Technical Peer Review of
“Estimation of Carbon Monoxide Exposures and Associated Carboxyhemoglobin Levels for Residents of Denver and Los Angeles Using pNEM/CO (version 2.1)”
(Johnson et al., 2000)

Summary of the Conference Call of Peer Reviewers

and

Written Responses to the charge questions submitted by the Technical Peer Review Panel

September, 2001

Prepared by
SCIENCE APPLICATIONS INTERNATIONAL CORPORATION
EPA Contract No. 68-D-98-113
Work Assignment No. 3-50
SAIC Project No. 1-0824-08-1818-000

Technical Peer Review of Estimation of Carbon Monoxide
Exposures and Associated Carboxyhemoglobin Levels for
Residents of Denver and Los Angeles Using pNEM/CO (Version 2.1)

Conference Call

September 20, 2001

Participants: Dr. P. Barry Ryan, Emory University; Stan Hayes, ENVIRON; Dr. William Ollison, American Petroleum Institute (API); Harvey Richmond and John Langstaff, Environmental Protection Agency (EPA); Richard Gardner and Denise Scott, Science Applications International Corporation (SAIC)

The conference call was held to discuss the findings of a peer review panel convened to review and evaluate the document "Estimation of Carbon Monoxide Exposures and Associate Carboxyhemoglobin Levels for Residents of Denver and Los Angeles Using pNEM/CO (Version 2.1)." Members of the review panel included: Dr. William Ollison, American Petroleum Institute; Dr. Barry Ryan, Emory University; and Stanley Hayes, ENVIRON. Richard Gardner, SAIC, served as moderator for the call. He began the meeting by asking the peer review panel to respond to the charge questions contained in their background materials. Following is a summary of the discussion.

1) ***Are the report and associated appendices clear and understandable for a technical audience? Are the assumptions clearly identified? If not, what areas of the report need to be modified?***

Comments

Barry Ryan thought the report was well written and well constructed and the assumptions were clearly defined. He noted that it was overall a good scientific document and thought it would be understandable to a series of technical individuals that could reproduce it if necessary.

Will Ollison stated that he was a fan of model and agreed that, although it was a large document and not an easy read; however, it was a good effort.

Barry Ryan thought that re-organization of some of the sections would make the document clearer. For example, there were instances were the results were presented first and the techniques used were described afterwards. Dr. Ryan added that this was minor complaint.

Stan Hayes was very supportive of this kind of analysis. He commended EPA in its effort to support this type of modeling. He felt quite strongly that this is the type of modeling that should be expanded in its scope and usage in other EPA programs. He complimented the authors on the amount of detail provided and that they were careful in describing the data. However, Mr. Hayes noted that he had some difficulty in following the discussion on occasion. Some of this difficulty was due in part to the complexity of the subject matter but some was due to explanations being split into different parts of report. He suggested that the authors address this as they think about polishing this report for publication. He added it would help if the authors could simplify and integrate the discussion more.

Barry Ryan concurred that this was a large amount of information and thought that an executive summary would help reduce confusion. He noted that Chapter 2 would be a good place for an overview.

Harvey Richmond asked the panel if they had any specific recommendations that would make the document better.

Will Ollison stated that given the importance of the document he was not averse to having it be a large document.

Stan Hayes asked Harvey Richmond to identify the audience of this document.

Harvey Richmond responded that this document is not intended for policy makers directly. He added that staff papers would be written for them. The target audience for this document is exposure experts and others on the Clean Air Scientific Advisory Committee (CASAC), and other interested parties along with their technical consultants.

Stan Hayes noted that there are a large number of assumptions in this model and he had a problem in identifying which of those assumptions were most important. He strongly recommended that there be sensitivity analyses contained in the report to provide the reader with enough perspective so he can identify the most important assumptions. He encouraged EPA to continue their efforts to distill the report down for publication in peer-reviewed journals. He noted that a lot of this report has already been presented in conferences. He thought that the process of distilling this down into journal articles requires a discipline that forces you to get to the most important information.

Barry Ryan agreed that much of this should appear in peer-reviewed journal articles. He concurred that there was a strong need for a sensitivity analysis and that it would add strength to the entire document.

Will Ollison agreed in part but not so much for the purpose of the Carbon Monoxide document itself as for its use in the subsequent Air Pollutant Exposure Model (APEX) development.   He added a sensitivity analysis would keep the readers focused on the most important part of model. He believed that you have to get into the nuts and bolts in order to direct further research.

2) ***Section 3 of the report explains the approaches used to prepare the CO air quality data for the exposure analysis and the air quality adjustment procedure to simulated attainment of the 8-hr CO NAAQS for Los Angeles. Are these approaches appropriate and technically sound?***

Comments

Stan Hayes questioned the relationship that is assumed to exist between the monitored CO data and the resulting exposures. He noted there are different micro-environments such as a near roadway and indoor environment that are being handled by some microenvironment multiplier weights that are deterministic not probabilistic. He added this is a critical assumption because everything we are doing suggests we can extrapolate from monitoring data at a single site within a large geographic area that we can make reliable estimates of micro-environmental concentrations to which people are exposed. He said there is a strong need to evaluate the relationship between monitored data and exposure. He asked Mr. Richmond to address this concern.

Harvey Richmond said that the authors went as far as they could given the limited amount of data. He added they identified the objective but were limited by information available.

Will Ollison asked if they had enough data to give a reasonable deterministic slope and intercept but not enough to do the probabilistic part or did it require too much time to do the probabilistic part.

Harvey Richmond stated he thought they were using a distribution on the Denver data and that it wasn't deterministic.

Will Ollison noted that if they were limited by run time then it is a lot quicker not to do the distribution.

Stan Hayes asked because he was not sure whether they did it probabilistically or not.

Barry Ryan stated that in certain aspects where constant multipliers were used instead of some type of probabilistic approach was a concern if the module was to be used as a prototypical model for some of the other pollutants. He added CO may not be such a problem but some of the other pollutants like ozone and particulate matter might be.

Harvey Richmond was not suggesting that the approach for this module would be used for other pollutants.

Will Ollison identified a concern that emerges with the use of a fixed site monitor. Monitors are classified according to spatial extent they represent. He noted that in Denver at least some of the monitors are micro-scale. These monitors are typically used where you expect a hotspot. If you are using a community monitor then taking your relationship between fixed site monitor and exposure concentration might give you one result but if you are taking your statistical relationship and basing it off a micro-scale monitor you might get another result. He urged caution in using micro-scale monitors with this multiplier approach.

Harvey Richmond said he would get back to the peer review panel regarding this issue but it was his understanding that a single factor was not being used. In Denver it was tied back to the distribution of actual monitored exposures in different micro-environments and tied to different monitored environments. Mr. Richmond noted that downtown there is a significant population exposed where a micro-scale monitor is used and you don't see large differences between the monitors.

Will Ollison identified a potential problem. He said that if you are going to take a community based monitor and try to infer from it a near roadway concentration and with multiplier which you will assume is 3. Then if you are applying it to a micro-scale monitor you are possibly double counting. He did not know if this was a problem or not.

Stan Hayes stated that this report uses an exposure-**district** based structure instead of one EPA used many years ago that was a neighborhood type.

Harvey Richmond agreed adding that there is memo documenting the switch and that there is an evaluation that looks at this issue that he will make available.

Will Ollison noted that if this multiplicative factor is distributional in nature the text needs to make this clear.

Harvey Richmond agreed.

Stan Hayes noted that it wasn't clear to him how they fixated on. This was not clear in the report.

Harvey Richmond said that some of this work had been done in the criteria document. An air quality analysis showed that for four areas the shape was maintained over a ten-year period even though overall levels came down. It is included an appendix to the air quality criteria document.

Will Ollison asked Harvey Richmond to confirm that what he is saying is that the GSD stays the same when you roll back the geometric mean.

Harvey Richmond responded yes.

Barry Ryan asked when you do the general rollback why is it that you would think that some other monitor isn't the big one. He asked Will Ollison if he had comments on this.

Will Ollison responded that he was asking the authors to perform a trend analysis, which apparently had already been performed.

Harvey Richmond noted that the Office of Research and Development (ORD) did the trend analysis for the criteria document

Will Ollison said that he was surprised that it was monatomic linear.

Barry Ryan expressed concern that when you do a rollback and the peak monitor just meets the criterion then another monitor's values may go up. He added that he has always been concerned about this occurrence.

Stan Hayes added that regarding the Los Angeles exposure assessment, there is a question regarding monitor representativeness. He noted there are limits on the range of people whose exposures are being included as there are for Denver. He suggested that EPA provide better terminology to describe this assessment to identify the population included in the exposure estimates. For example, in Los Angeles there are many people that live east of Pasadena that are not included in this analysis. Similarly, people that live in the San Fernando Valley are not included either. He understood why they weren't included but said that EPA should be more precise in describing the populations identified in the title of the report.

Stan Hayes asked if it would be helpful to give a preface for what the tool would be used for.

Harvey Richmond agreed and noted they would look into the suggestion.

Barry Ryan noted the model is geared toward finding people with the highest exposures in these areas.

1) ***Section 4 of the report discusses a modified version of the mass balance model presented by Nagda, Rector, and Koontz (1997), which is used to estimate indoor and in-vehicle CO concentrations. Are the approaches and algorithms used in the mass balance model appropriate and technically sound? Do the approaches use the best available data to estimate distributions for the various input parameters to the mass-balance model? If not, what additional data should be considered?***

Barry Ryan thought the model was a standard-type model- a compartmental mass balance model. He thought there were several things that were shaky but that overall the model is the appropriate one to use.

Harvey Richmond asked the panel if they had any concerns about sharing their comments with the CO team?

All peer reviewers agreed to have comments given to the CO team.

Stan Hayes noted that as a general matter, using these types of models is an improvement over using indoor/outdoor ratios that they used to use.

Barry Ryan agreed.

Stan Hayes reiterated his comment regarding sensitivity analysis and noted that it still applies. He said that EPA also needed to be cautious about extrapolating from limited data. For example, air exchange rates for the "residences-windows closed" microenvironment in Denver during the summer and autumn appear to be based on extrapolations from regional data having only 2 and 23 samples. You have to live with the data you have but he said that extrapolating from small numbers is always something to be nervous about. He added that maybe a sensitivity analysis would show you if they were important or not. Similarly for Los Angeles for the same microenvironment air exchange rates the distribution were extrapolated from regional data and he wasn't sure where the data were gathered from but there are strong temperature gradients as you move inland which is important. This will affect windows being open and air conditioners being used.

Barry Ryan agreed and noted that he did not know whether the variability was taken into consideration. He discussed a study he performed in the mid-1980s on the variability of air exchange rates from Santa Monica inland. If you have two data points you can't account for this variability.

Will Ollison asked if Barry Ryan thought there was a general extrapolatability, in other words a house and a window is the same as a house and a window its just a function of whether the window is opened or closed.

Stan Hayes noted that Harvard researchers have been finding that windows opened and closed can be a surrogate for the air exchange rate.

Harvey Richmond asked Barry Ryan to elaborate on his written comment regarding Section 4 of the report where Ryan stated "While such an assumption may not introduce much error, glossing over it suggests some problem in understanding the model. Unless a high quality numerical integration scheme is implemented, attempting to use this approximation is doomed to failure."

Barry Ryan stated that it depends on the numerical integration scheme. If you have a very rapid air change rate and you take a 1-hour time step, which it sounds like it is, in a forward time stepping type process it's going to fail immediately. That's just simple numerical analysis. That can easily be shown. He did not know what was done and the details of solving the differential equation. However, if it solved analytically then you have all the problems associated with time dependent air exchange rates and sources contributing. If it was solved numerically you can change those as a function of time but you have to have a robust numerical integration technique in order for the solutions to be reasonable.

Harvey Richmond believed it was the later but he would have to follow up on that.

Barry Ryan said there are full and explicit techniques that essentially do the whole thing in one shot, forward time-stepping techniques, or techniques that are somewhere in between.

Harvey Richmond stated that they used a 1-minute version in some of the microenvironments, such as vehicles.

Barry Ryan said that would make sense because those air exchange rates are once every 2- or 3-minutes.

Barry Ryan said that essentially the time-steps that are important are the inverse of the air exchange rates.

2) ***Section 5 of the report describes the approach to estimate alveolar ventilation rate as a function of oxygen uptake rate, which is in turn estimated as a function of energy expenditure rate. Is the overall approach and specific algorithms described in this section appropriate and technically sound? Do the approaches use the best available data to***

estimated distributions for the various input parameters? If no, what additional data should be considered?

Will Ollison noted that this is the second, if not the most important comment he had. He noted that this was the first time use of energy expenditure. He thought the authors needed to see if it works by comparing the results against measured rates to make sure you are in ballpark. He highly recommended that EPA verify this algorithm.

Harvey Richmond agreed.

Stan Hayes agreed while noting that this approach is a very important advance. He agreed that it needed to be analyzed and gain acceptance but it is an advance over qualitative approaches used in the past.

.

Barry Ryan supported this also. He noted that it is much better than saying here is the concentration now we know all we need to know. It is interesting to understand how it gets into the body. He thought people would look on this as a prototypical module that can be used with other pollutants.

Harvey Richmond responded that this was being used in other parts of the agency.

Stan Hayes stated that there might be a concern as you apply this to ozone and other compounds. How well does this model map into exposures-response relationships? He agreed this algorithm should be consistent with chamber studies.

Will Ollison noted it has to have the range of the chamber studies. He thought it would but the problem is the next step - what fraction of population picks up these ventilation rates? This is a key point to sensitivity tests. You need to be able to estimate the duration, frequency and size of the group.

Barry Ryan reiterated that he thought overall this module was a strong addition and an advancement over methods that were used in the past.

5. ***Section 6 of the report addresses how commuting patterns are estimated and included in the exposure analysis. Is the overall approach appropriate and properly implemented for the purpose intended? If not, why and what should be used?***

Comments

Barry Ryan supported a criticism regarding people that live in outlying areas.

Will Ollison added EPA needed to check on including non-workers. He offered comments in his written response on how to test this.

Stan Hayes noted that the report indicates that the greater the percentage of allocated versus those detected by census the greater the uncertainty in the results. He asked if this was an important limitation?

Harvey Richmond said he could not answer this off the top of my head but he did not have an impression that it was a major limitation.

.

Will Ollison urged that EPA put commuting of non-workers in and also that EPA put in the school community model as well.

Harvey Richmond noted that a limitation might be that non-worker commuting data is generally not available.

Barry Ryan supported Will Ollison's comments. He gave an example of mom's driving their children around and that both the mom and children are getting exposed.

Harvey Richmond noted that in order to address this you would need to use old data. In doing this, you would need to know whether the commuting patterns are the same or have they changed.

Barry Ryan noted that a lot of data are from the early 80s and asked if activity profiles are remotely matching? He expressed a concern about using old data.

6)5 Section 8 of the report discusses the major limitations and uncertainties associated with the exposure analysis. Does the report adequately and fairly characterize the major limitations associated with this analysis? Are any significant limitations and uncertainties omitted that should be included in the report? If so, what needs to be added?

Barry Ryan stated that the sensitivity analysis feeds into this section overall reiterating that you need to know the important variables. Sensitivity analyses are critical to address the contents in Section 8. Section 8 tries to be comprehensive but doesn't give quantitative estimates about the uncertainties.

Will Ollison suggested that for ground truthing we have more modern CO personal data that can be used. There are data from a Baltimore study that he conducted that could be used.

Harvey Richmond identified the Wilson/Colome study but said they are not like the Denver study because he didn't know what children's studies have measured. He asked Will Ollison how they might use the Baltimore data?

Will Ollison said that you would have to use Baltimore CO data. The model would be the same but you would use Baltimore specific data.

Stan Hayes strongly endorsed comments by Will Ollison on ground truthing. He said he does a lot of air quality modeling where EPA has recommended models that have had performance evaluations. Models have to prove themselves at some point. If a model can't reproduce observed data then it needs additional work.   At some point there has to be an effort by EPA to do a performance evaluation of this model.   He stated this really needed to be done

Barry Ryan agreed adding that this is the best game in town and stated that nobody else is doing anything like this.

Will Ollison added that maybe you don't have to evaluate the entire model only certain aspects.

Barry Ryan agreed that maybe you just have to measure the drivers.

Will Ollison noted that in Appendix J there is a characterization of angina/non-angina populations. He asked why did they use the 0-54 age group when they are modeling the 18-54 age group. He noted that in Appendix K there are a lot of statistical tests to see whether differences between model runs are significant or not. He added that if you perform more runs you could make it significant.

Harvey Richmond noted that EPA has gotten questions from CASAC regarding significance.

Barry Ryan agreed noting that Appendix K doesn't really say anything of importance about the variables.

Harvey Richmond expressed his appreciation on behalf of EPA for tackling this large document in such a short period of time.

ENVIRON

September 11, 2001

MEMORANDUM

To: Richard Gardner, SAIC

From: Stan Hayes, ENVIRON

Subject: **Technical Peer Review of "Estimation of Carbon Monoxide Exposures and Associated Carboxyhemoglobin Levels for Residents of Denver and Los Angeles Using pNEM/CO (Version 2.1)"**

I strongly support historical and ongoing efforts by the U.S. Environmental Protection Agency (USEPA) and others to develop and improve population exposure assessment models and data they require, and to apply them in NAAQS review and other contexts. Such efforts are warranted, bring greater realism to important matters of public policy, and have substantially advanced the state of the science in this field. I commend USEPA and others for their sponsorship of such work and urge that the use of such techniques continue and be expanded to other criteria and air toxic pollutant applications.

The time and resources provided by USEPA for this review are limited. Given that, the scope and complexity of this report and its appendices make a full and comprehensive review impractical. Subject to that caveat though, I offer my comments, which I divide into two parts: first, comments on the overall report, and then, responses to the standardized questions posed by the peer review charge. While I have a number of additional comments than those I state here, I focus on those I believe to be most important.

A. Overall Comments

The following are my overall comments:

- High overall quality. The report and its methodology are of high scientific quality. The report employs the latest version of pNEM/CO, which is the result of an ongoing evolution of exposure models at USEPA and constitutes an ambitious and comprehensive attempt to improve the realism of exposure estimates for NAAQS review.
- Need to more precisely state scope of exposure assessment. The report's scope should be more precisely stated. Its title implies that CO exposures for all residents of Denver and Los Angeles are included. But, only exposures for adults are estimated, and not for children. Also, not all adult exposures are included. For example, exposures in the Los Angeles basin are confined to the higher-CO portions of the basin; no areas east of Pasadena or in much of the San Fernando Valley are included. And, no exposures that oc-

curred at distances greater than 10 kilometers from a monitor were included, nor were exposures for smokers.

- Need to further evaluate uncertainty introduced by use of exposure district approach. The report should further evaluate uncertainty introduced into the exposure assessment by pNEM/CO's use of an exposure district-based formulation. Because CO is largely a motor vehicle-related pollutant, it exhibits strong spatial gradients near roadways and other areas of vehicle emission concentration. In recognition of this roadway proximity effect, a "neighborhood type" approach was used some years ago in NEM/CO analyses. Now, an "exposure district" approach is used, in which cities are subdivided into large geographical regions, within which outdoor CO is assumed to be accurately characterized by a single monitor (some nearer roadways than others). An issue to be examined is the extent to which the reliability of the assumed relationship between fixed-site monitoring data and personal exposures is affected.

- Need to further evaluate validity of assumed relationships between concentrations measured at fixed-site monitors and those in microenvironments. To estimate exposure concentrations, weights [M(m)] assigned to each microenvironment are applied as multipliers to concentrations measured at fixed-site monitors. The values of these multiplier weights are critical, since they represent the systematic differences that exist among microenvironments, due to such factors as differences in proximity to motor vehicles. The basis of and the validity of assumptions about these microenvironment-specific weights and how well they capture systematic differences among microenvironments is unclear to me. More discussion of these and other uncertainties introduced by the model's exposure district formulation is appropriate and needed, along with more discussion about analyses that might have been done to examine and resolve such issues.

- Need to further evaluate and demonstrate validity of large number of model parameter assumptions. The report should further consider the cumulative effect of the large number of assumptions made about model parameter distributions and formulation. While discussion of model methodology and data in the report and supplemental documentation is extensive (and to be commended), one must always be concerned about the effect of uncertainty when such a large number of assumptions is made, particularly if based on samples of limited size (e.g., residential air exchange rates in Denver during summer and autumn are based on just 2 and 23 samples, respectively).

B. Peer Review Charge Questions

My responses to the questions posed by the peer review charge are as follows:

Question 1 – Are the report and associated appendices clear and understandable for a technical audience? Are the assumptions clearly identified? If not, what areas of the report need to be modified?

- Extensive detail provided. The report and associated appendices are extensive and provide a great deal of detailed information about pNEM/CO's methodology and model parameters. The authors are to be commended for this.

- Difficulty in following discussion. Rather than collect a complete discussion of a particular aspect of methodology or data together in one place, the report often cross-references text and tables presented elsewhere in the report. Although perhaps unavoidable to some extent given topic complexity, I found these discussions sometimes disjointed and difficult to follow. This problem is compounded by the large number of assumptions made about methodology and data distributions.

- Need for sensitivity analysis. So many assumptions are made that I found it difficult to identify those that are most important and to judge the effect of their uncertainty on exposure estimates. A good qualitative summary of the principal limitations of the pNEM/CO methodology is presented in Section 8. But, I would strongly recommend that a more formal and quantitative sensitivity analysis be conducted to identify key assumptions (and data gaps) and to quantify the resulting range of uncertainty in exposure estimates.

- Need for more extensive publication in peer-reviewed literature. I strongly urge that results and methodology be published more extensively in the peer-reviewed scientific literature. A review of report references demonstrates that most related documentation is in USEPA reports and memoranda, with only a limited number of conference presentations. More extensive publication in the peer-reviewed literature is particularly important in light of the number, significance, and complexity of elements incorporated into the pNEM/CO model, particularly in its current updated form.

Question 2 – Section 3 of the report explains the approaches used to prepare the CO air quality data for the exposure analysis and the air quality adjustment procedure to simulated attainment of the 8-hr CO NAAQS for Los Angeles. Are these approaches appropriate and technically sound?

- Need to further evaluate and verify the assumed relationship between CO concentrations at fixed-site monitors and personal exposures. Although not really discussed in Section 3, a critically important issue is the technical soundness of the relationship assumed to exist between CO concentrations measured by fixed-site monitors and those to which people are actually exposed. The differences among microenvironments appear to be represented in pNEM/CO by microenvironment-specific multiplier weights M(m). Further discussion and justification of these weights, their validity, and their representativeness is needed, particularly in light of the roadway proximity effect noted earlier. Several questions of importance include: For a given microenvironment, was the same value of M(m) used for every exposure district? If so, why? Could the choice of M(m) be included probabilistically, perhaps as a distribution? How representative was the personal monitoring study from which they were derived?

- Appropriateness of Denver monitoring sites unclear. The appropriateness of several of the monitoring sites chosen for Denver exposure analyses is unclear. Two of the seven chosen monitoring sites are categorized by USEPA as having a "microscale" spatial scale. No spatial scale is given for three additional sites. It is unclear, therefore, whether these monitors adequately characterize air quality in their respective exposure districts, since "microscale" monitors are by definition representative only of conditions in their immediate vicinity. How such microscale effects would affect the validity of the assumed relationship between monitoring data and personal exposure, particularly in the choice of M(m) weights, is unclear.

- Limits on spatial coverage of Los Angeles exposure assessment should be more clearly stated early in the report. None of the monitoring sites in the Los Angeles basin used in the exposure assessment are located east of Pasadena or west of Burbank in the San Fernando Valley, where many basin residents live. While exclusion of these areas may be appropriate for CO NAAQS review purposes, since CO levels there meet the standards, the report should clarify in its title and summary (that is, early in the report) that exposures calculated for Los Angeles are for the higher-CO portions of the basin.

Question 3 – Section 4 of the report discusses a modified version of the mass balance model presented by Nagda, Rector, and Koontz (1997) which is used to estimate indoor and in-vehicle CO concentrations. Are the approaches and algorithms used in applying the mass balance model appropriate and technically sound? Do the approaches use the best available data to estimate distributions for the various input parameters to the mass-balance model? If not, what additional data should be considered?

- Support for use of explicit indoor model. I strongly support use of an explicit model to estimate concentrations in indoor microenvironments. The mass balance model used by pNEM/CO seems a reasonable choice and a significant improvement over the use of fixed indoor-outdoor ratios, as in some prior modeling, so long as model parameter values are appropriate (see comments below).

- Need for sensitivity analysis. A large number of assumptions are made in defining model parameter distributions. However, it is difficult to identify which of these assumptions are most important. As recommended earlier, I believe that a systematic sensitivity analysis should be done to identify critical assumptions, determine the range of parameter variation, and quantify the effect of parameter uncertainty on exposure estimates. Such an analysis should also determine if significant data gaps exist in key model parameters. If so, additional data should be gathered and incorporated into future modeling.

- Need to be cautious about extrapolating from limited data. Model parameter distributions are sometimes derived from limited data (e.g., having only a small sample size or of uncertain representativeness). For example, air exchange rates for the "residences–windows closed" microenvironment in Denver during summer and autumn appear to be based on extrapolations from regional data having only 2 and 23 samples, respectively. Similarly, while from a sound study, data from just a single residence are used to specify the distribution of air exchange rates for the "residence–windows open" microenvironment in both cities.

- Need to account for possible geographic variations in model parameter distributions for Los Angeles area. Air exchange rates for the "residences–windows closed" microenvironment in Los Angeles were extrapolated from regional data. I am uncertain as to the representativeness of those data for all of the modeled area, since there are wide and systematic variations in air temperature from coastal to inland areas of the Los Angeles basin that might well affect when and whether windows are closed. This coastal-to-inland temperature variation might also affect the percentages of homes using air conditioning, perhaps introducing a need to use different distributions in coastal and inland areas.

Question 4 – Section 5 of the report describes the approach to estimate alveolar ventilation rate as a function of oxygen uptake rate, which is in turn estimated as a function of energy expenditure rate. Is the overall approach and specific algorithms described in this section appropriate and technically sound? Do the approaches use the best available data to estimated distributions for the various input parameters? If no, what additional data should be considered?

- Support for the use of such methods. I support the use of such methods to more explicitly incorporate the effect of exercise level into exposure assessment. I commend USEPA and others who have sponsored the development and improvement of such methods. Those efforts represent a major improvement over qualitative exercise categories, as originally used in past NAAQS exposure and risk assessments.

- Need for sensitivity analysis. A large number of assumptions are made in the ventilation algorithm that is used. As I have stated repeatedly above, I strongly recommend that a sensitivity analyses be conducted to identify key variables and to quantify the effect of their uncertainty on exposure estimates. The sensitivity analysis should include key variables in the ventilation algorithm. Because of its importance, particular attention should be paid to the model's assumption that the ratio of V_A to VO_2 is "relatively constant regardless of a person's physiological characteristics or energy expenditure rate."

- Need to match with exercise rates in exposure-response studies. Care should be taken that the ventilation algorithm used to characterize exercise levels produces results that are consistent with the exercise levels in chamber or other studies used to quantify exposure- or dose-response relationships used in any subsequent health risk analyses.

Question 5 – Section 6 of the report addresses how commuting patterns are estimated and included in the exposure analysis. Is the overall approach appropriate and properly implemented for the purpose intended? If not, why and what should be used?

- Reasonable approach. The approach used is similar to other origin-destination table-based methods used in past NEM modeling. Given the limitations of the BOC database that is used, the approach taken in the modeling seems reasonable.

- BOC database quality. I understand that the WR ratio is a measure of the percentage of people allocated in the BOC database to commute "flow," rather than determined directly from census responses. Some WR values for Los Angeles seem a bit high. How important is this? Would uncertainty in these values have any important effect on exposure estimates?

Question 6 – Section 8 of the report discusses the major limitations and uncertainties associated with the exposure analysis. Does the report adequately and fairly characterize the major limitations associated with this analysis? Are any significant limitations and uncertainties omitted that should be included in the report? If so, what needs to be added?

- Reasonable discussion of limitations and uncertainties. The section identifies and discusses a number of potentially important limitations and uncertainties. The discussion is appropriate as far as it goes. However, the discussion in Section 8 is only qualitative. It is difficult to determine what the effects of these limitations and uncertainties are on the report's results.

- Need for quantitative characterization of uncertainty range. As suggested elsewhere in these comments, I urge USEPA to conduct a systematic sensitivity analysis, first to identify the most important limitations and sources of uncertainty and then to estimate their effect on modeling results.

Technical Peer Review of "Estimation of Carbon Monoxide Exposures and Associated Carboxyhemoglobin Levels for residents of Denver and Los Angels Using pNEM/CO (Version 2.1)."

P. Barry Ryan
Emory University

Overview

The report Estimation of Carbon Monoxide Exposures and Associated Carboxyhemoglobin Levels for residents of Denver and Los Angels Using pNEM/CO (Version 2.1) by Johnson, et al., describes the development and implementation of a probabilistic modeling framework for the determination of carbon monoxide (CO) concentrations and carboxyhemoglobin (COHb) levels experience by individuals living in urban environments. The pNEM model has been extant in the literature for more than a decade. Version 2.1 represents an updating of the entire modeling package. The report focuses on evaluating CO concentrations and COHb levels in two cities: Denver and Los Angeles. It uses both monitored and modeled data in developing such estimates.

Chapter 1 of the document gives an overview of the report and describes its contents. The description of procedures and methods used in pNEM v2.1 is outlined in Chapter 2 of the document. The ensuing chapters explore in detail the procedures used.

The document as a whole is well written and well developed. Few, I believe would venture past Chapter 2. Only those with keen interest in the modeling details would delve into the "deeper" sections of the document. I feel, however, that those that do make the effort will be rewarded by the rich detail provided by the authors. While small quibbles can be made, the authors are to be congratulated for and impressive modeling effort.

The charge questions given below are specific to each chapter. Comments on details of the chapters are found in the individual sections.

Charge Questions

1) *Are the report and associated appendices clear and understandable for a technical audience? Are the assumptions clearly identified? If not, what areas of the report need to be modified?*

The report as given is both clear and understandable for a technical audience. Chapter 2 is most useful in giving an overview of the individual elements of the document. In general, assumptions are well developed and well described. A technical expert in modeling with sufficient time and resources could reproduce the modeling results after proper development of a modeling framework and codes. Make no mistake, however, that such an undertaking would be a formidable task requiring many person-years of effort.

As no specific charge question was brought forth for Chapter 2 and, I believe that this chapter will be read by many as opposed to the few who will read the subsequent chapters in detail, I will raise a few questions regarding Chapter 2 here.

On Page 2-3 in the third complete paragraph, the authors discuss descriptive statistics after accounting for missing data. It would be good to discuss the missing data substitution techniques here.

On Page 2-9, the first full paragraph discusses the development of Exposure Event Sequences (EESs). A few words about the efficacy of this procedures and the uncertainty in such approaches would be nice here.

On Page 2-11, the last full paragraph discusses the effects of active smoking on CO exposure levels. I am not clear on what the coding problems associated with the Consolidated Human Activity database (CHAD) are that would preclude characterization of passive smoking exposure for active smokers. A few words here would clarify.

The Table beginning on Page 2-12, does not add to this summary section and may well be relegated to an appendix or at least to the Section describing such in more detail.

On Page 2-18 under the heading Pollutant Concentrations, it would serve the authors well to describe the intent of the model better. I found the presentation confusing until I realized that the CMIN values are meant to be exposure above a long term background that occur over short time periods and that, therefore, the background concentration should be viewed as not a long-term average but rather on "non-source" exposure experienced by individuals. A discussion of this point will help clarify.

On Page 2-20 in the second full paragraph, we see discussion of the Denver activity study. It would be useful to assess the impact of the 20 intervening years on CO exposures in this city. It might be reasonable to assume that ambient concentrations in Denver are now lower and that activity patterns may have changed. Are these facts of importance in the modeling effort?

On Page 2-21 in Equation 2-2, I think it would be of use to give some rationalization of the multiplicative model used. Why was it selected over an additive approach? Following onto the next page, we see the selection of various values for certain important parameters. Some discussion of why they were selected and why constant values were chosen would be of interest.

On Page 2-25 seven lines from the bottom, the authors state that there is a "…need for parsimony…" in the selection of their parameters. While there may be a desire for parsimony, there is certainly no need.

In the following pages, most notably on Page 2-27, the authors first present results and then tell how they obtained them. I think it would be clearer if the opposite approach were taken. Tell the reader first what was done, then what the results were.

On Page 2-30 there is a discussion of the ventilation rate given the symbol VA. I think using such a symbol unadorned is asking for confusion. Later, it may be necessary to divide by some volume, say the lung volume normally symbolized by a V as well. We would have the unfortunate circumstance of having VA/V that would appear unitless, but would have units of inverse time. A dot over the VA is a standard way of indicating a time derivative.

Clarification is needed on Page 2-33 under the Physiological Profile Generator. It is stated that: "blood volume was determined as a function of weight and height, where height was estimated as a function of weight…." This makes blood volume a function of weight only since height is a function of weight. Is this what was intended?

2) *Section 3 of the report explains the approaches used to prepare the CO air quality data for the exposure analysis and the air quality adjustment procedure to simulate attainment of the 8-hr CO NAAQS for Los Angles. Are these approaches appropriate and technically sound? If not, what changes to the approach should be chosen and why?*

In general, the Section 3 of the Report describes approaches that are both appropriate and technically sound. I believe that the Section could be shortened somewhat by simply eliminating discussion of sites with insufficient data to perform analyses and focusing discussion on those chosen to aid in the model development. There are many data tables in this section that might be more effectively gathered at the end or, perhaps, put in a separate appendix.

On Page 3-13, the authors discuss the Johnson and Wijnberg (1981) model for time series data missing value estimation. Since this paper is a difficult-to-get conference presentation, it would be of interest to see the model developed here. In the next paragraph, the authors report that substitution of the missing values into the data stream do not significantly affect the distributions of any data set. One asks: How could they? If the data are properly modeled, they must fit the time series of the data and, therefore, could not affect the distribution. If they did, the time series model would be incorrect. This should be clarified.

As a general point, I find the model terms, e.g., CMON(m,h,s) hard to decipher. Perhaps it is my mindset, but the parameters names and variables in the model do not seem intuitive.

3) *Section 4 of the report discusses a modified version of the mass-balance model presented by Nagda, Rector, and Koontz (1997) which is used to estimate indoor and in-vehicle CO concentrations. Are the approaches and algorithms used in applying the mass balance model appropriate and technically sound? So the approaches use the best available data to estimate distributions for the various input parameters to the mass-balance model? If not, what additional data should be considered?*

I am more familiar with mass-balance models than many of the other approaches used in this document and, therefore, spent more time reviewing. The model as presented in Equation 4.1 represents a rather complete description of pollutant concentration change in typical indoor environments. However, the differential equation is somewhat incomplete in that the air exchange, ν, and the source term S are likely to be time dependent, especially in the case were sources of CO, such as combustion appliance or active smoking, are present. One may argue with the assumption that the mixing factor and fractional effective volume are equal to one as well, but the assumption has been made here essentially to reduce the complexity of the model.

The intergraded form of the model, equation 4-3 tacitly assumes that the air exchange rate and source terms are time-independent. Again, this is problematic. Further, the equation is only strictly true if the time increment, Δt, is a differential element dt. In the case described, Δt is one hour- a time scale long with respect to the scale of the model, i.e., ν^{-1}. While such an assumption may not introduce much error, glossing over it suggests some problem in understanding the model. Unless a high quality numerical integration scheme is implemented, attempting to use this approximation is doomed to failure.

Confidence in the procedures is shaken further by equations 4-13 – 4-15 in which the exponential $e^{-\nu}$ appears. Recall that ν is the air exchange rate has units of inverse time. Mathematically, the argument of the exponential <u>must</u> be unitless so these expressions are wrong. The authors have been somewhat careless in their notation because they have multiplied the air exchange rate by 1 hr, a factor that makes the product unitless, but they have only included the previously defined ν value. This may just be poor bookkeeping, or it may be indicative of deeper problems in the model. For example, the model may work well for 1 hour time increments, but fail for any other time increment. This should be checked.

The remainder of this section describes data gathering needed for the parameters of the models described above. The authors have provided a service to the scientific community in tabulating these results. I have made no effort to ascertain whether they have completed their task effectively nor have I addressed the each reference to determine whether they have transferred the results well. I believe that they have. However, I do question some of the results. In particular, the results referenced as Johnson Memorandum No. 1, 1998 begs for peer review (See Table 4-6, Page 4-17). Further the Burner Emission factor does not distinguish between pilotless and pilot burners. Other problems may be noted as well.

In general, the remainder of the algorithms selected seem appropriate. Selection of various factors are based on probabilities found in the specific region being modeled- either Los Angeles or Denver- and appear to have been done properly. Specific problems, such as selection of a lognormal distribution based upon the Johnson 1998 memorandum or the Rosenbaum, et al., memorandum to Harvey Richmond describing air exchange rates in vehicles, do not significantly affect the impression of the overall parameter selection effort. Assuming these algorithms and fitting procedures for previously collected data are reasonable, then one accepts the modeling effort as state-of-the-art. I would have more confidence, however, if the authors were to present their work to the scientific community in the form of peer-reviewed publications rather than reports to EPA. EPA may find this acceptable, but in the long run there will be greater acceptance of such models if they are exposed to the "light of day."

Note that the model developed in Section 4.6.2 Residential Locations is identical to that developed in Section 4.1 and thus is subject to all of the objections raised in that section regarding the integrated form of the model, the value of the argument of the exponential function, and the numerical solution to the differential equation. These objections should be addressed in the final version of the report.

Many assumptions in this section are not fully justified. Generally, these include the assumption of lognormality of the variables, and the decision to truncate the values selected from the distribution at certain percentiles. While not well justified in the text, the assumption of lognormality is not likely to be in serious error. It is the experience of this reviewer and many other researchers in the field that the results of simulations such as these are not strongly influenced by the analytical form for the distribution selected. Further, the truncation of the distributions to ensure that non-physical, very small or very large compartments for vehicles or homes are not encountered is likely to produce better results rather than worse.

4) *Section 5 of the report describes the approach to estimated alveolar ventilation rate as a function of oxygen uptake rate, which is in turn estimated as a function of energy expenditure rate. Is the overall approach and specific algorithms described in this section appropriate and technically sound? Do the approaches use the best available data to estimate distributions for the various input parameters? If not, what additional data should be considered?*

My expertise is not in the area of physiological modeling so my comments in this area should be viewed as those of a relative novice.

I noted the presence of a constant in Equation 5-5, i.e., 19.63 that appears to be the results of a statistical analysis, perhaps a regression on some data. The two references that follow, Joumard, et al., 1981 (which I have encountered before) and Galetti 1959 (which I have not encountered previously) do not appear in the reference list. Clearly they should, and I am interested in pursuing them as this equation is of interest.

Continuing through this section, it would appear that the authors have made reasonable efforts to model alveolar ventilation rates as accurately as possible and to ensure that such modeled rates represent values that are physically reasonable. Although relatively complex in nature, it would appear that the methods selected are both appropriate and scientifically sound. I can make no judgment about the selection of the data for parameterizations as the "best" as I am not familiar with the literature in this field.

5) *Section 6 of the report addresses how commuting patterns are estimated and included in the exposure analysis. Is the overall approach appropriate and properly implemented for the purpose intended? If not, why and what should be used?*

The algorithm used in developing commuting patterns for use in pNEM investigation use data drawn from the 1990 US Census regarding flow of individuals among census tracts. The ""Origin-Destination" algorithm appears to be well developed and appropriate for this type of modeling. It is somewhat disconcerting that certain individuals living in the Denver and Los Angeles areas cannot be modeled as they live too far from a monitoring site. It would seem that some approximation for these individuals could be made.

Despite the criticism above, the approach selected for this component of the modeling appears to be solid and scientifically defensible and properly implemented for the purpose intended. Little valid criticism can be made.

6) *Section 8 of the report discussed the major limitations and uncertainties associated with the exposure analysis. Does the report adequately and fairly characterize the major limitations and uncertainties associates with this analysis? Are any significant limitations or uncertainties omitted that should be included in the report. If so, what needs to be added?*

The presentation given in Section 8 of this report develops, categorizes, and attempts to quantify the major limitations of the pNEM model inputs and outputs. I think it does so fairly and with good intent.

One discussion not given is the uncertainty introduced in house volume by assuming a constant eight-foot ceiling height. Volumes are generated with lognormal characteristics completely determined by housing square footage. Ceiling heights do vary, generally between approximately 7.5 feet and 10 feet with a distribution that may be knowable. Even if not, an assumption of the distributional character of this variable, i.e., uniform or triangular with most likely value of eight feet, might yield a better assessment of the uncertainty in house volume.

Responses to Charge Questions

As the reviewers consider the charge questions, they are asked to keep in mind the requirement that inputs to the exposure analysis generally be based on literature cited in the Air Quality Criteria for Carbon Monoxide (EPA, 2000), other peer-reviewed materials, and widely accepted data bases (e.g., Bureau of the Census data, AIRS).

1) Are the report and associated appendices clear and understandable for a technical audience? Are the assumptions clearly identified? If not, what areas of the report need to be modified?

Sensitivity Testing - Since pNEM/CO Version 2.1 is the most up-to-date and advanced version of the CAA regulatory exposure model and is the prototype of the next generation, APEX, it would be prudent to conduct a more elaborate sensitivity test of model input assumptions. Synchronized random number sampling, as discussed in Appendix K, provides an efficient means of testing stochastic models. An extensive sensitivity analysis[1] of the pNEM/O3 model used in the 1997 ozone rulemaking has been previously provided to the Agency. Results from an expanded analysis of pNEM/CO input algorithms would provide a quantitative basis for evaluating parameter uncertainties beyond that discussed qualitatively in Section 8 of the report. Such analyses would also provide a means of prioritizing model research needs and of focusing APEX development towards specific model algorithms with the most impact on exposure estimates.

2) Section 3 of the report explains the approaches used to prepare the CO air quality data for the exposure analysis and the air quality adjustment procedure to simulate attainment of the 8-hr CO NAAQS for Los Angeles. Are these approaches appropriate and technically sound? If not, what changes to the approach should be chosen and why?

Rollback Algorithm - A proportional (Eq. 3-1) rollback approach is used in pNEM/CO to adjust hourly Los Angeles air quality data to levels just attaining the CO NAAQS. Denver air quality is thought just to attain the standard and so is used unadjusted. No basis is given for the Los Angeles proportional rollback assumption. Neither was there an empirical basis provided for the three different monotonic rollback approaches (Weibull, proportional, quadratic) evaluated during the 1997 ozone rulemaking. Recent publication[2] of empirically based trend analyses indicate that, in general, ozone air quality approaches attainment non-proportionally and non-monotonically (i.e., higher and lower concentrations decrease more rapidly than mid-level values which may even increase modestly). Given the impact of the assumed rollback approach on estimates of population exposure, a similar trend analysis of historic CO levels from areas approaching attainment should be completed to provide a realistic means of adjusting CO air quality to compliance.

3) Section 4 of the report discusses a modified version of the mass-balance model presented by Nagda, Rector, and Koontz (1997) that is used to estimate indoor and in-vehicle CO concentrations. Are the approaches and algorithms used in applying the mass balance model appropriate and technically sound? Do the approaches use the best available data to estimate distributions for the various input parameters to the mass-balance model? If not, what additional data should be considered?

Open Window Status Algorithm - Although algorithms simulating air exchange rate distributions for open and closed windows in vehicles and residences are discussed in the report, there is relatively little explanation of the basis for the window status algorithm used to project whether residential windows are open or closed during a particular period. Neither is a reason provided as to why diurnal (3-hour) patterns of open window status were not simulated as they were in the 1997 pNEM/O3 model. Appendix G of the report notes that the residential window status algorithm is also used for vehicles, even though residential behavioral patterns for opening windows are not the same as for vehicles, since "there does not seem to be any available good data directly applicable to vehicles." Recent research[3], however, has produced such data for vehicles that should be used to develop an improved vehicle window status algorithm for pNEM/CO. Furthermore, TRJ Environmental, an EPA pNEM/CO contractor, has completed 2001 summer measurements of window status patterns for vehicles and is initiating fall surveys for both vehicles and residences. These data should also be available shortly and should be used to verify or develop improved simulations of this key parameter.

4) Section 5 of the report describes the approach to estimated alveolar ventilation rate as a function of oxygen uptake rate, which is in turn estimated as a function of energy expenditure rate. Is the overall approach and specific algorithms described in this section appropriate and technically sound? Do the approaches use the best available data to estimate distributions of the various input parameters? If not, what additional data should be considered?

Breathing Rate Algorithm – EPA's use of energy expenditure to estimate breathing rate in the pNEM/CO model sets a precedent for its further use in other more important pollutant exposure assessments (e.g., ozone, fine particles). For this reason, OAQPS should verify this methodology *now*, by comparisons of estimated and measured ventilation rates, before using this new approach in pNEM/CO, APEX, or any other subsequent regulatory risk assessments. Using paired run approaches demonstrated in earlier[1] sensitivity tests of pNEM and discussed in Appendix K of this report, the Agency should match simulated breathing rates to measured ventilation patterns for the same associated behavioral patterns reported by the monitored subjects. For example, datasets[4] within CHAD for Los Angeles elementary and high school students provide 94 daily pairs of monitored behavioral and breathing rate patterns. In addition, there are 79 measured pairs for 19-51 year-old subjects in companion Los Angeles studies[5, 6] not included in CHAD[7] that are also available for comparison. These datasets should be used to validate the breathing rate algorithm in pNEM/CO prior to its use in rulemaking.

5) Section 6 of the report addresses how commuting patterns are estimated and included in the exposure analysis. Is the overall approach appropriate and properly implemented for the purpose intended? If not, why and what should be used?

Non-worker Commuting – As noted in Section 2.1, the susceptible population chosen by EPA for CO includes only adults since the incidence of ischemic heart disease in individuals younger than 19 is minimal. Members of non-working demographic groups older than 18 are also constrained to remain within the home exposure district. This assumption appears unrealistic for the target population. As discussed in Section 5.6, the Agency concludes that "the differences between angina and non-angina subjects in percentages of time spent outdoors or in a motor vehicle were not generally statistically significant." If ischemia is relatively evenly distributed between workers and non-workers of an age group, then it's difficult to rationalize why these generally older women (i.e., without children in the home) must make so many more short trips near home, in order to balance the vehicle time expended by workers who commute relatively long distances for extended periods to work or school. The more reasonable explanation may be that most adult non-workers travel similar or even greater distances than do workers, on average, particularly if they choose to do so during the less congested non-rush hour periods. OAQPS should test whether allowing non-workers to commute yields better matches to monitored exposures than does restricting them to the home district. Should the Agency decide later to include children in the CO assessment, a school commuting model should also be included[8].

> 6) Section 8 of the report discusses the major limitation and uncertainties associated with the exposure analysis. Does the report adequately and fairly characterize the major limitations and uncertainties associated with this analysis? Are any significant limitations or uncertainties omitted that should be included in the report? If so, what needs to be added?

Exposure Model Verification – As noted in the report at the end of Section 8.3, "researchers have not attempted to validate [pNEM/CO] Version 2.1 through the use of personal monitoring data." This failure attributed to the fact that "there has not been a large-scale personal monitoring study conducted during the 1990's" and that "personal monitoring data from the 1982/83 Denver study are considered to be unrepresentative of current exposure conditions." To the contrary, continuous personal CO monitoring data has been reported[9] for technicians following scripted behavioral patterns typical of older (65+ years old) adults during the spring/summer and fall/winter of 1998 in Baltimore, MD. Approximately 30 subject-days of hourly personal activity/exposure data are available from this source for comparison to exposures estimated by pNEM/CO Version 2.1 for identical activity scripts. It is crucial that verification of the pNEM/CO model be attempted prior to its use in rulemaking. Such ground-truthing[1] of pNEM/O3 in 1997 to a Los Angeles scripted technician study[10] indicated substantial model over-estimates of measured hourly personal O3 exposures.

References

1. Johnson, T., W. Ollison, (1997), "Sensitivity Testing of pNEM/O3 Exposure Estimates to Improvements in Model Algorithms", VIP-74, proceedings *AWMA Measurement of Toxic and Related Air Pollutants Conference*, Research Triangle Park, NC, pp. 226-245; see also Appendix A in API, (1997), Sensitivity Testing of pNEM/O3 Exposure Estimates to Changes in the Model Algorithms, by T. Johnson, TRJ Environmental, Inc. API HESD Pub. No. FR 2. Washington, DC.

2. Lefohn, A.S., D.S. Shadwick, S.D. Ziman, (1998), "The Difficult Challenge of Attaining EPA's New Ozone Standard," ES&T 32: 276A-282A.

3. Long, T., T. Johnson, W. Ollison, (2001), "Frequency of Open Windows in Motor Vehicles under High Temperature Conditions: A Videotape Survey in Houston, Texas during September 2000," JEAEE (submitted).

4. Spier, C.E., D.E. Little, S.C. Trim, T.R. Johnson, W.S. Linn, J.D. Hackney, (1992), "Activity Patterns in Elementary and High School Students Exposed to Oxidant Pollution," JEAEE 2: 277-293.

5. Shamoo, D.A., T.R. Johnson, S.C. Trim, D.E. Little, W.S. Linn, J.D. Hackney (1991), "Activity Patterns in a Panel of Outdoor Workers Exposed to Oxidant Pollution," JEAEE 1: 423-438.

6. Linn, W.S., C.E. Spier, J.D. Hackney, (1993), "Activity Patterns in Ozone-Exposed Construction Workers," J. Occupational Med & Tox 2: 1-14.

7. McCurdy, T., G. Glen, L. Smith, Y. Lakkadi, (2000), "The National Exposure Research Laboratory's Consolidated Human Activity Database (CHAD)," JEAEE 10: 566-578.

8. API, (1999), "A Home-to-School Commuting Model for Use in Population Exposure Assessments," API/HESD Publication No. FR 5, American Petroleum Institute, Washington, DC, April.

9. Chang, L.-T., P. Koutrakis, P. Catalano, H. Suh, (2000), "Hourly Personal Exposures to Fine Particles and Gaseous Pollutants: Results from Baltimore, MD," JAWMA 50: 1223-1235.

10. Johnson, T., K. Clark, K. Anderson, A. Geyh, W. Ollison, (1996), "A Pilot Study of Los Angeles Personal Ozone Exposures during Scripted Behaviors," In *Measurement of Toxic and Related Air Pollutants*, VIP-64, Air & Waste Management Association, Pittsburgh, PA, pp. 358-365.