you submit it, Doug? Thank you.

MR. PELL: The codicilion point in the admittedly brief excerpt which is used in my statement from the NCAQ panel on atmospheric dispersion models is probably the sentence that that particular NCAQ panel recognized, quote, "that models are useful technical tools for air management decisions, but models alone cannot be used as a sole determinant in such decisions," and I believe it was the sense of the committee that that was the cogent point worth bringing to your attention

today, but certainly there was never an intent to diminish the magnitude both qualitatively and quantitatively of the work of either the Commission or of that panel.

MR. TIKVART: Any other questions or observations? Thank you.

MR. PELL: Thank you very much.

MR. TIKVART: Next we have Richard Hanson who represents ITT Rayonier.

MR. HANSON: Thank you. My name is Richard Hanson. I'm the Air Quality Group Leader, Olympic Research Division of ITT Rayonier in Shelton, Washington. My comments deal with the use of dispersion models in the regulatory decision making process and on the accuracy of these models.

They relate primarily to modeling in complex terrain. However, they are applicable to modeling in any complex situation whether it is terrain, source or meteorological.

Dispersion modeling in complex situations is difficult. No model currently exists that is uniformly adequate for complex terrain. For the most part, existing models in complex terrain are conservative. This may be satisfactory for a PSD analysis if enough increment is available and no other new source is likely until more advanced models are developed.

However, regulatory agencies have considered designating non-attainment areas based on modeling results and they have not adequately considered the conservative nature of the models.

My first recommendation is that is non-attainment decisions should not be made only on modeling results. They should be made using actual monitoring data. The modeling results should only be used in support of the monitoring data.

If a model predicts a non-attainment area, the problem should be documented with actual monitoring data. Yes, this would be expensive, but not nearly as expensive as unnecessary mission reductions.

You may have to monitor a long time before the worst case or second worst occurs. Perhaps the worst case does not exist in the real world.

My second comment deals with the validation or calibration of models applied in complex terrain. There should be some physical justification for changing the measured meteorological data other than just that the model doesn't work without it.

I'm talking mainly about the measured wind direction; however, it also applies to the other meteorological parameters. Other likely causes that should be considered include unquantified sources may have

a major impact. Fugitive sources can cause high ambient concentrations. Their mission rates are many times not known.

The measured meteorological data used may not be representative. If the wind direction shows that a receptor is not impacted during an averaging period, all the other meteorological parameters may also be wrong.

The model may not allow for the proper plume interaction with terrain. The model used may not be suitable for the situation. If the meteorological data is adjusted it is possible that the model would show that the emission sources which in the real world do not impact the receptor during the averaging period, are the cause of the ambient concentrations.

Under this situation any modeling is totally meaningless.

My third comment is about representative meteorological data. On site meteorological data should be mandatory when modeling for regulatory purposes. At least wind speed, wind direction and ambient temperature should be measured near the emission point or in the receptor area.

On site data should be used even if meteorological data is available for a longer period of time

from a nearby site. Data from a few kilometers away may be totally unrepresentative. If sufficient on-site data is not available, it should be obtained before decisions are made.

These points are not based on a hypothetical case. My company was faced with a non-attainment designation based primarily on modeling results. Ambient SO2 standards had been exceeded several times near a pulp mill. We had determined that the source of the ambiet SO2 was a storage lagoon.

A temporary control for the SO2 emissions had been completed with plans for permanent control; however, two dispersion model studies had shown that the pulp mill excluding the lagoon could cause a non-attainment area.

Nonrepresentative meteorological data had been used in both validation studies and attainment analysis. These data were available for a longer period of time than the representative data. The emissions from the lagoon had not been quantified or properly considered. The wind direction was adjusted in the validation.

These studies resulted in totally erroneous conclusions, incorrect sources were identified as the problem. As a result, reduced emission limits were

proposed that would have had little impact on the ambient air quality; however, they would have had an adverse impact on the continued economic viability of the pulp mill.

Fortunately we were able to show that the major cause of the ambient SO2 was not from the incorrectly assumed sources in the pulp mill. Enough on site meteorological data was available to challenge the attainment analusis. However, this should not have been necessary.

This is one case that shows the need for some functional control over the use of dispersion modeling in the regulatory decision making process. That's the end of the statement that I filed with the recorder, but I have two comments that I'd like to make based on previous discussion.

We're talking about uncertainty in modeling. Uncertainty doesn't mean anything unless representative input data is available. Many times this is not the case.

Secondly, I agree with the recommendation about joint discussions between all parties before modeling is begun. That would resolve some of the problems that we had; however, much of the other work was done for a PSD application for another source.

They asked for a received a confidentiality agreement that restricted access to the data. Thus, we were not able to participate before the modeling was performed.

That's the end of my comments, and I appreciate the opportunity for making them at this meeting. I'm open to any questions.

MR. RHOADS: You were giving a real life example to make your point. It's not relevant to identify that particular plant; however, as I understand it, some modeling was done, and then you were able to point out through monitoring that the modeling that had been done was inappropriate.

MR. HANSON: No, that is not correct. We were able to use representative meteorological data. Enough of it was available during the key period of time to challenge the modeling done with nonrepresentative data.

MR. RHOADS: Okay, I understand. Did you consider putting out a monitoring network. Do you feel that your company when faced with that situation should put out a monitoring network to attempt to validate the model, or do you believe that should be more properly the government's responsibility?

MR. HANSON: In a situation like that where

it's an adversary role, I believe it's probably a joint participation so everyone agrees with what you're doing.

MR. RHOADS: Okay, thank you.

MR. HELMS: You spoke of making a non-attainment designation based only on modeling. Were there measured violations?

MR. HANSON: Yes, there were. The source was already controlled by the time the decision was made to look at non-attainment, so it would have served no purpose to declare non-attainment, because at that fixed date, it was no longer non-attainment.

MR. RHOADS: You had corrected the problem?

MR. HANSON: We had corrected the problem.

MR. TIKVART: Any questions or observations from the audience? Why don't you come all the way up, Ralph?

MR. SKLAREW: Ralph Sklarew, Form and Substance. This points out perhaps a big hole in the modeling that has been done heretofore in the guidelines, that is meteorological modeling, modeling to take meteorological measurements from a distinct point and simulate what the effects are at the local site.

I think that should be something EPA could spend a little more attention to and provide techniques for general use. That might get you out of the problem

with non-representative data.

MR. HANSON: I would agree with that. This is a very difficult place. I would think it's one of the most difficult anywhere we got in the country, but I don't believe that payment on non-attainment decisions should be made basically on modeling results.

The models just haven't been shown to be accurate enough in situations like that. This model that had been used, we had shown it probably now, it predicts by about a factor of two which is not really too bad considering the difficult situation.

MR. BONTA: Bill Bonta, from the Maryland Air Management Administration.

I heard this suggestion before from the men from Hunton and Williams yesterday, and it seems to be that you're inferring that you would in the failure of agreement on modeling terms set out a monitoring program to prove the point.

Are you extending that to evaluation by a control agency such as ours of a brand new grassroots facility?

MR. HANSON: Would you repeat the question? I didn't get the last part of it.

MR. BONTA: You seem to be suggesting that in the event that both parties do not agree on the use

of a model and its outcome that ambient monitoring measurements take a precedent.

MR. HANSON: Yes, I would say that. I would not imply that we did not agree on the model. We agreed which model to use. We did not agree, although I think the agreement is there now on which meteorological data to use, so it wasn't the question of model as much as the input data to the model.

MR. BONTA: Well, are you suggesting that one utilize the same technique in evaluating a grassroots plant, one that is not now there?

MR. HANSON: It is possible to do that, but that is a PSD type or an impact type of analysis, and if you have enough available, it's not hurting you, where attainment, non-attainment decision is a real world number that you're looking at with a monitor, and that is going to be more accurate than a model.

With the new source, you have no choice.

MR. BONTA: Okay, you're saying with the new source then you have no choice other than to use the model and make your determination based on that model alone?

MR. HANSON: That's correct.

MR. BONTA: Okay, thank you.

MR. YINGST: John Yingst with PG&E. Another

way you might have gotten around your problem was you had a lagoon that caused your observed concentration.

Now your model predicts an expected value. Your observed concentrations were extreme values. What you could have done is a rollback model type approach whereas if you reduce your emission concentration by X, your maximum observed concentration will not be linear but will be reduced by more than that.

So you might have gotten out of doing the modeling approach.

MR. HANSON: At the time we started with this, we did not know, nor did the regulatory agency know the size or magnitude of the emissions from this lagoon or their impact. It was only during the study that that came about.

MR. TIKVART: Thank you. We'll go to the next speaker. Ralph Sklarew, are you ready? Okay. We'd better get those in and move to the next speaker first while those get mounted, if that's okay with you.

The next speaker is Richard Fein who is with the American Petroleum Institute. Mr. Fein?

MR. FEIN: Thank you. I'm Richard S. Fein, a senior research associate with Texaco Incorporated, and, as was indicated, I'm speaking on behalf of the American Petroleum Institute this afternoon.

The API comments for this conference present a brief overview of the findings to date of two model evaluation programs. They also discuss the implications of those findings for current regulatory applications and offer recommendations for improvement of models and modeling practice and for review and revision of the regulatory applications.

API's model evaluation studies has focued on the standard Gaussian models recommended by EPA for assessing air quality impact for non-reactive pollutants. One program has utilized the results from short-term tracer dispersion experiments, and utilized the CRSTER model. The second program was described by Steve Wise yeserday, and it compared observed unpredicted sulphur dioxide concentrations around an industrial source complex using one year of continuous monitoring data collected by a nine station network.

Predictions were made using the industrial source complex short-term model adjusted for hourly emissions which is not a normal capability of that model.

Results from two of the tracer experiments are illustrated in Table 1. Note that 50 percent or median predictions, well, we see a ratio predicted to observe values for the maximum observed -- for the maximum on each sampling arc, so these have been adjusted for

wind direction, but they are coincident in time.

Note that the 50 percent or median predictions are baised towards over-prediction, but that ten percent of the predictions are at least about a factor of two too low. In other words, the predicted is only about half the observed at ten percent cut-off point on the frequency distribution of errors.

Also note that ten percent of the predictions are at least several-fold too high, much greater than a factor of two, as a matter of fact. In fact, in terms of the bias here for these two programs, approximately three out of four of the predictions were overpredicted.

The next table illustrates results from the industrial source complex for what we've termed three model use categories, and we've done this for three different averaging times.

The first category, areawide cumulative frequency presents a comparison of predicted and observed regardless of time and/or location. The second category sites specific cumulative frequency, presents a comparison based on the top ten observed and predicted concentration values carried by a station, and the third category, site and time specific concentrations, present a comparison based on the ten highest observed values with the predicted concentrations at the same time and station.

The ten percent median and 90 percent cut-off levels are indicated. Results of these studies indicate that the median agreement decreases and the range of uncertainty of the agreement increases as the model is required to predict the location and time of the impact.

Note that the highest observed concentrations, the righthand column there, are badly underpredicted and that ten percent of these high observed values are actually predicted to be zero.

The next figure, and I'll make a further point with respect to this, it shows the point that Steve Wise made yesterday, that predictions for some locations are biased towards overprediction while others are biased toward underprediction.

Also, as we just got through seeing here, most of the time, the highest observed concentrations are underpredicted whereas, although we didn't show it on the previous slide, most of the highest predicted concentrations are overpredicted.

This seems to be a generalization that occurs at monitoring sites most of the time. It's apparent from the results of these two studies that the Gaussian models, even in these rather ideal situations, and these were rather ideal, are not capable of predicting

concentrations at a given time or location with any substantial degree of certainty or consistency.

As time or location constraints are relaxed, model performance improves until in the areawide frequency case as was pointed out yesterday, the models perform quite well.

In light of this finding, it is worthwhile to examine the demands which common regulatory applications place upon a short term model's predictive capacity. The next table presents a schematic attempt to depict the model use type corresponding to various regulatory applications.

The first applications group, that is the top group there, represents cases for which the model is used to predict the magnitude of expected impacts, but the regulatory decision is relatively insensitive to the location of the impact. Hence, we talk about areawide.

For an isolated remote source or for the case where impacts from one source are predominant, the model is required only to estimate the concentration magnitude independent of time or location for an appropriate averaging time. For situations where multiple sources at different locations or a complex of sources at one location contribute substantial impact, the predicted

combined impact is highly sensitive to the location of impacts for each source for the same time interval and when we say same time interval here, we mean for a given set of meteorological conditions.

Hence for those cases where you need to use site and time specific concentrations. The second applications groups represents cases where the impact at a given location, such as a PSD Class 1 area or a non-attainment hotspot must be estimated.

These applications clearly require the model to predict a site specific concentration and pose a more stringent requirement for model performance. If the combined impacts of a number of sources must be estimated for such a region, the worst case combination scenario arises in terms of model prediction.

I think from what we've seen that it's obvious that modeling areas are too large to adequately achieve many, if not most, of the purposes of these regulatory applications.

Thus, for example, modeling for PSD baseline determination and increment tracking often requires accuracy and precision, that is far smaller than the approximately three to at least tenfold 80 percent confidence bands for predicted value shown in previous tables.

Use of inaccurate models for these PSD

purposes reduces the regulatory process to an arbitrary paper exercise which, as was stated yesterday, only creates an illusion of certainty.

In this illusory exercise, the model predictions take on a public relations and a legal significant, but bear no demonstrable connection to actual air quality. In view of the fact that modeling is the only method available for estimating the impact of a non-existent source, that is a proposed source or one that existed in the past, it becomes imperative to try to bring modeling applications for regulatory purposes into better agreement with modeling capabilities.

This can be accomplished by improving models and modeling practices and by modifying the regulatory requirements for modeling.

We'd like to make the following recommendations to the EPA, and these are summarized on the next slide.

We would recommend requiring that monitoring data be utilized to assist decision making involving modeling whenever such data is available. To do this it's necessary to develop methodologies for adjusting models with monitoring data and for using models for interpolating and extrapolating monitoring data.

Thirdly, we believe that the EPA should very carefully examine each of the regulatory applications

of modeling to determine how to bring these applications into better congruity with modeling capabilities, and then based on these careful examinations to either modify the regulatory requirements for the modeling applications or to recommend legislative changes necessary to modify these requirements.

Obvious modifications that are needed include one that was discussed yesterday, the necessity to re-express ambient air quality standards to better average out the uncertainties of air quality monitoring and model.

And, also we believe that in view of the errors in modeling that we should recommend elimination of the PSD increment system since one cannot calculate baselines or in any realistic fashion or increment or track the influence.

To protect Class 1 areas, the API believes that monitorable maximum concentration limitations should be established for each Class 1 area to protect the air quality values that are specific for that area. I thank you and would be happy to answer questions.

MR. TIKVART: I have a question relative to the previous slide, if we could go back. What do you mean by the multi-source example? The reason I ask that question is, in my experience, many times when

you have large sources which might be considered multi-source, they are still located too far away to have a joint impact at the same -- a joint maximum impact at the same point, the reason being that most maximum impacts occur within a few kilometers of the source.

Can you give some examples of what you mean by the multi-source case?

MR. FEIN: A typical large oil refinery, for instance, would be not -- would be a complex source which contained a multiple of individual sources.

MR. TIKVART: Okay, so in that case you're talking about multiple point sources that are really part of the same source complex?

MR. FEIN: Right, or other examples where one could get into this sort of problem would be in say the golden triangle area of Texas where there are many oil and petrochemical installations down there which jointly impact the same area and impact them significantly.

MR. TIKVART: Okay. One observation on the lower lefthand block. I think you make a correct observation, but it is probably also a function of the proximity of the source to the receptors you're interested in, and yet the proximity is relatively close, it probably is closer to the upper lefthand group if the distance

is large and the angles where there can be an impact, the wind directions where there can be an impact is a relatively small angle, your point is probably very valid.

MR. FEIN: I would agree with that.

MR. TIKVART: Any other comments?

MR. RHOADS: Would you elaborate on your recommendation for protection of Class 1 areas? You said monitorable increments?

MR. FEIN: Monitorable concentrations. For each class 1 area, there are air quality related values. Visibility is an example where perhaps you find particulates which you need to measure.

As for the visibility that's needed to protect the scenic values of that class 1 area, you might establish a concentration limitation for fine particulate in the atmosphere, and establish it at a level where you could measure it, because you certainly can't calculate with any degree of accuracy.

And then with monitors, you could determine if you were protecting that class 1 area.

MR. RHOADS: I understand.

MR. TIKVART: Any questions or comments from the floor?

MR. SKLAREW: Ralph Sklarew, Form and Substance.

I'd like to disagree with the agreement. If I understood what was just aid, then it goes against how things are used presently. If we have an increment in the PSD area, and it's partly used up by one plant in this whole area, it impacts, let's say, in the northeast, and now we build a plant that's going to impact in the southwest, it still goes against the same increment, what's left, and so the multiple source case turns out everybody uses up the same increment whether they impact at a coincident location or time which is a very, very conservative way of understanding it or again air quality standards where we're going to be exceeding standards.

If my interpretation of how things are working is not correct, I'd like to be enlightened.

MR. FEIN: These are the experts on how things work.

MR. TIKVART: The EPA doesn't have a position that I'm aware of as to how to go about tracking increments. I think that's left pretty much to the discretion of individual states.

MR. RHOADS: That's correct; however, if you're looking at a receptor point, a hypothetical point, and you have a source that is 20 kilometers north of that and a source 20 kilometers south of that, it is unlikely that those two would be additive in consuming increment.

That was not the intent of the increments. They were to -- even though we cannot monitor the values contributing to the increments because they are so small, they are nevertheless intended to represent the real world deterioation, not a hypothetical deterioration.

MR. SKLAREW: But the problemcomes about in application. If we realize that models in a sense can only give frequency distributions, areawide frequency distributions, they can't tell us what time impact is or where it really is, then all the increments in worst case assumptions are additive, and we get ourselves into a tremendously overconservative snaffu.

MR. RHOADS: It doesn't necessarily happen that way, but it could, you are right.

MR. SKALREW: Unfortunately a lot of the things that we've got involved in, it seems like that's the interpretation.

MR. HAYNES: Eldewins Haynes, North Carolina Division of Environmental Management. Could I see the graph that was a comparison between predicted and observed?

Now you made the statement that the -- at least correlation wise, it's obvious that there's pretty bad correlation between the predicted and observed, and I'm personally surprised at the number of

overpredictions in this case. However, if we examine this for regulatory purposes and compare our maximum predicted versus maximum observed, and even the second highest predicted versus the maximum observed, that's pretty close.

MR. FEIN: You're absolutely correct on that. On the other hand, if you were supposing that the maximum observed was too high, and you needed to take some corrective action, this particularly complex source consists of two sets of four sources which are separated in space, and if you want to take corrective action based on conditions for the maximum predicted concentrations you would probably be making the wrong corrective action so that there's a real need here, I think, to have the predictions and the observed coinciding much better than they are, if we're going to use these models for the kinds of decisions that we are forced to take.

This could be done if we dug deeper into the distribution and averaged out some of these errors, I believe.

MR. TIKVART: One last question.

MR. CHASE: Ed Chase, Fairfax County Air Pollution Control, it seems to me that your proposal on monitoring in class 1 areas seems incomplete to me. You established some criteria concentration, and you set

up monitoring for it and then what do you do? Suppose you reach it. What do you then know in terms of what you can do from a control strategy standpoint?

MR. FEIN: Then you have to rely on models to try to take corrective actions and you'll have to consider the various modeling uncertainties in doing that.

MR. TIKVART: Thank you. That was a very informative presentation. Ralph, are you ready? Let's go.

MR. SKLAREW: Thank you, and thank you for the opportunity to speak. It's nice to be back after three years.

First I want to apologize for not having my slides. I left them in my hotel room and had to run back and get them. From this point of view though, Doug Fox's comments are exactly opposite. The podium hasn't moved to the right, it's moved to the left, and maybe some of the things that are happening now are a little bit more to the left.

I'd like to speak about decision making with inaccurate models. Inaccuracies in the standards EPA guideline models have been acknowledged for sometime. EPA sponsorship of this conference demonstrates this recognition of these modeling errors. This conference,

however, seems to be implicitly institutionalizing these inaccuracies. Instead, the modeling community should concentrate on reducing them.

This paper focuses on four concerns about decision making with inaccurate models: the reasons for inaccuracies, the need for quantifying the errors -- actually I don't have a button here, can I have the first slide? I have slides just to illustrate the whole thing. Next slide.

The reasons for inaccuracies, the needs for quantifying the errors, the cost effectiveness in modeling and the use of the models in emergency response situations.

Certaintly there are valid reasons for inherent and irreducible inaccuracies in modeling the atmosphere, but the standard models are far from this level of accuracy.

Modeling improvements rather than acceptance of gross levels of inaccuracy would have been a more appropriate focus. The highest priority in dealing with inaccurate models is to quantify the errors, not for a handful of cases, but for many, most or even all of the major applications.

Error assessment for all major regions of the country, especially urban areas should be conducted routinely. Since the standard models are grossly

inaccurately, cost effectiveness of modeling should be considered. Substantial amounts of money are now being spent on modeling itself, and decisions now based on model results are monumental in their costs.

Emergencies occur in which toxic substances are released into the atmosphere, and the result is serious harm to the public. Accurate models are needed tp respond to these emergencies, to allocate vital resources and to actually save lives.

These four concerns are being presented because they are ones in which we at Form and Substance have been able to make, we feel, significant constributions.

The primary causes of modeling inaccuracies are the dual nature of the atmosphere, deterministic and stochastic, the inconsistencies in the models and the use of models that are inapplicable.

The atmosphere of levels of resolution used in dispersion models has both a deterministic and a stochastic component. The winds are divided into a mean wind causing pollutant transport, and fluctuations about the mean wind causing dispersion. The division is arbitrary and usually ignores both influences that produce variations in the mean wind of things like surface roughness, terrain, vertical wind shear, et cetera, and the organization of some of the assumed stochastic

components, due to thermals, surface roughness, wind shear, terrain, et cetera.

Some are amenable to treatment. For example, considerable progress has been made in the simulation of winds in rough terrain, the effects of change in roughness and remote measurements of the winds aloft.

Slide 3. The ultimate stochastic component will always lead to a minimum irreducible level of error. By their nature =, atmospheric measurements correspond to specific realizations whereas models by their nature correspond at best to ensemble averages over the unmeasurables. These errors must be accepted, but they may not be large. There are certainly vast improvements possible over where we are presently.

A second cause for errors in the standard models is the inconsistencies in the model itself. A Gaussian model, and all the EPA recommended models are Gaussian, assumes a split between mean wind and dispersions. The mean wind is held constant in the model and the dispersive portion grows with downwind distance.

For example, in ths slide I show two puffs. One would be at perhaps one kilometer, one at three kilometers, and they're growing at a different rate. Instead of being Sigma growing at X to the one-half, Sigma grows as a different power of X, and so it's

not proportional downwind distance. This inconsistency results in the Gaussian model producing incorrect results from multiple sources. The model is nonlinear and superposition does not hold.

This problem is most easily seen in modeling two adjacent area sources; using the virtual point source method used, for example, in ISC, two adjacent area sources disperse faster when modeled as one larger source rather than two separate sources. Next slide. There's the S one half compared to the Sigma Y variations.

Another example if provided by a simple gedanken experiment. Next slide. Consider a plume point source at a distance downwind. If the plume's concentration pofile can be replaced by Gaussian source distribution such that the two concentration profiles are equal at this distance where there's a cross-section slice, further downwind the two concentration profiles diverge.

These errors due to the Gaussian model inconsistencies may dominant multiple source and urban model applications, models such as ISC, RAM MPTER.

Another major cause for dispersion model error is the use of models that are inapplicable to the case being modeled. Examples abound. Single wind Gaussian models used in areas of complex terrain, high wind shears of shorelines, single mixing height Gaussian models

in areas of shoreline diffusion. Next slide. This is an example of some of the errors in plume rise. Urban heat island or mountain upslope winds or plume rise formulas when strong stratification, wind shear or downwash is present.

Most real world applications have complexities such as these that limit the standard model's accuracy and if dominant, make the model totally inapplicable. Models have been developed for specific complexities, but more efforts must be encouraged, and their results evaluated and implemented.

Evaluation or validation of models should be the highest priority for contributing to their correct use in decision making. Next slide. Only if the error bounds of a model are known can correct interpretation of the model results be made. Models can and have been evaluated in application to a few intensively monitored cases. However, another complementary approach is possible, routine model validation, using the routine monitoring data taken in numerous regions throughout the nation. Next slide.

Monitoring networks have been or are being established in almost all the major regions of the country, both urban and industrial/mining areas. Next slide. Many or most of these networks are automated with a

central computer for control and data collection. Modern micro and minicomputers performing as central computers have excess capability with which to perform concurrent model calculations, but they're underutilized. Next slide please.

Thus, for over a hundred regions, model testing can be routinely conducted at a small marginal cost. Next slide. Results of such tests can be compiled into error bounds, directions for model improvements and ultimately an accurate region specific model.

We can only guess and plan how well decision making can improve if the accuracy of the models are known for the specific regions and even for the actual worst case conditions. Next slide. Next slide.

And the next slide. The costs of applying inaccurate models include the modeling itself and those associated with the decisions based on model results. Applying models typically involves an in-house modeling specialist or consultant to compile input data, make computer runs, analyze the results and generate reports as well as agency personnel to review the reports, and, perhaps, verify the results by applying models independently.

Modeling specialist charges can run man-weeks to man-year per application. Input data compilation

may require local field monitoring. The computer charges alone can be costly. A recent conference it was reported that the ISC modeling requiring 32 hours on the EPA Univac 1100/82 was conducted routinely, typically an hour of computer time would cost $1000 commercially, so we're talking about $32,000 for routine applications.

Some costs of model based decisions are well known to everyone in the field. The costs, in human terms, were brought home to me in the case of a youth denied the funds to travel to an interstate competition. The sponsoring local businesses were suffering from closing of a major industry in town. It was closed due to the high cost of complying with air quality controls determined by inaccurate dispersion modeling.

Next slide. P rhaps the most demanding application of air dispersion models is in the planning of an adequate response to emergencies such as a toxic release or nuclear venting. Response time is critical, and inaccuracy may mean severe injuries or deaths. Next slide.

A response plan is based on a model. It may be as simple as an evacuation of everyone downwind or as detailed as multi-fluid hydrodynamics and exposure calculations, next slide.

With limited resources available to respond

to an emergency, correct focus of efforts is vital. Evacuation of peoples not endangered may leave others with a critical exposure. Next slide.

The best possible models are needed, and any redicible inaccuracy cannot be tolerated. Next slide. Furthermore, the models must be in a rapid response easy to use package. Next slide.

Next slide. There is a modeling application that is not entering into decision making -- supplemental or intermitten control. In a supplementary control system, emissions are reduced when there is potential for deleterious impacts.

The needed reductions as based on model results are produced by load reduction, fuel switching or engaging effluent controls, for example precipitators.

It is not being used in the United States. In other countries, however, this methodology has proved to be the most cost effective. In fact, we are developing a supplementary control system for a power plant in Melbourne, Australia to control even photochemical ozone.

In keeping with the administration's push toward industrial productivity and cost effectiveness and with its decision making topic of this conference, it is strongly recommended that supplementary control be considered even with the present model inaccuracies.

Next slide. The concerns about decision making with inaccurate models were mentioned, model erros, their causes and measurement, model costs and emergency use. All have been the subject of recent advances in the private sector. These advances should be reviewed and applied in government decision making. Thank you.

MR. TIKVART: Thank you, Ralph. Time is growing short, and there are a number of people who I know wnat to speak this afternoon. What I'd like to do is take about a ten minute break.

I'm sorry. Were there any questions for Ralph? What I'd like to do is take a ten minute break and start promptly at 3:30 with Mr. Witten. Don't wander too far please.

(Whereupon, a brief recess was called.)

MR. TIKVART: I hate to keep juggling the schedule on you, but there are several people who have indicated that they really need to leave this afternoon, so I have six speakers left, and I would propose to take the three who have indicated they need to finish up in the following order, Witten, Kohm and Wright. The other three are Moon, Maxwell and Wurmbrand. Do any of those latter three need to speak this afternoon and leave by a certain time? Do Moon, Maxwell or Wurmbrand need to leave by a certain time? No, okay.

We will proceed with Witten, Kohn and Wright in that order.

The next speaker then is Alan Witten from the Oak Ridge National Laboratory.

MR. WITTEN: I would like to summarize the stochastic air quality analysis effort at Oak Ridge and discuss the implications of this work on dealing with uncertainty in regulatory decision making.

Our work to date involves three aspects -- single source model development, multiple source model development and data analysis.

The scope of this work is, however, expanding to consider economics and health effects. Most of the work was performed as a part of a regional assessment for the Office of Fuels Conversion, Economic Regulatory Administration of the Department of Energy.

The single source model is based on an analytic technique utilizing Chi over Q values from an air quality model in which the ground level concentrations are a linear function of source strength.

The model used for the case presented here is CRSTER. The Chi over Q values were generated by running CRSTER with a unit emission rate. The model requires the specification of a source strength cumulative distribution function either as an analytic

expression or as a function constructed from emissions data. The model assumed perfectly autocorrelated emissions. Autocorrelation data is not typically available so that some assumption is required.

The perfect autocorrelation limit was selected as being most representative of large coal fired power plants operating from a large coal pile and utilizing coal preparation prior to boiler feed.

The case presented here uses a longnormal distribution function with a mean emission rate of 2000 grams per second, and a standard deviation of 600 grams per second. Could I have the first slide please?

This slide shows contours of constant probability of one and only one exceedence of the 24 hour PSD standard for SO2.

You can see contours of ten percent, 20 percent and 30 percent exceedence probability with the highest probability occurring about four kilometers southeast of the source. The source is located at the coordinate origin in the center of the figure. Next slide please.

This shows contours of constant probability of violating the 24 hour PSD standard for SO2. You see five, ten and 15 percent contours. Once again, the highest violation probability occurs approximately four kilometers southeast of the source.

The model also predicts violation -- the probability of a violation anywhere in the region, and for this case, the violation probability was 19.3 percent. The model can also be used to construct emission maps, that is contours of constant violation probability where the violation probability is defined as being anywhere in the model region as a function of emission mean and emission standard deviation.

Could I have the next slide please? This shows such an emission map. The horizontal axis mean emission rate, the vertical axis is standard deviation in the SO2 emission rate, and this tells you the combinations of these two parameters that can produce 5, 10, 15 or 20 percent violation probabilities.

If regulations were modified to allow, for example, upto a 10 percent violation probability, then sources would have a range of parameters in which to operate while being in compliance. This is shown in the next slide.

As in the previous figure, the horizontal axis is mean emission rate. The vertical axis is standard deviation in emission rate. Staying in the white area or the compliance side with these parameters will result in violation probabilities less than 10 percent, so that a source could operate anywhere in the white range

and satisfy this hypothetical regulation. Before going on to the multisource results, I'll briefly present some preliminary data. Next slide please. We looked at SO2 data from a power plant that scrubbed. We have inlet SO2 data which is inlet to the scrubber and outlet data which is the SO2 concentrations coming out of the scrubber.

This is smooth inlet SO2 data. It's smoothed by taking a running 240hour average. The data was collected at 150minute intervals, so it represents a running average of 96 data points.

The data is not -- does not show a great deal of variability. The standard deviation here is approximately ten percent of the mean. Next slide please. Here is a similar smooth plot time series of the outlet SO2, and here you see that the variability has increased greatly as a result of passage through the scrubber.

Standard deviation in this case is approximately 50 percent of the mean. This increase in variability is an expected result. It comes about because of the non-linear behavior of scrubbers. Next slide please.

This shows, the X's on this figure are the actual cumulative distribution function for the inlet SO2, and the solid line is the curve fit to a lognormal distribution, and you see here that a lognormal

distribution in this case is a very good assumption. Next slide.

Here is the same type plot, but now it's curve fitting the outlet SO2 to a lognormal, and the curve fit is not so good. In fact, that would almost defy fitting to an analytic function. Next slide.

This is the autocorrelation in SO2 emissions as a function of lag time for the inlet data. You'll see that it shows perfect autocorrelation. That's the horizontal line at the top going right across at one. Next slide.

The autocorrelation from the outlet data is not -- does not show the perfect correlation; however, the correlation time here is quite large. If you extrapolate, you'll find that the auto correlation time is on the order of 75 days.

That's all the data I have to present. I'll go on now and talk about the multisource model. The model is an extension of the single source method. It utilizes similar assumptions; however, in this case, the individual sources can be any correlation among them can be specified. For the case shown here, the sources, the individual sources were taken to be statistically independent.

The model was used to assess the impacts of

converting power plants in and around the New York City area to coal.

The 24 hour PSD standard for SO2 was used as an indicator of these impacts. Next slide.

That's fairly dim. Can we dim the lights any more? What this shows, whether or not you can see it, is a base map showing Long Island, Manhattan, portions of upstate New York, Connecticut and New Jersey.

You may not be able to read the numbers but you can probably see where they are. They represent the source locations that were included in the study. There are 30 sources. We took all of these 30 plants to be unscrubbed. Next slide.

We used 107 receptor locations, and these are shown in this slide. We used for the single source, we used CRSTER to generate the Chi over Q values, in this case we used RAM. Bear in mind that the region we modeled is certainly beyond the limits of the RAM model, but it's useful in demonstrating the method.

The model can allow for a PSD increment to be independently specified at each receptor. This allows us to calculate violation probabilities at each receptor that represent violation probabilities based on the locally available increment, and that's what we did here. We broke the region up into areas of full increment,

two-thirds of the increment and half of the increment. We ran the model for these 107 receptors, and constructed contours of constant violation probability. We then used that as a tool to identify sensitive areas or areas showing high violation probability for further study. Based on this, we selected 17 receptors for a more detailed analysis.

Next slide. This figure shows the 17 receptors selected. These sets of triple bars. Now in some cases you only see one bar or two bars, but there actually are three bars there. The location of these sets of triple bars show the receptor locations.

The height of the first bar reflects the probability of no exceedences. That's drawn in green if you can make out the colors. The height of the second bar represents the probability of one and only one exceedence. That's in blue, and the height of the third bar which is red reflects the probability of the violation of the 24-hour PSD standard for SO2.

The peripheral bar graphs, what we call source break-out for each receptor, you can see an arrow from each receptor drawn to each bar graph, the height of the bars in the individual graphs represent the relative contribution of the dominant sources of that receptor.

We can use this source break-out as a tool

for identifying sources that contribute to a high violation probability. For example, there's a receptor about in the center, in the center of Long Island that's full red. There's an arrow going to a bar graph, and that shows, I believe its source is 7, 10 and 11 which are contributing to that large violation probability. We can then use this information as a tool for evaluating controls to be imposed on various sources.

Next figure please. This is another set of triple bars. Once again, the locations of these triple bars are at the 17 receptor locations on the previous slide. The first bar represents the violation probabilities as shown on the previous figure.

Now the colors don't show up too well. The way these things read is it's the non-green portion that represents the violation probability, so on the first bar, it's the orange color, the height of the orange color that represents the violation probability, and you see there are a significant number of receptors with the very large orange region.

So based on the source break-out, we selected three plants to scrub. The violation probability with three plants scrubbed is the second bar of the set. Now that's the blue area which isn't very distinguishable from the green, but it turned out that only one receptor,

after scrubbing three plants, there was only one receptor that had a non-zero violation probability, and I'll go point out that receptor.

That receptor, in fact, had 100 percent violation probability. We went back, looked at the source break-out again and selected two more plants to scrub. That gave us a total of five plants scrubbed. The violation probability in that case is the third bar in the set and at every receptor those bars are all green indicating that with the five plants scrubbed, you would achieve zero violation probabiliy at each receptor.

We were trying to get the violation probability below ten percent, and we ended up getting it to zero. The implication of this is that we satisfied a hypothetical ten percent violation probability standard by imposing controls on only five of 30 plants.

Excluding all other considerations then, this would indicate just based on the 24-hour PSD standard for SO2, any additional controls would be excessive. Next slide please.

Okay. This is a repeat of the emission map that I showed before based on a 10 percent violation probability. The shaded area, the area shaded with dots in the lower part of the curve shows the compliance region under the existing regulations. You see that

it's a very narrow region as compared to the 10 percent violation probability, and, in fact, most sources operate in the end of that curve, in the end of that region that's down towards the higher means.

This shows that under a probabilistic standard that there is much greater freedom in how a plant is operated. One way this method could be used in changing regulations is to simply specify something like a 10 percent violation probability, anything upto 10 percent is allowable.

Constructing a fuel map such as this or an emissions map such as this and then allowing the source to operate anywhere it likes in that region, this would tend -- we feel that this would tend to decrease a utility's capital and operating costs by allowing greater flexibility in both plant design and plant operations.

Such a standard would also likely reduce environmental effects. It would do so by encouraging emitters to reduce the mean emission rate in order to take advantage of the large standard deviation that's available for the lower means.

I would also like to address a comment that was raised before when someone made reference to Bernie Steigerwalt's discussion of the ExEx method and criticized it for not accounting for plant reliability or

equipment reliability. This method which is quite similar to the ExEx method, both the ExEx method and this method have the capability of implicitly allowing for component reliability.

If an emissions map such as this is used in regulations then that woudl be the only restriction placed on the source, The designer could factor in any reliability information he would like and then any upset conditions that occur at the plant are just another data point that must fall in that compliance region.

I would like to conclude by briefly stating where we're going from here. We're about to embark on a very ambitious study where we will look at some of the implications of going to a probabilistic, regulatory format.

We will assume something like an allowable 10 percent violation probability, look at how a plant would be designed under existing regulations, and then also look at modifications to the plant design that would be allowed under a hypothetical 10 percent limit.

We will investigate the economics of these other scenarios to see if they will, indeed, provide any cost savings to industry and utilities. We'll also look at the other side of the picture and look at the environmental effects. We will be constructing a

hypothetical city using demographic data. For each scenario identified, we will be superimposing contours of constant violation or exceedence probability. We will be providing the health scientists any data that they would like to see that they can use in making their assessment such as specific information at particular points, for example, probability of exceeding a certain value, probability of exceeding a certain value three consecutive days in a year, et cetera.

This will be done for a single source. Then we'll look at how a probabilistic regulation will affect development in an area, and we'll go through the same kind of analysis trying to develop our hypothetical city under the existing regs and under a hypothetical probabilistic reg, and see what the differences are, and that's all I have to say.

MR. TIKVART: Thank you. Do you have a paper that's available, or do you have copies of your slides?

MR. WITTEN: I've given copies of the slides with the reporter. I'll leave him my notes for what they're worth, and then I'll also say, the single source method has been published. It appeared in the July journal of the air pollution control association. That, as far as I know, is the most recent. It's undergoing internal review now, and will probably be submitted

probably somewhere in the next month.

MR. TIKVART: Is Mr. Kohm still here? Do you turn into a pumpkin in five minutes? Okay, can we take some questions first? Any questions or comments of Mr. Witten? No?

Thank you very much. The next speaker is Robert Kohm representing Alcoa.

MR. KOHM: My name is Robert Kohm, and I'm the manager of Environmental Planning and Analysis of the Aluminum Company of America, Pittsburgh, 15219. Under agreement with the chairman, I'm not going to read my prepared results. You can all cheer now.

The my have been appropriate first thing this morning, but they're a little bit esoteric for this time of the day. What I will do is just summarize a little bit what, in fact, was in there.

The first point that I wanted to make was the fact that really when we looked at the dispersion models, I don't think that there's anybody in this room who would be willing to stand up now and say that they believe the dispersion models are accurate all the time.

I'd like to see who that would be, you know, waste time trying to convince you at this point that dispersion models accuracy is subject to debate, but I also throw out a question that if everybody can put

their biases aside and forget about any particular ego trips they have to suffer because they're a model developer, just think in terms of what is the likelihood of making any quantum leaps towards modeling accuracy.

I think the probability of making a quantum leap toward the improvement of modeling accuracy is quite small, so what we have to do is face reality, in fact, that although the system may not be perfect, we can improve upon models incrementally. We're not going to tackle in any meaningful way the discrepancies and agreement with data we've seen presented here today and yesterday.

So with that in mind, I think that realistically Alcoa's position is that we need to move rapidly towards a probabilistic compliance approach. It seems to be that EPA's approach has been one of if it can't be done exactly, we're not going to do it at all, because we've caught enough flack in the past for doing things premature.

We're not going to catch it this time, so we're going to let the status quo fly. There are some differences. The ExEx model is a refreshing change in view from the standpoint that the only way you can meaningfully interpret those results in through a probabilistic compliance attitude, but there's another more

pressing problem, and that is that even if we were to today have the authority to make the changes, to say we're going to move towards a probabilistic compliance approach, there would be a great deal of consternation by state agencies and people who have to interpret the modeling results because it's a new area for them. It's an area that's beyond their level of expertise, and true understanding of dispersion modeling results requires literally a graduate degree in statistics, and there probably aren't very many around, so when you get to that point, the people who have to interpret those results are going to be dealing with things that are not in their bailiwick and that's going to cause some more problems.

So what we'd like to recommend is that EPA make some administrative changes directly and expeditiously to allow for probablistic compliance arguments in the dispersion modeling field, and really not make any direct recommendations as to thou shalt do it this way as has taken place in the past and let people take a creative scientific approach to the interpretation of data as exhibited to some degree by the last speaker and evaluate what comes in and through that experience develop an understanding of what people can come up with.

You can sit in ivory towers and try to formulate the best life, but regardless of what happens, somebody's going to come up with a better way that we haven't heard up. So I'm just saying open the door and see what the cats drag in, and review that with a realistic viewpoint.

Consider it honestly and whether or not it makes sense. At this point, I'm willing to answer any questions you might have.

MR. HELMS: Does the issue of consistency bother you, going that approach?

MR. WITTEN: Oh, the issue of consistency bothers me immensely, and what bothers me is that we try to be consistent. Again, I'll take anybody in this room who feels that we should take the models as currently defined by the guidelines and apply them universally everywhere that you're going to get the same level of accuracy.

I don't think there's anybody here that believes that, so the issue of consistency is just really a way of presenting your own, you know, it's easing the administrative burden, and one way to ease the administrative burden is to stop trying to enforce your way of doing things and listen to what is a meaningful way of interpreting the data and whether it makes sense. Open mindedness is a virtue.

MR. RHOADS: It doesn't bother you from a -- I'll rephrase it. Does it cause you great concern to feel that if you went into North Dakota, you would enter a different regulatory climate, different models and different interpretation of those models than if, for example, you went into South Carolina?

MR. WITTEN: We have that today from region to region. Don't we? I mean, that's the status today. We're just accepting that fact rather than trying to gloss over it.

MR. TIKVART: Any questions from the floor? Okay. Thank you very much. The next speaker is Ray Wright speaking for the utilities air regulatory group.

MR. WRIGHT: Good afternoon. My name is Ray Wright. I'm Director of Environmental Affairs of the Ohio Power Company, a subsidiary of the American Electric Power Corporation, and I am speaking today on behalf of the Uility Air Regulatory Group commonly referred to as UARG.

UARG is an ad hoc organization made up of 88 electric utilities, the Edison Electric Institute and the National Rural Electric Association. UARG's members produce most of this country's electricity.

UARG is pleased to attend the Second EPA Air Quality Modeling Conference. I will outline UARG's

views on the use of atmospheric models and the Airlie House recommendations. We will submit more complete written comments on these issues and others raised during the Conference before the close of the docket.

We support the Agency's efforts to re-examine the proper role of modeling in regulatory programs as exemplified by the Airlie House Workshop and this Conference; however, we are concerned with the increasing rigidity with which models are being used in air regulatory management programs.

Rather than being used to provide information to decision makers over the past several years, untested models have been pushed beyond their limits. They have been used to impose emission limits on the basis of single value rare events, even though most experts concede they perform most poorly when predicting those events.

In many instances, modeling decisions have caused or have threatened to cause serious economic consequences with no prior investigation of the accuracy of the model.

Many times monitored data has been set aside in favor of modeled estimates with emission limits being based more and more on hypothesis in an observation. In the past models have been part of a decision making

process which has failed to give adequate consideration to the uncertainties associated with model predictions.

UARG believes that realism must be the central goal in all modeling efforts. Realism is the goal, the inclusion of the uncertainty information and a regulatory process will help focus on this goal.

We concure with efforts such as the EPRI Plume Model Validation Project, the Woods Hole Workshop and AMS-EPA Cooperative Agreement which indicated that model performance needs to be further evaluated and that model uncertainty should be quanitified and reported routinely as part of the decision making process.

Also, we endorse generally the efforts and many of the recommendations from the Airlie House Workshop, and we hope that many of the Airlie House recommendations will be elaborated upon and ultimately be reflected in EPA's air quality management program.

Now, as to UARG's specific recommendations, we support the Airlie House recommendation or recommendations that model selection should be a flexible and cooperative process with participation by all interested parties.

That process should insure the use of the most realistic model in all instances. With realism as the goal, all models used in regulatory programs

should be tested against available data to establish levels of skill and performance. No model should be recommended by EPA, thereby establishing it as the model of choice until it has been evaluated.

The skill of the evaluated, recommended model should establish the minimum acceptable performance level for other models of that class. If no tested model exists, any theoretically sound computational correct model should be acceptable.

Apparently from yesterday's presentations, EPA has begun evaluation of some models. The information presented yesterday morning is the type that should be made public for peer review and discussion prior to the use in a regulatory context.

We stress that this review process must be open to the public with full disclosure and ample time for comments.

For instance, the CRSTR evaluation reviewed yesterday for the Muskingum and Stuart Stations offers an example of the need for careful public participation in modeling evaluations.

Although we have not had time to review and prepare comments on the conclusions presented, we are concerned that EPA's analysis improperly averaged predictions and observations. The evaluation results

were reported for the 99th percentile, not the statistically less stable but normally used highest second high concentration which is currently used to set emission limitations.

The other results referred to as demonstrating the skill of the CRSTR, RAM and other models misleads one to believe that these models are accurate to within 10 to 40 percent. These data should be open to public scrutiny and peer review before they are accepted for use in regulation.

UARG will submit additional comments on these data. I would like to request today a copy of those studies and data that were cited in yesterday's presentation. I could give you this afterwards. It's in the footnote.

In order to minimize disputes in technical modeling, UARG endorses modeling protocols developed in open and cooperative meetings prior to the modeling exercise. The protocol should be detailed and should bind the regulatory body to the same extent that it binds the applicant.

Reasonable yet firm deadlines should be set for both parties. UARG recommends briefing the responsible decision maker on the measured technical recommendations contained in the protocol and on major areas

of dispute which arose during this negotiation. Discussion of these key technical points prior to the modeling exercise would afford the decision maker a greater understanding of the uncertainties involved in the specific areas.

There is a need for some technical review group composed of agency and non-agency modeling experts to review numerous types of mod ling issues. Such a group should be available to render technical advise on disputes which arise in the development of modeling protocols and in the selection of models for recommended status.

Such a group could oversee many of the functions suggested in the Airlie House Summary for a modeling center. If such a group is established, EPA must assure that it is adequately staffed and funded so that it does not become an impediment to modeling analysis.

UARG recommends that the modeling community should seek to reduce the sensitivity of regulatory decisions to all uncertainties by identifying and quantifying the uncertainties.

Currently the perspective of the decision maker is narrowed by the fact that reliance is placed on a single prediction out of the multiplicity of

of available model outputs. The use of one extreme value based on freak, sometimes never observed, conditions often adds to the arbitrariness of decisions.

UARG recommends that uncertainties be made explicit thorugh all available means, in all modeling related decisions. It is important to develop more effective methods for conveying modeling uncertainty in a meaningful way.

Model sensitivity analysis and Monte Carlo simulations may produce useful technical data on the short-term. Field evaluation will provide more meaningful information in the more distant future. The use of the protocol briefings to inform the decision maker of the uncertainties at an early point may also be useful.

UARG expressed strong support for continuing the efforts started with the ExEx methods designed to incorporate probabilistic concepts into the modeling framework in place of unrealistic worst case modeling approaches.

In the interim, we urge the use of alternate model statistics in a decision making process. UARG will submit additional comments concerning the replacement of the highest,, second highest concentration estimate by other statistical measures of the type

recommended by the Airlie House workshop. Use of such alternate statistics might serve immediately to reduce the impact of model uncertainty on final decisions.

UARG feels that additional flexibility is needed in PSD decision making. Since PSD air quality increments are not strictly health or welfare related, the decision maker should not be constrained along a rigid pathway.

Generally, air quality management decisions should reflect the uncertainties in models, the regulatory goal to be protected and the burdens imposed on the regulated industry.

The decision making process should encourage thorough consideration of available monitored data especially where modeling results are highly uncertain or only poorly reflecting reality.

Since monitoring data are in fact reality, they can be used both as a check on model performance and to temper the interpretation of available model output.

Because realism should be the primary goal of air quality modeling, improved and more realistic procedures must be incorporated into the Air Quality Modeling Guidelines.

Since this will necessitate frequent revisions,

UARG recommends that the Guildeline be updated at least every 18 months. Under this schedule, every second revision would coincide with the triennial EPA Modeling Conference. However, as major changes in modeling guidance occur, such as the incorporation of newly recommended models, the Guideline should also be updated.

In all cases, proper comment and review procedures must be followed. As a matter of policy, however, advances in modeling techniques should not be the cause for continual review of established SIPs or PSD permits. As the Airlie House participants noted, emission limits based on past model demonstrations performed in good faith should be grandfathered.

Even if past modeling results are drawn into question through improved models, the inequities which would result from a reanalysis of the existing emission limits counsel strongly for grandfathering. Only when monitoring establishes that a primary standard is being violated should mandatory reanalysis be permitted.

In conclusions, while models are a necessary elements in our current air quality management programs, they must be applied with their inexact nature in mind.

I stated previously that UARG will be forwarding additional comments in more detail.

MR. TIKVART: Two remarks. One is most of

the report referred to and Bill Cox's presentation are already available as contractor reports. I say that with minor qualification. There may be one of those reports that have not been released yet, and I will have to check that once I see your list, but the majority of those reports with one exception are already available as contractor reports.

MR. WRIGHT: Yeah, I think one that was footnoted number 4 was still in draft form.

MR. TIKVART: Okay. That's probably the Teknekron report. That's the one I'm referring to, so all but one are alreadl available.

The second point, with regard to the 99th percentile, we also wanted to show the second highest, but because these are reports that are already completed, the individual values are lost, and we could only use the values that were available to use from the report.

MR. WRIGHT: Well, I think that was the original Mills work that might have been done in '75.

MR. TIKVART: That's correct.

MR. WRIGHT: And as to the Muskingum situation, there were four monitors. One was background. Three were in-line downwind, and it showed overprediction close in and underprediction far out, and about 10 to 40 percent.

MR. TIKVART: Whatever the figures were were reported.

Any other questions or comments? Any from the floor? Okay, thank you, Ray. I'm sorry. Val?

MR. DESCAMPS: Val Descamps, Region I. What frequency were you urging for the guideline update?

MR. WRIGHT: Well, we're not urging any frequency at this time. I only pointed out that if we're going to use like a 99 percentile to evaluate a model, that doesn't seem to be quite squaring with the fact that we use second max's to regulate. There should be some consistency to make sure evaluation does reflect the regulatory process.

MR. TIKVART: Thank you. The next speak on the list would be Don Moon. Don, if you care to make your presentation now, go ahead.

MR. MOON: I'd better go while I still have somebody left out there. Good afternoon. My name is Donald W. Moon. I'm supervisor of Salt River Project's Air Quality Division. Salt River Project, a political subdivision of the State of Arizona, provides electric power and energy to more than 300,000 residential, industrial, commercial and agricultural consumers in and around Phoenix and other portions of central Arizona.

SRP is a member of the Utility Air Regulatory

Group and endorses the major conclusions given by them at this conference. We're also a contributing member of the Electric Power Research Institute and fully support their plume model validation efforts.

SRP is the operating agent for the Navajo Generating Station, a 2250 megawatt coal-fired generating station located near Page, Arizona and the Coronado Gneerating Station near St. Johns, Arizona, comprised of two operating 350 megawatt units, and an additional 350 megawatt unit under construction.

We are also part owner of the Mohave, Four Corners, Craig and Hayden coal-fired generation stations. Needless to say, we are vitally concerned about the appropriateness and the accuracy of atmospheric dispersion modeling as it affects our operation and the uility bills of our consumers.

Today I am going to address only one aspect of the problem -- the incorporation of model uncertainty in regulatory decision making.

First, I believe we must recognize that uncertainties exist to a degree that serious problems are being encountered routinely in applying model results in the regulatory arena. It's clear from discussion presented in this conference and in the scientific literature that such uncertainties do exist. The

regulatory decirion makers need to fully acknowledge that existence and effect on siting and abatement decisions.

Secondly, we must recognize that modeling uncertainties are not going to vanish or decrease in magnitude appreciably in the foreseeable future. The record clearly shows that improvements in modeling accuracy have been painstakingly slow. Regulatory decision makers need to recognize that the solution to the on-going uncertainty problems will not come through pie in the sky basic model improvements.

Thirdly, and perhaps most importantly, we must recognize that many of the regulatory problems and uncertainties that are posed can be solved by regulatory or statutory change. Regulators needs to look at alternative solutions to the dilemma generated by the use of modeling.

They need to examine those standards for which modeling is virtually the only technique available by design to derive related emission limits, and where possible use more straighforward empirical approaches.

We must remember that in nearly all cases the use of modeling versus some other analysis technique or combination of techniques is an administration decision.

In examining the incorporation of model uncertainty in regulatory decision making, we must not simply resolve ourselves to living with such uncertainties, but rather take positive regulatory and administrative steps to minimize or eliminate such uncertainties.

Let's examine the Federal statute concerning the use of modeling. The Clean Air Act requires that analysis be performed so as to assure that certain pollutant sources do not cause or contribute to air pollution in excess of National Ambient Air Quality Standards, Prevention of Significant Deterioration ambient increments, visibility impairment criteria and air quality related values.

The Act implies, but does not specify, that stmospheric dispersion modeling will be the principal tool for such analysis. Such use is as the discretion of the Administrator.

Section 165(a)(2) concerning preconstruction requirements limits issuance of permits to those facilities for which, quote, "the required analysis has been concuted with regulations promulgated by the Administrator," unquote. Thus, if the Administrator chooses modeling as the only means of analysis, regardless of suitability or reliability, that section becomes law.

Section 165(a)(3)(d) also concerning

preconstruction requirements states that the administrator, quote, "shall specify with reasonable particularity each air quality model or models to be used under specified sets of conditions," unquote. Thus, modeling is implied to be preferred analytic tool and although suitability of models to specified sets of conditions is addressed to some extent, the issue of reliability or accuracy is not.

Section 169(a)(3)(b) concerning EPA's report to Congress on visibility impairment requires recommendations for, quote, "modeling techniques (or other methods) for determining the extent to which man-made pollution may reasonable be anticipated to cause or contribute to such impairment," No mention is made of suitability, reliability, accuracy requirements expected or desired.

Section 171(2) concerning nonattainment areas states, "The term 'nonattainment area' means, for any air pollutant an area which is shown by monitored data or which is calculated by air quality modeling (or other methods determined by the Administrator to be reliable) to exceed any national ambient air quality standard for such pollutant."

Thus modeling is inferred to be reliable, and all other methods must be determined to be reliable before they can be used.

Section 320 provides guidance to the Administrator on the conduct of air quality modeling conferences such as this with special attention to be given to appropriate modeling necessary for carrying out part C of title I relating to prevention of significant deterioration of air quality.

No guidance is given concerning the suitability, reliability or accuracy requirements expected or desired. Further, no guidance is given concerning their use if uncertainties are found to be unmanageable.

It is clear then that analysis, not necessarily modeling, is needed to set emission limits so as to assure that pollutant sources do not cause or contribute to air pollution in excess of National Ambient Air Quality Standards, PSD ambient increments, visibility impairment and air quality related values.

It is also clear that considerable latitude is given the administrator in establishing how that analysis might be accomplished, or even if separate analysis beyond that accomplished in the setting of new source performance standards is necessary.

Let me repeat that. Or even if separate analysis beyond that accomplished in the setting of NSPS is necessary.

I'd like to address some specific areas of

concern where modeling uncertainties pose serious problems in the regulatory decision process and suggest some possible administrative or regulatory solutions.

Perhaps the greatest modeling uncertainty is encountered in the use of non site specific and unverified models in complex terrain and seashore applications. The degree of uncertainty has never been quantified to everybody's satisfaction which by itself leads to serious conflict.

It's been our experience that uncertainties of upto a factor of ten may be encountered usually biased towards overprediction. Such uncertainty creates havoc for planners when tolerances are narrow as for PSD permitting and potential costs of abatement are out of sight.

We have no overall solution to that problem, but would like to offer the following suggestions to decision makers.

One, recognize that modeling results are only fir approximations. Two, examine all other analysis results including those using tracer and comparative measure techniques. Use representative ambient measurements wherever possible, particularly where multiple units are involved, multiple follow-on units, that is, and, four, verify model estimates with on-site

ambient measurements and modify source emission limits accordingly.

Uncertainty is encountered also through the use of multiple worst case input parameters. To exacerbate things, the probability that concurrent worst case events might occur is usually ignored by the modeler. To eliminate that uncertainty, we suggest that all inputs be expressed in probabilistic terms, as in the expected exceedance methodology.

All inputs, not just fuel sulphur content was described by Dr. Steigerwalt yesterday. Such a requirement would necessitate that all models be modified to accept inputs in probabilistic input, a rather small cost considering the potential costs involved.

The results, of course, would also appear in probabilistic terms, which would be far more amenable to standard statistical treatment than present outputs. Conversion of inputs to probabilistic terms should be done regardless of whether standards are ever restated in probabilistic terms.

Another modeling uncertainty is encountered in the modeling of shorter term concentrations as compared to the modeling of longer term concentrations. The uncertainty is undoubtedly greater in complex terrain than if appropriate peak to mean ratios were applied.

We would suggest that where short term, that is 24 hours or less, dispersion estimates cannot be demonstrated to be reasonably suitable, reliable and accurate, that is at least within a factor of two, that only the annual average ambient concentration estimates be used for emission setting purposes.

If shorter term comparisons need to be made for other purposes, we would recommend the use of annual to shorter term ratios as derived from actual mean measured values in similar terrain circumstances.

Such an approach would not only help minimize the uncertainties involved, but would also considerably streamline the permitting process.

The use of the standard steady state Gaussian modeling techniques has been extended in time and space beyond reasonable bounds forcing ever-increasing uncertainties in the process. This is basically the problem in attempting to estimate shorter term concentrations.

It is also certainly true in attempting estimates beyond the 50 kilometer range from a source. We suggest that no steady state technique be attempted beyond 50 kilometers. Only dynamic technique should be used.

We recognize that statutory change beyond the scope of the Administrator in this conference may

be necessary to provide additional and sufficient relief from modeling uncertainties.

This is true specifically of the PSD increment system where the basic problem of atmospheric loading from distant sources is not necessarily reflected in ambient concentrations which we are attempting to model.

It is also true to some extent of the dispersion module of visibility modeling attempts, of long distant transport associated with possible acid precipitation and of stack height modeling attempts.

In summary, the Administrator has many avenues open within the guidance given in the Clean Air Act to reduce modeling uncertainties and associated permitting conflicts and costs.

She should not try to live with the uncertainties but rather take positive steps to minimize or eliminate those causes of uncertainty within her jurisdiction. There is need to eliminate shorter time period modeling, to eliminate the use of multiple worst case input parameters, to limit the spatial extent of steady state modeling, to use measured ambiet values and extrapolation techniques wherever possible, and, in general, to not place such absolute reliance on dispersion modeling results as has been done in the past.

We thank you for the opportunity to participate

in this conference. If there are any questions, I'd be pleased to replay.

MR. TIKVART: Any questions from the floor? No? Thank you, Don. Next I have Mitchell Wurmbrand representing Western Energy Company. I hope I've pronounced your name not too badly on several occasions.

MR. WURMBRAND: Good afternoon. My name is Mitchell Wurmbrand. I am employed by TRC Environmental Consultants of Weathersfield, Connecticut. I am a certified consulting meteorologist, and part of my work involves assessing the accuracy of air quality dispersion models.

I am speaking today on behalf of Western Energy Company of Billings, Montana. Western Energy Company is the owner and operator of the Rosebud Mine near Coal Strip, Montana, and Western Energy Company's ability to secure necessary permits to operate is strongly influenced by the accuracy of existing air quality dispersion models.

Western Energy Company appreciates the opportunity to address this conference. Today, I will talk about two deficiencies in the existing air quality models and in the way the models are applied by decision makers.

These two deficiencies introduce a systematic

error in the prediction of total suspended particulate matter downwind of open pit and area surface mines, such that TSP concentrations computed by the models are significantly larger than those that actually occur.

The consequence of these deficiencies is that some surface mines that should otherwise be granted air quality permits may instead be denied permits, and that overly conservative modeling studies may predict total consumption of PSD increment when, in fact, some PSD increment remains.

While the consequences of the modeling deficiencies are most often felt at the state level, particularly in states whose enabling legislation requires that fugitive dust consumes PSD increment, the remedy can be effected most easily by EPA in its congressionally mandated role of fostering greater standardization in accuracy and modeling.

The first deficiency is the absence of a deposition algorithm in the type of dispersion model that is frequently used to simulate surface mines. Every meteorologist familiar with the intricacies of modeling surface mines knows that correct simulation of deposition of particulate matter is essential to computing realistic concentrations, yet surprisingly there are some western states in which the regulatory

agency responsible for granting permits routinely simulates the air quality impact of a mine by using models that totall ignore deposition.

The use of these simplistic models introduces a systematic error, because concentrations predicted by a model that ignores deposition will always be larger than a model that properly simulates deposition.

Let me give you an idea of the magnitude of this systematic error. An EPA publication titled survey of fugitive dust from coal mines presents a curve of source depletion factor versus downwind distance for a windspeed of five meters per second and a particle settling velocity of five centimeters per second.

This combination of wind speed and settling velocity is one that is representative of conditions at a typical surface coal mine. At one kilometer from the source under neutral stability conditions the value of the source depletion factor is about four tenths.

Assuming that the source depletion factor correctly describes particle settling behavior, this EPA curve says that a model which totally neglects deposition will compute particulate concentrations two and a half times larger than actually occur.

Of course, we all know that an aravis magnitude is not uncommon in simulating atmospheric dispersion.

But, we generally tolerate errors of a factor of two only when we do not know what causes them. The consequence of ignoring deposition is that the modeling simulations overestimate particulate concentrations, and if feed simulations are used as a basis of checking whether a given surface mine will comply with ambient air standards, then it is conceivable that a model be wrongly denied a permit.

In regions of the west where several surface mines are clustered together or in states whose regulations allow fugitive dust to consume PSD increment, the consequence of ignoring deposition in modeling simulations is that the available PSD increment for the total suspended particulate may be erroneously consumed.

While there are some who will argue that deliberate neglect of particle deposition is desirable because it makes the regulatory analysis more conservative, I would point out that there is already a great deal of conservatism built into the emission factors, models and the ambient air standards.

If more conservatism is desired, then it should be added knowledgeably, not haphazardly. The failure to properly account for deposition is not a fault of the available models. There are a host of

models that simulate particle and aerosol deposition, and I would refer conference participants to Dr. Ray Hosker's excellent paper titled, "Practical Application of Air Pollutant Deposition Models: Current Status, Data Requirements and Research Needs."

Furthermore, EPA has funded numerous deposition model development projects, one of which culminated in the industrial source complex model, now part of the unimap series.

The failure to properly include the effects of particle deposition is actually a failure to achieve consistency in applying available models. To remedy this situation, I ask that EPA require that regulatory agencies empowered with the authority to review surface mines, stimulate particle deposition in a manner consistent with the existing technology. This requirement can be enacted through the state implementation plan and attendant delegation of authority.

The second deficiency in the existing modeling review of surface mines concerns the manner in which the available models simulate transport of particulate matter from inside the pit to the mine boundary. Many different fugitive dust producing operations occur inside the pit, including overburden removal to some extent, coal drilling and blasting and coal removal.

Depending upon the mine configuration, there may be a large portion of the mine's total haul road emissions generated in the pit as well. In the western states, these pits are sometimes as deep as 800 feet, although I would guess that an average depth is more likely to be 200 feet.

It is intuitively obvious, and recent measurements support this observation that only a fraction of the fugitive dust generated within the pit escapes to the mine boundaries. Yet, when it comes time to model a surface mine, the regulators generally employ a flat terrain model that assumes that pit emissions occur at the same height as the surrounding terrain.

More complex models capable of handling area sources, such as ISC, do not adequately simulate the mine either, because they cannot accommodate emission heights that are below the receptors. In fact, in the ISC code, if the source is below the receptor, the ISC model prints an error message and automatically terminates execution.

The emission factors associated with surface mining activities result from measurements taken inside the pit, generally very close to the pit activity in questions. So what we have here is a situation where a surface mine is modeled as if its pit emissions occur

at ground level, and these emissions are then transported to the mine boundary along a horizontal plume centerline, when, in reality, the pit emissions occur well below grade, and only a fraction of this dust escapes from the pit.

The phrase that is used to describe this phenomenon is pit dust retention, and I suggest that currently used dispersion models do not adequately address pit dust retention. The failure to simulate this phenomenon in existing models causes the models to predict ambient particulate concentrations higher than those that actually occur. As with the previous problem I just discussed, this deficiency could also prevent the permitting of some mines or could prompt unnecessary control measures in order to demonstrate on paper that a mine meets standards.

I ask that the TPA as the driving agency for research methods and air quality and meteorological modeling address the issue of pit dust retention. Additionally the decision makers and the regulatory process and the scientific community involved with modeling should be made aware of this modeling deficiency. Thank you.

MR. TIKVART: Any questions from the floor? Thank you very much. Is David Maxwell still here?

Do you wish to give a presentation now? Okay.

MR. MAXWELL: I appreciate the opportunity to be last, but I'm going to follow up briefly on what Mitch just said. It's fortunate that the last two dealt with mining, because I was surprised that in the entire two-day session very little of it concerned mining, and that is a big problem out west.

MR. TIKVART: Can you state your name and affiliation?

MR. MAXWELL: Yes, I'm about to do that. I was about to say I'm Dave Maxwell. I'm an air resource coordinator with ARCO Coal Company in Denver, and I've had experience in state and federal government including EPA Region 8, consulting companies, and now industry. I just want to start out by saying that the National Research Council in its recent study entitled, "Controlling Airborne Particles" concludes that improving modeling capabilities will require as a first step, expanding emission inventories to include the size distribution and chemical composition of conventional ducted emissions, fugitive process emissions, anthropogenic and natural fugitive dusts and other natural sources.

Currently, the most dependable and accurate models are those which estimate local air quality impacts of a single point source, such as from a smokestack.

Estimation of air quality impacts from mining operations, however, is much more complex for several reasons. First of all, mining operations are composed of multiple individual sources, such as area sources, such as emissions from conveyor belts, storage piles, and loadout areas among other things. Secondly, mining operations are often located in complex terrain as they are in the western United States, surface and underground mining, and this reduces the reliability of modeling results.

So this reliability is compounded not only by the different sources but also by the terrain. Thirdly, the mining operations have high variable particulate emissions over time due to the nature of individual operations and because of variations in meteorological conditions, and, fourthly, the fugitive emissions originating deep within strip mines or open pits do not get totally emitted out of the pits, as Mitch said.

The in-plume particle removal rates or plume depletion for low level fugitive sources is probably much greater than current dispersion models allow. The combination of source complexity, variable emission output and high dependence upon variable meteorological conditions together with conservatism in modeling predictions will make mining operations prone to predicted

violations of PSD increments as well as national ambient air quality standards. Furtheimore, the wide variation in the use of emission factors from such studies as PEDCo and EPA studies contain a significant degree of uncertaintly for model emissions input.

Now we may ask what are mining companies doing to resolve model uncertainties. Well, ARCO among other companies has sponsored a fugitive emissions dust study which has taken about three years to complete, and it has involved up in northeast Wyoming which is the center for mining activity in the western United States for surface mining, and it's taken three years to do a detailed and rather laborious study to determine the effects of fugitive emissions on the impact of mining operations, and the results, even though they're not printed yet, they will be published soon, indicate preliminarily that the fall-out or deposition rate is much shorter, a much shorter distance than what has been given credit for in any models, and among other things, it includes a look at sedimentation rates and also in addition to the deposition, it looks at the variable meteorology conditions that tests were made in, so I would recommend that as part of this record, if I can, submit to you a copy of this study that was performed for a consortium of energy companies

in the western United States if we can get it before the September deadline that you mentioned.

We really think that this is a way where industry is trying to look at the uncertainties in modeling of emissions from mines, and particularly surface mines, and we hope that not only the federal agencies will look upon this, but the state agencies as well, because they're the ones in the west that will have to work with the federal government, and some states in the western United States have PSD authority to make the decisions themselves.

So we feel that this is one way that the mining industry has been taking a good look at the impact of their own mining emissions on the impacting of the environment in the western United States, and I may say that the mines in the western United States are pretty far away from any major metropolitant areas also.

Only a few of the available dispersion models have the ability to vary fugitive emissions with wind speed, and none have the ability to explicitly conserve the effect of precipitation on emissions. This variation could be significant since precipitation can reduce emission to virtually zero.

Although several theoretical and empirical

techniques simulate gravitational fall-out and turbulent deposition, none of the settling and deposition techniques have been adequately validated by field studies, and we realize this is a very difficult process, and we're working on that, so we're not trying to look at the regulatory agency as being hard on the mining industry. We're just saying, let's look at all the facts that we have, and together let's evaluate some of these models that have been previously developed and also that may be developed in the future.

So we want to work with the regulatory agencies in developing the best way to determine an impact on the mining operation on the environment. I want to conclude that although significant process has recently bee made in the development of models which can predict regional impacts of sources of sulphate and other fine particles, similar process has not been made toward the development of regional models to estimate regional air quality impacts from inhalable particulate sources, and this is where the breakdown comes that we discussed yesterday.

We're looking at a certain breakdown, so the mining industry feels that if you're going to have a particulate standard, why not look at the health effects, namely the ones that deal with inhalable

particulates, 15 microns or less or whatever standard is set.

Further study of sedimentation rates of various particles by size and composition is needed so that atmospheric residence times may be more realistically incorporated into dispersion models.

The ISC model which was mentioned, it's the only dispersion model that addresses deposition, but does accurately simulate the dispersion of fugitive dust in mines.

The dispersion model that would incorporate factors unique to the mining industry would eliminate much of the guesswork in the modeling of mines and thus enable regulatory agencies to more accurately determine the impacts of the mine on PSD increments and the national ambient air quality standards.

So I would conclude that we've started something. We've gotten the regulatory agencies to work with the mining industry, and we need to work a little harder. We need to deal with model uncertainties, work with the mining industry, the regulatory agencies, and any research and consulting companies and studies that need to be developed to more accurately determine what effects dispersion modeling has on the impacts of emissions from mines, and out in the western United

States fugitive dust is the big issue out there, particularly in the less populated states, like Wyoming, Utah and Colorado where mining is becoming a more evident source of energy development, and an important environmental issue out in those states.

Thank you for your attention, and I'm willing to address any questions.

MR. RHOADS: You mentioned that ARCO had been participating in a rather large study of fugitive emissions for the last three years. Have any of the regulatory agencies been participating with ARCO on that study?

MR. MAXWELL: To my knowledge, the study I'm referring to was basically a study that was done just by the mining industry; however, any time something like this is done, they would like, once they get the results and the internal agreement among all the companies, they would like the comments from the state and the federal agencies on that.

At this point, that study is not available for publication, but hopefully will be very soon. I'm not the one to say when it will be, but we would like to send a copy to the federal and state regulatory agencies involv d.

MR. DICKE: I would just like to comment

that while certainly we would like to have a model which accomplishes a number of these depositions, sedimentation, wash-out, rain-out processes, no model that was essentially -- no model has been submitted to EPA for review in the context of the solicitation of models last year, so none of the models that we have received so far from private developers have accomplished or incorporated these mechanisms which you're suggesting.

So we would be just as happy to receive one for review as you would, I'm sure.

MR. MAXWELL: Right, and I think one thing that has to be looked at, I think mining operations can certainly improve their environmental programs simply by even watering their haul roads on a regular basis, and ARCO has done some studies on that that indicate that watering haul roads maybe twice an hour depending on climatic conditions is just about as good a control method as using chemical dust suppressants on a more frequent basis.

So I just think a simple conclusion like that would be enough to maybe help in the development of an environmental program for a mining operation, but maybe in giving a little more credit as far as the modeling goes of the particular impact that the mine would have.

I believe that most of the modeling that involves fugitive dust really doesn't look too much into the control methods involved, so I think that we have to work with the regulatory agencies on giving you information upon what we determine as good control measures on mining operations to keep down dust.

A simple thing like watering haul road, EPA used to give 50 percent credit for. Our studies indicate higher than that, so I think that we can maybe work on this together, not only ARCO but other mines to work on a better way of improving overall emission factors.

MR. TIKVART: Any questions from the floor, observations?

MR. HENDERSON: I'm Donald Henderson from the National Park Service, and I wanted to ask Dave if the mining industry has decreased any of the uncertainties in the emissions for mining operations sufficient that it would warrant applying models that have deposition in scavenging.

MR. MAXWELL: At this point, that's a difficult question to answer. I can't speak for the other mines. I know that one of the things that the dust studies that they've been looking at and some of the work on the fugitive emissions, the mining consoritum has been

looking at has involved some of this work. I'm afraid it's going to take quite a while, just like it has in model development to actually come up to EPA or the state agency and say look, we've got better data thatn you have, so, first of all, we're in the process of gathering the data, publishing it, and then submitting it for comment.

When you get a consortum of companies, each one is going to have different pet peeves, and we've found that out in this case, and the difficulty is to get everybody to agree.

Just like in any group of people, you're going to get people who are more in favor of something than another, so the first step was to get everybody on the industry side to get together and agree on a particular format and the way things were done as far as the methodology.

Now we're in a stage of publishing it, and now we're going to have to submit it for peer review, so I can't say right now that that'll be any startling revelations or not, but at least it's a start that a series of industries have gotten together and tried to improve the situation and make it easier for, I think, everybody to evaluate the impact of fugitive emissions in a mine.

MR. HAMBURG: Pardon me if I can't help saying, but there seems to be a meeting of mines.

Anyway, I would just like to make a suggestion. I've been away from this deposition business for many years, but in reviewing some -- sorry, Fred Hamburg, RMC, et cetera.

In reviewing some of the current literature on models that include deposition terms, I find that they're extremely primitive compared to models that were put out about 20 years ago in an entirely different area, namely fall-out prediction.

We kind of got out of that, because the nuclear money ran out with the end of the Nevada testing, but the approach there, I think, should be reviewed. Much of that literature had been confidential. They were never into the secret category, but they were classified as confidential, and they are surely available in the open literature today.

I think you will see some explicit terms used that could be incorporated into prediction models today. I doubt very much, however, if the gaussian approach is going to work with these. These were all numerical type of puff type models, and it's the only way they can get it to work.

One paper that I remember because I had

occasion to write a criticism of it back in the days of the Journal of Meteorology before it became the Journal of Atmospheric Sciences, was a paper on the D model put out by Al Anderson of Navy Radiological Defense Lab.

He had incorporated as much as he could in the open literature of some of the concepts that were being used in deposition type models, and I would just suggest that you might want to look into those things.

MR. MAXWELL: Right. That's a start. You know, we have a long way to go, and I think it's up to industry to work with the regulatory people and even consultants as a third party for peer review not only with visibility. I think visibility and the mining models are just about in the same boat, except visibility, I think, is ahead, because they've had some models that have been printed up, and that has as least been printed.

I don't think anything has been printed as an official document and guidelines for the mining, but I think we have to work together, because it's a very important issue, and mining emissions are related to visibility, particularly in the western United States, so I look forward to working with a lot of different people on this, because it's such a wide open project.

It's going to become a very important issue, and I thank you.

MR. TIKVART: Bill?

MR. BONTA: Bill Bonta from the Maryland Air Management Administration. I was a little bit surprised to hear, Jim, that you said you didn't get any inputs. Actually although we didn't submit our exponentially tilted plume model that we have developed specifically for use in fugitive dust situations and have used a number of times on coal export terminal operations, similar, but not exactly the same, it wasn't submitted to you in a formal fashion, but it was submitted, and I think you remember that.

We did show that with the glass bead experiments that were used to validate the ISC particle settling subroutine that, in fact, this model of ours seemed to produce slightly better correlation with the measured field data than did the ISC's tilted plume.

What's more, the exponentially tilted plume does show that in far range deposition results which we don't have too much data in the glass bead experiments to begin with, and especially with the heavy particles that there is a very much greater deposition velocities and far lower concentrations.

But that -- you can have that, we can give

you that paper any time you want it, but one other comment is on these emission factors.

We find in evaluating these terminals going into Baltimore that just one operation, a hundred ton coal car dumpers, rotary car dumps, dumps it out flat on the ground, we get estimates of emission factors that range over three orders of magnitude, just for that one operation alone, from looking through the literature, and I don't see how you can stand around here, and everybody says just routinely that Tikvart's models are inaccurate and need to be improved when you look at the garbage in, what are you going to get on the other side.

MR. TIKVART: Okay, thank you very much. That completes the list of speakers we have for this afternoon. Was there anybody that wanted to speak that didn't get the opportunity to do so?

Any further questions or observations? As announced in the Federal Register, the conference will continue tomorrow. We know of at least one speaker that wants to make a statement tomorrow morning. If there are no further comments, we'll close for now to be opened tomorrow morning at 9 a.m. Thank you for your attention and contributions.

(The meeting adjourned at 4:54 p.m.)

CERTIFICATE OF REPORTER

I hereby certify that the foregoing transcript represents the full and complete proceedings of the 3-11-81 aforementioned matter, as reported and reduced to typewriting under my direct supervision.

Neal R. Gross

NEAL R. GROSS

WEDNESDAY AU 12

GOVERNMENT OF THE UNITED STATES

ENVIRONMENTAL PROTECTION AGENCY

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2nd CONFERENCE ON AIR QUALITY

MODELING

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Wednesday, August 12, 1981

The Conference was held in the Thomas Jefferson Auditorium, South Agriculture Building, 14th Street & Independence Avenue, S. W., Washington, D. C., Joseph Tikvart, Chief, Source Receptor Analysis Branch, Conference Chairman, Presiding.

PRESENT:

Joseph Tikvart

Richard Rhoads

James Dicke

MR. TIKVART: Good morning. I'd like to welcome you back to the third day of the Modeling Conference. This morning we do have two scheduled speakers, so I'd like to begin with that right away and perhaps we'll have some informal discussion after those two speakers or others that wish to volunteer are done.

The first speaker, speaking for the National Academy of Sciences, is Myron Uman.

MR. UMAN: Thank you Joe. I speak for the Academy only when I describe results of the Academy studies. Anything else I say is for myself.

My remarks in fact, are based on the work performed by the Academy's recent study of the implementation of the PSD program as published in the report titled "On Prevention of Significant Deterioration of Air Quality."

The report contains a detailed analysis of the existing PSD provisions and, of special interest to this Conference, a discussion of air quality modeling for use in PSD decision making.

For the record, I submit excerpts from the Academy report on the questions of accuracy and comparability of models and of a statistical approach for managing air quality.

The report points out that there are likely

always to be uncertainties in the results of air quality modeling for specific events, such as the highest or second highest concentration, because it is impractical to expect we shall be able to develop the definitive deterministic model, one that is manageable and affordable and also accounts for every detail of the dynamic meteorology in any terrain.

We shall, of course, try. But decision makers will, in all likelihood, have to use a predictive tool of limited capability. It behooves policy makers to take account of the limitations when they define the objectives and rules of the decision process.

In the technical community, the classical methods for dealing uncertainties incorporate the principles of statistics and treat outcomes probabilistically. Hence, the Academy's PSD report suggests that modeling would be a more effective tool for PSD purposes, if the short-term increments had more robust statistical criteria for compliance than one permitted violation per year.

The Academy also believes the same conclusion can be reached about the NAAQS. The draft recommendations of the American Meteorological Society, presented earlier by Mr. Fox, echo the Academy's conclusion. The work in EPA on expected exceedances is in this spirit but does not address the problem of uncertainties in modeling.

While the Committee did not go farther than this, because its charge was limited, I would like personally to suggest that EPA take another tack in approaching the problem of statistical analysis applied to air quality management, an approach that is more in keeping with the Academy's conclusions, the recommendations of the World Health Organization, and, I believe, those of the AMS.

The analytical approach described by Mr. Steigerwald does not deal with the uncertainties inherent in atmospheric modeling, but only with time variations in emissions. Thus, modeling for expected number of annual violations of one, even over a period of ten years, is still looking for rare events, way out on the tail of frequency distribution.

The "exex" concept as incorporated in the ozone standard also raises the specter of having to regulate the frequency distribution of emissions, as Mr. Steigerwald suggested.

I think EPA would be on firmer ground if it concentrated, at least for now, on the problem of uncertainty in atmospheric modeling. The data being presented at this Conference, an elsewhere, strongly suggests that it is much easier to validate models for frequency distributions of concentrations than models for worst-case

events. The NAS report, the AMS draft, and a WHO position paper all argue for a statistically robust measure of compliance which in regulatory terms would be expressed as an ambient concentration that could, on the average, be exceeded a specified number of times per year, preferably more than once.

So, based on effects data and understanding of the stochastic nature of atmospheric processes, for example, a 24-hour concentration could be selected that could be exceeded 18 days per year without significant risks to air quality.

When I use the term "significant" in this context, I mean statistically significant. The concentrations of pollutants representing the levels of acceptable rist would be chosen on the basis of effects data. Other, lower concentrations that could be exceeded, say, five per cent of the intervals per year, would be determined based on the statistical characteristics of atmospheric and meteorological phenomena.

This idea, of course, is not new. The value of moving towards statistically robust measures of compliance has been pointed out by many individuals and groups. I hope there is some way this Conference can convey this message to those whose responsibility it is to develop the rules within which the modeling community

operates. Thank you.

MR. TIKVART: No questions from the panel. Any questions from the floor? Comments? Okay, thank you.

The second speaker we have this morning is Richard Kerch from the National Coal Association.

MR. KERCH: My name is Richard Kerch with Consolidation Coal Company and I'm here today representing the National Coal Association. Keep this very brief and let us all get back out into the rain.

I'd like to take the opportunity to make some brief, extemporaneous comments this morning, primarily to alert the Hearing Panel that NCA will be providing more extensive written comments to the record after we've had a chance to reflect on some of the things we've heard today, and get our thoughts together as an organization in responding to the theme of this Conference.

We find that this whole process a very laudable effort by EPA and we want to let them know that we support the notion of formally quantifying the various uncertainties associated with modeling and finding how these uncertainties can be incorporated into policy and regulatory decision making.

We would note, however, that the modeling community which can probably be relied on to quantify uncertainties, perhaps is not the proper group to rely on

wholly in integrating uncertainties into decision making. Perhaps EPA needs to broaden their audience in this area in soliciting advice.

The second point I'd like to make today is more specific to the coal industry. I apologize for not hearing the last two speakers yesterday afternoon, and I may be covering some ground that was covered yesterday afternoon. Most of the emphasis on, if not all the emphasis, in any modeling conference that I've ever been to has been on the bouyant, elevated release from point sources.

In the coal mining industry, we have the unique situation of having non-bouyant plumes released at, or near ground level from area sources. We think that the fact that very little effort has, by comparison, gone into developing appropriate models to treat the emissions from surface coal mining is, in fact, a source of uncertainty.

I'd like to point out three areas that, in particular, that we have some concern: One, as an industry, we're not satisfied with the treatment of deposition at the ISC model gives; we think that there would be more realistic ways to handle that.

Secondly, we think that there probably are better methods of handling the non-buoyant, ground-level releases from area sources. I heard some information

given today, I forget by which, or during this conference, I forget by which speaker, that related to the fact that if you took an area source and calculated the concentrations and then you divided the area source in two pieces, and modeled it, you got a different result. That doesn't sound right to me.

The third area that we would try and create some attention to is the problem of emissions which are generated below ground level, typically in a Western surface coal mine, we have pits which are anywhere between 100 feet to 400+ feet beneath the surface. A good fraction of the total emissions coming from that surface mine are generated in that pit and we know that they all don't get out. But today's models that are available to us pretend like the earth is flat and all of these fugitive emissions are accounted for in the model.

We would urge EPA to place some of their resources in addressing these areas.

The third broad comment I would like to make. A group from NCA environmental committee met informally together last night. We'd like to inform the panel that we plan to take up the challenge of doing some creative thinking that was thrown out at the audience by Dennis Trout yesterday and, really, which is the underlying theme of this Conference, to attempt to address some of the

"how-tos" rather than the "shoulds". Or, if we are not very successful in coming up with some ideas on the "how-tos", we plan to take a couple of steps backward and look at the whole issue from a little more broad focus and see if there might be some alternatives that might allow us to finesse the whole question.

The last comment I'd like to make today is primarily a personal opinion and not so much representing the National Coal Association. We heard some information presented at this Conference. I know Mr. Tikvart made a presentation to one of the Senate or House Subcommittees on modeling recently, Mr. Rhoads sent out to the participants a statement on the accuracy of models, and as a scientist, I take little comfort from the fact that models such as the CRSTER model which has been identified to be accurate $\pm$ 10-40% when it is comparing distributions of high concentrations to distributions of observed concentrations cannot predict the spatial and temporal locations of those high concentrations and does a significantly worse job at doing that.

I think while the highs estimated and the highs predicted some place around the source match, that gaives me no real comfort that we know what's going on in the model. You're being essentially right for the wrong reasons, and I think that in the PSD process, it's even

more scary that we don't know where the high is located or when the high is located.

For a remote source, that may not be a problem. But, when you start to bring in the second source or multiple sources, I think it's important if we're going to have any chance of keeping track of increment, and not placing undue control requirements on the subsequent sources coming into an area, that models do better than just predict what the high, or second high, or distribution is in the immediate area.

We've got to know what the location of that high is; we need to know when the time is.

That concludes my remarks today.

MR. TIKVART: First an observation and then a question. Regarding your last point, I don't think we know for sure that it's the models that cause the problems with time and location. It may well be our knowledge of what the atmosphere is doing. I suspect it's a contribution of both. But I don't think the results that have been presented indicate that the models cannot accurately estimate the concentrations in time and space, but that certainly the data bases are insufficient to describe what the atmosphere is doing.

Now, unless you can describe what the atmosphere is doing, the models can't estimate concentrations

correctly. But I think that there's contributions from both in that and, perhaps, another contribution is our knowledge of what the emissions are.

Okay. The question. You suggested that we should bring in a broader group of people to look at the inclusion of uncertainty in decision making rather than just modelers. Could you expand on that? What other sort of people should become involved in this and, perhaps, in what form?

MR. KERCH: Okay. Let me respond to your observation first while I'm trying to think of an answer to your question.

I think I used the term modeling semantically different in context than, perhaps, you do. I look in the way I used it then, I used the word modeling to mean the whole process of starting with some data of some sort and coming out with some computer sheets at the end. And, in that sense, modeling doesn't give me the comfort.

Whether you have the right, actual algorithm in there, and I think that's what you meant when you used the word modeling, I was not prepared to argue that with you.

MR. TIKVART: Okay, I think semantics problem is that many technical people don't appreciate. In other words, when they say modeling, they think of the models

right away and forget about the data base problems.

MR. KERCH: With respect to your question, I think you got me. I guess my comment is a reaction against placing all of this burden on the modeling community. I think that it can provide some service in terms of identifying the technical questions.

But when you begin to ask a technical body to deal with non-technical issues, then I'm not as comfortable with the results that are likely to come out of that exercise.

Whom I would suggest to be contacted, I think I'll further think on that, and include that in our written comments to you.

MR. TIKVART: Okay, because as a technical person, I share your concern. We have to get the right people to ask the right questions, and we can't, as technical people, tell the decision makers what questions they should ask us.

We would like to provide them with the information they think they need, so, if you give us some further input on that, I think that would be good.

MR. KERCH: Thank you very much.

MR. TIKVART: Any questions or observations from the audience?

Okay, thank you, Dick. At this time, is there

anyone else who would like to make a presentation, ask a questions, or ask a observation? Name an affiliation, please.

MR. HELLUMS: Lloyd Hellums, Phillips Petroleum Company. Yesterday, EPA and the other Governmental agencies issued a request requiring outside organizations or the public in general to make contributions in order to help out with the question of uncertianty in modeling.

May I address that this morning?

MR. TIKVART: Yes, sir, you may. Would you care to use the lectern?

MR. HELLUMS: It doesn't make any difference to me.

MR. TIKVART: Why don't you come up?

MR. HELLUMS: May I break this problem of uncertainty into three areas. The first area I'd like to mention is source inputs. These are emission-type of variations and characteristics of emissions and the uncertainty involved in the emissions whether it be a petroleum plant like we in the oil companies understand or whether they be a coal mining operation, there are always uncertainties in the emissions and the type of emissions being involved. This is the first point I would like to mention.

The second area of uncertainty is in the

meteorological variables themselves, because there is a certain amount of stochastic processes involved in the atmosphere yet, however, I think that without question, patterns and certain types of trends can be shown to hold in meteorlogical conditions, as some of our meterological friends yesterday have pointed out.

So that the thing is not completely without patterns and discernible type of phenomena going on that can be characterized to some degree.

The third area of uncertainty is in the model itself. Now, there are many types of models. You can have the numerical partial differential equation models in 3-D, three-dimensional space, or you may have a Gaussian Plume model which is a derivation of the partial differential equation models under certain assumptions, and is an algebraic type of model.

You may have many different variations of the Gaussian Plume model itself.

So let me back up. Let's first start, if you want to start pinning down the uncertainties, you've got to break it down and use some control and separate out these variables. And, the best way I know of doing this, is to start with the Gaussian Plume model since it is the recognized equation being used presently by EPA, and which includes, of course, the Briggs' Plume Rise formulation

in with that and to take a look at that under very controlled type of situations using the best data that we have available.

The best data we have available, at this point in time, is that which has been collected over the past 30, 40, 50--I don't know how far this goes back because I first started taking a look at these questions in '77, so I'm not a meteorologist, I'm not, my background is not in this area, my background is in complex modeling and non-linear optimization.

However, it is a mathematical problem and whether you're modeling the atmosphere, atmospheric problems, or whether you're modeling problems in physical sciences as in container reactor vessels or other type thing, the mathematical formulation and then resolving of the solution to determine an optimal best set of that model to the data is still the same basic mathematical problem.

So, the problem I want to tackle with you this morning is a fundamental problem using a control data base and today, that data base the best we have is Prairie Grass, Hanford, Ringo, Cross-through, Ocean Breeze.

API, American Petroleum Institute, has collected about 17, in fact, I think 17 exact is the number, data bases in which the best information that is available

today, in the area of modeling, has been assimilated into a computer-library type format and we are now proceeding to analyze this data base.

In this particular data base, you do have the meteorological variables defined as probably as best as any base to date. There are still some areas of unreliability even in that data base in the fact that wind direction does vary, and the actual wind direction or effective wind direction, if you're looking at tracer releases where you have arcs then, out, proceeding away from the source of release, your effective wind direction at a given arc will vary according to what has happened in the process from the time of release to the point where particular pollutant rates that particular arc.

So, this is an unknown within the system and it must by itself be determined in the optimal analysis. Now, I want to define now what I mean by optimal analysis. I want to determine, since the Gaussian Plume equation has coefficients, dispersion coefficients for one, I want to eliminate the Briggs' Plume-rise formulation that has some more coefficients which must be determined, so if we go to the data base I'm talking about, there all released at some particular elevation, either ground, or above ground, so we can eliminate the Briggs' Plume rise equations from our formulation, and we can look strictly

at the basic, fundamental equation of the Gaussian Plume formulation.

And the major coefficients within that are Sigma y's and Sigma z's. They are a function of distance and, also, stability.

Let's make a mathematical formulation, a least squares mathematical formulation in which we can optimate, determine the coefficients that will best fit a Gaussian Plume formula to the data base, to a given test.

Now, I want to bring up a point here. In the past, when people have calculated from the basic data Prairie Grass, Hanford, you can go down the list, to the best of my knowledge, and I must say that because my knowledge is limited as far as the literature in this field, to the best of my knowledge, what they have done is they've calculated Sigma y's in a directform way and then they've gone and calculated the crosswind integrate concentrations or CWIC's, if you'll allow me to use the abbreviation, and when they do this, they separate them.

They break apart the Sigma y and the Sigma z and say it's independent because that's what they assume in the way that they make the analysis, the mathematical analysis. If you will look at the Gaussian Plume formulation, you can mathematically prove that it is impossible to break apart the Sigma y and the Sigma z determination

independently of each other like this. It's a very simple little proof, you take your least squares formulation and then simply by taking the derivatives and setting them equal to zero, you can come up with what is in the optimal solution for fitting a Gaussian Plume formulation to a given data set and you can very quickly ascertain that the problem is inseparable for a Sigma y and Sigma z.

Therefore, Sigma y and Sigma z in order to determine coefficients for the Gaussian Plume formulation which will optimally match the data must be done simultaneously.

The problem is further complicated in that Sigma y's and Sigma z's must be formulated in such a manner that they're a function of downwind distance.

Okay, this is our basic problem. About a little over a year ago, in studying what had been done and looking at the problem, I realized that the only way we were ever going to come up with a really accurate determination of what the uncertainties in Gaussian Plume model is, is this particular exercise would have to be done. It's a rather complex, constrained, least square optimization problem. So, I proceeded to, I'm on a API task force, AQ7 non-reactive pollutant task force, so I looked at this and the questions came up to me about separability and there was a lot of question about it, so

I said give me a little time and the next meeting I came back and I had produced a mathematical proof form showing that the Sigma y's and Sigma z's are not separable and that, in fact, if you wanted to determine Sigma y's and Sigma z's which will optimally, in a best-fit manner, represent the data, it must be done, they can not be calculated as has been done in the past, but must be calculated using different type of procedures more involved mathematical techniques.

And, off the top of our head, it looks like a fairly simple little problem, three or four weeks, I can set up a mathematical analysis package to do that, Well, one year later, the problem was much more difficult than I had anticipated. It was a very, very, nasty--when actually I got into it -- I found it was a very, very, nasty, curving ridge, back surface in space.

A very difficult problem to solve in the optimization field which I have been working in ever since 1959. And, therefore, I found out that, also it had to be a constrained optimization. It took me a year before I finally resolved all the technical difficulties and the numerical to resolve and solve this problem.

But, they have been solved. At present, within Phillips, we have a air quality state of technology budget. The management at Phillips has been very receptive to the

proposition of trying to do some fundamental work to help out and to looking into the technical problems in the air quality field. And, with my making some presentations to management, they have agreed to devote some money, some computer, which it requires a lot of computer capability to make these types of analysis. We do them on a 3033. The programs are very large. To just get one little case solved which may be just Hanford, for instance. In Hanford, we have 73 cases within that data set.

To just analyze one of those cases takes about five minutes, however, that is five different analyses within that. The program is now being set up and we're starting to run. I just got through Hanford, the first 35 cases which is a zinc sulfide pollutant emissions, it took 262 minutes of CPU time.

But we are going to process that entire data base and what is coming out of it is a rigorous mathematical analysis in order to determine what the exact optimal coefficients Sigma y's Sigma z should be to give an optimal fit to that data.

This will then allow us to tell you exactly how good a plume formulation which is only one of many models will actually fit the data, because these are optimal solutions.

In other words, this is the best solution that

the Gaussian plume formulation can get through that particular data base, and as you can see, we got hundreds and hundreds and hundreds of data bases that we're looking at individually in this analysis.

And it is an optimal fit. You're not going to to be able to do better in this fit. Now you can go to a different model and you can make a better fit. But, with a Gaussian Plume, the standard that we're using, the RAM, the basic RAM model is what we're presently using in the analysis, which is, by the way, the same as is, we just got finished doing a comparison of ISC short of RAM and of MPTER which we have in-house, on a data case which involved a petroleum refinery, and if you use the same technology that's common to all of them, for instance, RAM does not have the Huber Downwash algorithm and it does not have elevation possibilities, if you eliminate that and compare them on equal comparison, all three give the same result, which is what you would expect if you have the same basic formulation and the same plume rise equations.

If they're all the same in three algorithms and they're run and in a case in which the capabilities are all the same, you would expect the same result, and that's exactly what turns out. They are equivalent to within a per cent or less, in some cases.

Okay, let me go back then. So what we're coming up with is an optimal capability on what the Gaussian Plume model is capable of doing. This will then tell us, and I can tell you right now that there are cases we run into in which they are not Gaussian -- the data is not Gaussian in nature. It is bineural.

I mean by this the concentrations will go up, they'll dip and then, they'll go up again and you'll have two maxima across the arc. What the optimal procedure does, it puts a least squres optimal fit for all the data points through that entire distribution.

What it does, it does an optimal fitting of a Gaussian Plume mathematical characteristics to that type of displacement. So it is a best fit and a best capability of the Gaussian Plume to represent that type of variation.

What I would like to do is, since Phillips Petroleum Co. and API is going to extensive effort in order to rigorously, mathematically, attack a problem which has never been solved as far as I know, to the best of my knowledge there has never been made concerning this large data base collected over the past 50, I don't know how many years it goes back, this data base, there has never been a rigorous, mathematical analysis made to determine what are the parameters of the model you are

applying essentially to determine an optimal (inaudible) to represent that data base.

That is being done. Right now, my plans are to finish that. I told Phillips management that I will have that through by the end of this year. The output is in microfiche because the output is massive, it is detailed analysis and also what the program does is, after it determines the optimal fit, these dispersions Sigma y and Sigma z coefficients as a function of downwind distance, it then goes back and for each of the arc, it breaks out equivalent stabilities.

That is very enlightening. Let me give you some brief results of these types of analysis. First, you will find out is that Sigma y's have a different stability than Sigma z's. There are different stabilities.

You will find that sometimes they will switch, going from arc to arc, stabilities. Not more than usually one clash, but there will be a change even in one clash in stabilities. These are very basic, fundamental, type of analyses which is throwing some tremendous insight back into what is going on for definitions meteorologically.

Now, there is been a lot of concern about does terrastability or your wind fluctuations, your sigma thetas, these other type. What are the best characteri-

of stability? This type of analysis is throwing some direct insight into this.

So, this output is on microfiche. To give you an example of the volume of microfiche, on the 35 cases I ran of the Hanford (inaudible), it produced 24 microfiche frames, I don't know how many pages in a frame now, but that's a tremendous amount of output.

I got the output by paperback also, and it was two boxes full and that's less than half of the Hanford system. And I have 10 of those data bases. Of the 17 we have at API, there's only 14 amenable to this analysis because some of them are puff release and some of them are line releases and we're only interested in point source releases.

So we're going through in a detailed analysis of this data base and the output's coming back. Now, you can do the analysis, it's being put out in output form and a very compact microfiche form, but still the job of analyzing because people still have to go through.

They have to know the trends, they have to know the patterns and all of these things. It takes people to do this. It is a time-consuming job to go back and analyze and get insight into the fundamental studies what is saying. And you cannot do this statistically.

You cannot do it because still, the best

pattern recognition is not statistical procedures, but the human mind.

Now, the thing about it is, the mind can do things like integrating as you go through all type of variables, so we need help, and we'd like to see EPA join in this effort. If EPA is really interested in finding out what is the uncertainty of models, we're making an attempt to do that.

But there is a lot of work involved -- people involved. Just to go through and to analyze this data and ascertain the patterns, the trends, and come up then with the final recommendation on what is the ability of the Gaussian Plume formulation to represent meteorological data.

Now, this is coming out. It's going to come, I would love to see it come out. Phillips Petroleum Co. individual, I cannot speak for API, they have to -- let me speak for Phillips and I have not discussed this with my mates, but from what they've already indicated to me, they are perfectly happy to cooperate with EPA in any way in this basic data analysis in a cooperative effort, not in an antagonistic sense at all.

Because I have heard, as I listen to people talk, I sensed there was some antagonism in this meeting. That's not our feeling. My feeling is one of mutual

cooperation. If we have a problem, let's do it in such a manner that we jointly resolve it and use the best technical capabilities we have available.

EPA has experts. Industry has people who have technical capabilities that can be utilized. We can jointly approach the problem in a cooperative deal to come up as the Environmental Act has stated with a better and the most accurate model that we can produce in order to evaluate the environmental issues and come up with solutions acceptable to both the public at large and the industrial concerns, and if at all possible, we hope, in a cost effective manner.

I asked my management this question. I said which would you rather have: would you rather have not knowing the truth and have over-conservative models, or would you rather have not knowing the truth and having models that actually would allow us off easy, or permitting, because we're constantly permitting and getting applications for processes.

Here's the answer they came back with. We would like the most representative, accurate information we can get to make our decisions on for a very simple reason. If we put in a plant in which the model has not correctly predicted, and let's say for instance, and these are their remarks, that the model has actually said that

the concentrations were a lot more than they actually in practice turned out to be after monitoring that work to be put in.

It's very expensive going in, then, and correcting after the design is finished. The most economical place to correct design information is in the design stage. Once you've built that plan and you have to go back in and find out through monitoring networks around your plant that it is out of regs, then it is very costly to make modifications at that point.

The cheapest place to make modifications is in the design stage. So, management at Phillips is interested in having as accurate models and meeting a real-world condition. If we have to put the equipment in to meet what the public wants, we'll do it, but we do not want to put in equipment based on models which are inaccurate which cost us money and in turn that cost is being passed on to the public and the products and services that they use and unnecessarily charging them.

So, this is an issue I want to bring up before EPA and the other Governmental concerns, Departments that are concerned here, to throw this open to a cooperative venture. The output's coming out. Is EPA really interested in seeing what is the actual limitations of the Gaussian Plume model and what are the best capabilities

that we can put as far as determining paramemters in that model that we can really best optimally match.

Now, we still have not, after we have done this, we still have the whole problem of meteorological variation. And it's there. All we've done in this particular step, is taken a very rigorous analysis of the efficiencies of a Gaussian Plume model formulation.

Now, another thing this analysis is doing, by the way, is looking not only downind distance, but crosswind distance as Sigma y and Sigma z, I beg your pardon. Sigma z is only a function of downwind distance, but on the Sigma y, we're looking at it as a function not only of downwind distance, but also crosswind distances.

We found out that within the data that there are cases many times, the sooner you can get a much, much better fit to the data when you include crosswind distance. In general, I say that's true.

I'm open to questions.

MR. TIKVART: Okay, thank you very much. I can assure you we are very interested in tracking what you're doing. I've seen the interim report on the 17 sets of tracer studies and we looked at that in preparing the review that Mr. Cox presented here on Monday. So we are very interested in tracking the work that you're doing. Thank you. Norm?

MR. BOWNE: Norman Bowne, TRC, I have a question for Mr. Hellums and their fit. Are you using the source term for the Hanford?

MR. HELLUMS: We are using the data that Dick has -- TRC has put together IP0 data base for us and they are maintain it and the information that I have was gathered and sent by Dick and it does have the source information in there as far as emissions. Is that what you're referring to, Norm?

MR. BOWNE: Is the emissions part of your solution? Do you have to have the emissions to get your Sigma z?

MR. HELLUMS: Oh, yes. You must have the emissions and you must have the meteorological data that was associated with that data set as measured in the field.

These wind directions are measured. For instance, for Hanford, they have releases at 26 meters, at 56 meters, 111 meters. They have wind speeds measured at that height.

MR. BOWNE: I would suggest, then, that there's a third variable that needs to be accounted for here, especially in the particulate tracer tests because it appears to us, and in our report to you, that there might be significant deposition affecting these and this would make considerable difference in the sigma z that you

calculate and the Sigmz z that you might infer from the data if you have not accounted for that deposition.

MR. HELLUMS: We've looked at that, Norm. Dick did initially mention that. I looked at it in detail. In fact, at the last meeting we had, I gave a result of our month analysis where I had looked in my spare time.

This is a secondary project. This is not my main effort. The state of the technology is not my main effort. This is a side effort for me when I do my other functions within Phillips.

No, we looked at this in detail. As a gravitational settling is, I think we can mathematically prove according to the ISC type of information, it is not a factor. And, in fact, the analysis then that we're making sure that is also it is not a factor. There is the settling velocity and the friction at ground surface.

And, of course the friction goes (inaudible). The ultimatizor is also determining of these reflectors. There is some difficulty and in doing this and it takes certain types of information, certain types of data sets in order to do it because of the variations withing the data itself and because also of other types of things.

This is in about six pages of analysis that I gave, handed out, at the last API task force meeting and if you check with Dick, Norm, you can see some very

detailed information concerning this. Right now, I think I have proved fairly conclusively to my own satisfaction, that gravitational settling is not a problem within these data sets because settling volocities are so small, that they literally make no difference and that the main factor moving the particular particles down to the ground is not gravitational settling, but it is the eddy currents within the air.

MR. BOWNE: I'll look forward to looking at that, thank you.

MR. HELLUMS: Okay, thank you.

MR. TIKVART: Thank you very much. I'd like to encourage you to think about two things. Dick Kerch mentioned that they're going to, National Coal Association is going to go back and look some more at the should versus the how to.

As I mentioned yesterday, we've heard an awful lot of shoulds. We should do this, we should do that. Decision makers should consider these things, etc. But, we're talking about things that haven't been done systematically, and quantitatively in the past, so we would really encourage you to think about this and to submit written comments on this if no one has any further observations to make here, today, on it.

How do you go about expressing quantitative

--quantitatively-- the accuracy of models, and given that quantitative expression of their accuracy, how is that information used by decision makers to come to reasonable determinations relative to the emission limits and locations of new and existing plants?

That's one point. The second point is we had some discussion yesterday of what the process should be of submitting a permit for a new source or a siprevision for an existing source when one has a better technique as the gentleman just now indicated he was working on.

What is the process by which this better technique should be submitted to the control agencies -- whether it's EPA or a State or local government? And, how should the agency and the source, the industry involved come to a, again, a reasoned approach to determining whether that new technique is acceptable for this particular application.

What is that process, and we've heard that it's a good idea to conduct such a process but not much information on the mechanics of that process.

At this time, are there any other comments on those issues, or any other issues? Does anybody else wish to make a statement?

MR. MOE: Rod Moe with the State Department of Highways and Public Transportation, Austin, Texas. I just

want to reiterate something that was said yesterday afternoon. I feel that there are too many models and not enough data to support them.

And I think there has been too much piecemealing of research on model validation studies. I think that some types of sources haven't been validated at all, as far as trying to validate models, like the mines and things like that that have been discussed here.

We've done some work in line source modeling to try to establish data bases and once you've established the data bases then to try to validate them. I'd like to suggest that EPA have a national research effort to fund some of their research in a program to study how they can assemble these data bases, existing data bases, and how they can come up with new data bases to amplify on these things.

I think that quite often there is no adequate data base for validating models. None, for quite a few types of sources. I think that the measurements have been incomplete and I think that the monitoring has been poorly dome.

I think quite a bit of the original data has been lost and I don't think the data has been acquired that should have been acquired. And I think that instead of piecemealing the effort, there should be a coordinated

effort, a joint effort, and there could be quite a lot of contribution from the private sector in this regard.

Then, I think there should be national sites set up for continuing monitored studies. And I think that methodologies should be up-dated and revised and new instruments brought in, things like this.

Then, with a good data base, if somebody comes up with a new modeling idea, then you can test it against this data base, not against a data base that that fellow comes up with some sort of inadequate monitoring network to try to do this, but with a really complete network to test that data, that particular model, and to establish uncertainty and to apply a standard set of statistical tests across the board.

That's my suggestion, any comments on this?

MR. TIKVART: Thank you, anybody else?

MR. FEIN: My name is Richard Fein, I'm a Senior Research Associate with Texaco at Beacon, New York. And I would like to add to your list of things that we all ought to think about.

I'd like to add the point that was made by myself and a number of others yesterday. That is, that we look very carefully at the air quality management purposes of each of the applications of modeling, and then see if there isn't some better way to achieve these

ambient air quality purposes than using modeling the way we've presently used it.

And, I think this would be an important thing to do for everybody in trying to put together their final comments for this Conference. Thank you.

MR. TIKVART: If there are no other questions or comments, I would like to remind you that if you wish an individual copy of the transcript of the proceedings of this Conference, you should contact the court reporter directly, Mr. Miles Anderson.

The transcript of this meeting will be put in the Docket A-80-46. I would guess that it would appear in there some time early in September. The record will remain open until September 14 for written comments. Those comments should be submitted directly to the Docket, so that by September 14, there should be a complete record in the Docket of all written and oral comments made pertinent to this Conference on Air Quality Modeling.

If there are no other comments or statements, I would like to thank you for your attention and participation in this Conference and draw the 2nd Conference on Air Quality Modeling to a close. Thank you.

(Whereupon the meeting adjourned at 9:50 a.m.)

CERTIFICATE OF REPORTER

I hereby certify that the foregoing transcript represents the full and complete proceedings of the 8-12-81 aforementioned matter, as reported and reduced to typewriting under my direct supervision.

Neal R. Gross

NEAL R. GROSS