UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
WASHINGTON, D.C. 20460

September 30, 2002

EPA-SAB-CASAC-LTR-02-003

OFFICE OF THE ADMINISTRATOR
SCIENCE ADVISORY BOARD

Honorable Christine Todd Whitman
Administrator
U.S. Environmental Protection Agency
1200 Pennsylvania Avenue, NW
Washington, DC 20460

Subject: Review of the *Air Quality Criteria Document for Particulate Matter: Third External Review Draft* (EPA 600/P-99/002aC & 002bC): A CASAC Review

Dear Governor Whitman:

The Clean Air Scientific Advisory Committee (CASAC) of EPA's Science Advisory Board, supplemented by expert consultants (together referred to as the "Panel"), met on July 18-19, 2002 to review the two-volume April 2002 draft document, *Air Quality Criteria for Particulate Matter - Third External Review Draft* (EPA 600/P-99/002aC and EPA 600/P-99/002bC) (EPA, 2002), in a public meeting in Research Triangle Park, NC. This was the third CASAC review of the draft Criteria Document (CD) for particulate matter (PM) in the current cycle for reviewing the National Ambient Air Quality Standards (NAAQS) for PM. CASAC review of this document is required by section 109 of the Clean Air Act.

As noted below, the CASAC could not come to closure on this draft document and requested that the Agency revise the draft for another review.

1. BACKGROUND

The Panel reviewed the First External Review Draft of the PM Criteria Document (EPA, 1999) in December 1999, focusing primarily on the organization, structure, and presentation of material in the draft document. This was an early incomplete draft and it was understood that additional information would be incorporated in subsequent drafts. There was no intent nor expectation that the Panel would close on the draft document at this stage of its development. The Panel was generally complimentary about the content and quality of this draft, while noting the need for considerable development both in structure and content (CASAC, 2000).

On July 23-24, 2001, the Panel met to review the March 2001 draft document, *Air Quality Criteria for Particulate Matter - Second External Review Draft* (EPA 600/P-99/002bB) (EPA, 2001), in a public meeting in Research Triangle Park, NC. Although it was substantially

revised and expanded[1], CASAC could not come to closure on that draft document and has requested that the Agency revise the draft further (CASAC, 2001).

2. CASAC Review of the Third External Review Draft of the PM Criteria Document

General Comments and Overview

A major aspect of the July 18-19, 2002 meeting was a review of the statistical problems that have recently been identified with some of the epidemiological studies, specifically the daily time-series studies of both morbidity and mortality using generalized additive models (GAMs). Most GAM analyses have employed the software pack Splus with default settings, and those default convergence criteria are inadequate for this application. More generally, all GAM implementations now available include a computational approximation which underestimates confidence intervals. The affected time-series studies, which are summarized in Chapter 8 of the revised CD, are central to the quantitative assessment of risk to human health as a result of PM exposure and constitute a key segment of the epidemiological findings. It is not possible to proceed with the closure on the Criteria Document until these statistical issues have been adequately addressed. To this end, a series of invited speakers provided information on the nature of the problems and the approaches to resolve them in appropriate reanalyses. The GAM reanalyses in some cases have found that the estimated risks have been substantially reduced when more stringent convergence criteria are imposed. The more complete modeling of the confidence intervals to take into account the non-linear parts of the model has resulted in the confidence intervals typically increasing by 20 to 30%. The risk estimates have generally decreased in the data sets reanalyzed to date and the confidence intervals have increased. Thus, additional work is needed to ascertain the sensitivity of model results to the details of statistical modeling approaches.

Several difficulties exist in proceeding with the evaluation of the epidemiology, including identification of the studies that are most relevant to the policy decisions that will need be made, identifying approaches by which reanalyses can be completed in a timely manner, and providing a mechanism by which the reanalyses can be adequately reviewed to ensure that the problems raised over recent months have been resolved. It is clearly the responsibility of the original investigators and their project sponsors to ensure the accuracy of their results. However, it is useful for the EPA to facilitate the data reanalyses, particularly for those studies that EPA judges to be most directly relevant to the development of the staff paper. Once the investigators have performed their reanalyses, it will be important to have a mechanism for review of the work to ensure that it has actually resolved the numerical issues that were raised with the original analyses since in many cases, the reanalyses will be reported through unreviewed letters to journal editors or errata. These issues were discussed among the Panel members.

At the time of the PM Panel Meeting, the EPA had not yet completed its review of the studies to ascertain how many and which studies are likely to have the numerical problems with

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1. The EPA assessed approximately 1800 new references published since the October 1999 First External Review Draft (EPA, 1999) was released for CASAC review. This assessment, along with responses to CASAC's earlier comments (CASAC, 2000), were incorporated into the Second External Review Draft.

the estimated values and thus are in need of reanalysis. It was thus not possible at that time for EPA to make informed decisions on how to proceed with the review of epidemiology and the completion of the CD.

With respect to the problem of having an adequate review of any reanalyses, the Panel suggested the possibility of asking the Health Effects Institute (HEI) Review Panel to examine the reanalyzed results to be sure that they had addressed the known statistical issues. We recognize that the HEI panel could not subject the reanalysis results to a full HEI review that includes examination of the original data. However, they could provide a uniform review process that would yield a reasonable degree of confidence that the known problems have been resolved. Since the number and specific identities of the studies needed to be determined and the mechanisms of the review process need to be developed, the EPA staff was requested to prepare a plan that could be distributed to the Panel in early August and reviewed by the Panel in a teleconference to be arranged for the end of August.

Although we recognized that we could not complete the review of the CD and close on the document, we reviewed Chapters 2 to 7 of the April 2002 draft in order to move toward a document that we could close on at the next meeting of the PM Panel. In general the Panel felt that the 3rd draft of the CD is a significant improvement over prior versions. The decision to follow the basic risk paradigm from sources to atmospheric concentrations to human exposure to dose and identification of their health effects (toxicological or epidemiological) provides a much better flow of information. There is still concern that the document is made difficult to read by the detailed reviews of individual papers that particularly occur in the later chapters. We very strongly suggest that an alternative approach be taken in which the chapters provide the integrated summary of the applicable science highlighting what is known, but that the detailed discussions of individual papers that need to be included and reviewed go into appendices to the document (not to the chapter). Such an approach is often taken in National Research Council documents such as the BEIR VI report.[2] Those reports could serve as a model for future criteria documents. In the remainder of this report, the Panel's comments on individual chapters will be provided. The comments of the individual panel members on the chapters are provided in Appendix A to this report.

The NAS paradigm and the CD focus on human health risk which is appropriate. However, it fails to address the issue of ecosystem risk and a risk assessment approach was not part of the ecological assessment. This lack of an ecosystem risk assessment approach is a significant shortcoming in this CD. At this stage in the process, we are not suggesting a major change in approach for this CD and we address the ecological and other welfare issues in comments provided below on Chapter 4. However, in future CD development efforts, it will be important to adopt an explicit risk assessment framework for the ecological effects of the criterion pollutant.

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2. Committee on Health Risks of Exposure to Radon (BEIR VI), National Research Council, *Health Effects of Exposure to Radon: BEIR VI*, National Academy Press, Washington, DC, 516 pp. (1999)

We also note a general problem with the chapter summaries. Panel members made comments regarding virtually all of the chapter summaries with particular emphasis on the summaries from Chapters 5, 6, and 7. The summaries tend to provide too much of a point-by-point recapitulation of the material in the chapters rather than a summary of the key points, particularly those points that are strongly policy-relevant. It should be more evaluative in nature and provide a clearer view of what is known (i.e., the state of the science) regarding the topics covered in each chapter. These summaries would then provide much of the input into the integrated summary chapter where the pieces can be put together into a comprehensive review of key issues related to the setting of the NAAQS for particulate matter.

The Panel agreed with the idea that the document will include all of the relevant literature published by April 30, 2002. If there is a major paper that substantially affects our interpretation of some key aspect of the science being summarized in the CD, it would then be added, but there would not be an effort to add papers published subsequently to April 30. The Panel would then consider the CD on the basis of adequately reflecting the state of the science on airborne PM as of that date.

Chapter Specific Comments

Individual comments from Panelists are included in Appendix A. These comments are substantive as well as editorial, however, they reflect individual points of view and may not reflect consensus views of the entire CASAC PM Review Panel. These extensive comments are provided to give the reader a sense of the overall views on the review of the draft document and to insure that all panel comments are part of the public record.

The Panel agreed that Chapter 2 was substantially improved and could be tentatively closed. There are several ambiguous sections that could be improved. The discussion of the size/chemistry issues is not sufficiently clear in that the fine and coarse modes cannot be fully distinguished by size alone. There can be a tail of coarse crustal material down to sizes of the order of 1 μm while fine sulfate could undergo hygroscopic growth up to 2 μm. Thus, both composition and size are required to separate coarse from fine mode particles. There is uneven treatment and awkward organization with respect to semivolatile components and sampling artifacts. Some discussion of negative artifacts clearly belongs in discussion of semivolatiles like NH_4NO_3 and OC. However, positive artifacts are not, for the most part, a reverse of this. That is, they are typically more driven by chemical reactivity than by physical volatility. Gaseous organics do not need to be "semi-volatile" or even "volatile" (as suggested on pp. 2-41, 30) to react on quartz filter media. Various other positive artifacts such as from reactions of SO_2, HNO_3 or HONO on quartz or glass are also unrelated to volatility. It might be useful to have a specific section focused on sampling artifacts somewhere that is not constrained under the "semivolatile" heading, and in that section, some discussion of both the chemistry and physics of the positive artifacts should be included.

In that section (or somewhere), a discussion of positive NH_4 artifacts needs to be added. With the exception of relatively few, short-term, geographically-limited current or historical PM sampling programs, no routine measurement programs have taken adequate precautions to

ensure that ambient aerosol acidity is not neutralized by reactions with gaseous NH_3 during or after sampling, and so there is nearly always some mass-contributing NH_4 artifact on samples collected under conditions of strong ambient aerosol acidity. Another problem was the uneven treatment in some sections where there were more details of the inorganic analytical and continuous monitoring methods than are needed in body of the text. It would be useful to move these details to an appendix.

The Panel also found Chapter 3 to be much improved and it can be tentatively closed. There are a number of minor items to be corrected that are provided in the individual Panel member comments. As part of the risk assessment planning, EPA's Office of Air Quality Planning and Standards (OAQPS) agreed to use a distribution of background concentrations in their analyses. It would thus be useful for this chapter to provide as best it can, the scientific basis for this distribution. The Panel strongly encourages the inclusion of the 2001 $PM_{2.5}$ monitoring data into the chapter so that it can provide as up-to-date a review of the concentrations across the country as possible. The analysis of the spatial variance may produce more variability because of the limited amount of currently available data. It will be useful to observe the effect of the addition of the 2001 data to the spatial variability analysis. It will also be useful to provide as much of a review of the Speciation Network data as can be developed in the time available.

In the discussions of $PM_{(10-2.5)}$, there is discussion of possible average values, but no indication of the peak values. It will be useful to provide information on the distribution of the $PM_{(10-2.5)}$ values particularly at the upper end of the distribution, to help provide needed information for the Staff Paper evaluation of a possible $PM_{(10-2.5)}$ standard. There are uncertainties as to the relative amounts of primary and secondary organic carbon (OC) and of the variation between primary and secondary OC between the eastern and western United States. This uncertainty in the origin of the OC needs to be stated.

For the remaining chapters, the Panel agreed that they needed to be reviewed again at a future meeting. The revised Chapter 4 addresses many of the concerns offered at the earlier meetings. However, the chapter continues to fall short of being well focused to address the issue of risk due to PM exposure of ecological systems, natural resources and visibility. The committee recommends that the Agency more clearly adopt a risk-based approach in any future efforts, perhaps adopting the guidelines outlined in the SAB report, "*Framework for Assessing and Reporting on Ecological Condition*"[3]. Absent of a clearly articulated approach, it is difficult to link exposure, response, and valuation.

The treatment of visibility is completely reworked from the second draft, and the new version is much better in all respects. However, the third draft still avoids any substantive

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3. A Framework for Assessing and Reporting on Ecological Condition, (EPA-SAB-EPEC-02-009), Ecologocial Processes and Effects Committee (EPEC), US EPA Science Advisory Board, Washington, DC 2002

discussion of the close relationship between light extinction and fine particle mass concentrations. This is a critical shortcoming.

The discussion of visibility in the section on "Atmospheric Turbidity: Effects on Vegetative Processes" is especially weak and out of date (visibility trends are characterized by a paper published in the 1960s). A key issue or question that needs to be addressed (and logically belongs at the beginning of chapter 4 to introduce consideration of criteria for establishing secondary standards) is the extent to which EPA can or should consider the benefits or lack thereof that any secondary standard for PM might have on effects from both precursor pollutants and transformation products. Arguably, nearly the same kinds of integrated considerations might belong equally well in criteria documents for sulfur oxides and nitrogen oxides. If the secondary standards were tuned just right and appropriate control measures were implemented to attain them, we could anticipate that similar improvements in concentrations of SO_2, sulfate aerosol, $PM_{2.5}$, sulfur deposition and visibility might be achieved by secondary standards for either SO_2, sulfate aerosol, $PM_{2.5}$, sulfur deposition, as well as by accelerated implementation of the regional haze regulations.

Since a regulatory program is now in place that provides for protection and improvement for visibility in Class 1 Federal areas, and since this draft CD is focused on "particulate matter" (specifically $PM_{2.5}$ and $PM_{10-2.5}$), the visibility section is missing several key points that would seem necessary if EPA is to consider setting a secondary PM standard to reduce adverse effects on visibility. These include: additional discussion of effects on visibility in non-Class I areas (essentially everywhere other than large national parks) and additional discussion about the relationship of PM to these visibility effects.

There are significant problems with the section on climate change. There is too much material on the general subject of climate change and not a sufficient discussion of the scientific issues related to the effects of ambient PM on the climate. There needs to be a focus on scattering, absorption, and cloud formation and enhancement. The document also fails to adequately reflect recent work on the role of light absorbing particles

With one unfortunate exception, economics has been deleted from Chapter 4 (and therefore from Chapter 9), and an economic evaluation is a key component of setting secondary air quality standards. There are few new relevant welfare effect studies since the last CD (e.g., damages of air pollution on cultural materials, reanalyses of prior visibility studies, and limited other work). If these and past economic studies can be used in subsequent standard setting steps without reference in the CD, omission of economics (or human perceptions and values in general) is acceptable. However, without this literature, there is little in Chapter 4 to indicate the welfare significance to humans of the actual and potential visibility, materials, and ecosystem impacts identified.

The one exception to the omission of economics is the continued citation to monetary values for all ecosystem services (Pimentel et al., 1997; Costanza et al., 1997), and the subsequent discussions on pages139-140. As noted in prior comments, the veracity of these estimates is highly suspect. In addition, the information content from these estimates is

essentially zero in a standard setting process. Regardless of what the total value of all ecosystem services is, there is no particular percent of all ecosystem services that can be even vaguely assigned as being impacted (positive or negatively) by ambient PM (although it appears to be very small, but probably not inconsequential). It would be much more appropriate to value the services that can be identified as impacted by PM. These economic numbers and discussions are far less valid and useful for PM standards than are the omitted economics literature for visibility and materials. Thus, this chapter needs to focus on the primary reason for its inclusion in the CD, which is providing the scientific basis for making decisions with respect to secondary NAAQS.

Chapter 5 is significantly improved over the prior version. However, there is a major problem with the beginning of the chapter in that it does not discuss the relationship between exposure and effects. The introduction and summary sections need to be better organized in recognition of the primary purpose of the CD, i.e., to establish a scientific literature base to be used in the establishment of a NAAQS for PM in ambient air. For example, the paragraph on p. 5-2, lines 3-11, should be moved to line 7 on p. 5-1. In the chapter on epidemiology, most of the studies utilize data relating measured community levels of airborne pollutants to population-based health statistics. Of necessity, they have had to rely on some simplifying assumptions regarding exposures, such as that air pollutant concentrations measured at population-oriented monitoring sites can serve as surrogate indices of the average personal exposure of the population, or that these can be related to the average exposures of selected target populations of interest by suitable adjustment factors such as percentage of times indoors and outdoors, penetration of outdoor pollutants into indoor air, and residence time in indoor spaces. A paragraph summarizing the above should follow the paragraph that should be moved to the beginning of the Introduction as described above.

One unresolved issue that was raised is that of the most appropriate definition of exposure. The CD defines it in terms of concentrations in the breathing zone. The EPA Guidelines for Exposure Assessment (EPA/600Z-92/001, 57 FR 22888-22938; p. 17) state:

> "Under this definition, it is helpful to think of the human body as having a hypothetical outer boundary separating inside the body from outside the body. This outer boundary of the body is the skin and the openings into the body such as the mouth, the nostrils, and punctures and lesions in the skin. As used in these Guidelines, exposure to a chemical is the contact of that chemical with the outer boundary. An exposure assessment is the quantitative or qualitative evaluation of that contact; it describes the intensity, frequency, and duration of contact, and often evaluates the rates at which the chemical crosses the boundary (chemical intake or uptake rates), the route by which it crosses the boundary (exposure route; e.g., dermal, oral, or respiratory), and the resulting amount of the chemical that actually crosses the boundary (a dose) and the amount absorbed (internal dose)."

Although the breathing zone is the primary location on the body boundary, it may be better to rephrase the discussion to more closely match the terminology of these guidelines.

A final concern is the focus of the chapter on mean values and an insufficient presentation of distributions and corresponding uncertainties in exposure characterization. Thus, the extent of occurrences of high exposures and the identification of the population that may be at risk because of these high exposures are not adequately addressed.

The chapter on the dosimetry of particulate matter is significantly improved compared to the second external review draft. The chapter contains adequate documentation of the factors affecting the deposition of particles in laboratory animals and humans. In the review of the dosimetry chapter in prior draft of the CD, the Panel wrote, "The connections could be greatly improved by moving this chapter to follow the exposure chapter, by including illustrative examples of relationships between particle size and regional deposition, and by providing examples of the magnitude of deposited and retained doses resulting from environmental exposures. This information is critical to setting the stage for evaluating how toxicological information might apply to the epidemiological observations in subsequent chapters." The chapter was moved, but it was quite disappointing and frustrating to the Panel that the dosimetric estimations were not made and included. It is important that dose estimates be made to provide the background for the toxicology and epidemiology chapter and make the connection to the exposure discussions of Chapter 5. There needs to be clear connections between the particle size distributions in ambient air and regional dose. This provides the basis for an adequate understanding of the toxicology chapter.

In our view, it would also be useful to show dosimetric comparisons between laboratory animals and humans. Such information would help put the animal toxicology studies in perspective to human exposure levels as part of judging the relevance of the animal results for potential risks to humans from exposure to PM. It is also important to provide dose estimates for exposures to experimental animals to identify when there may be situations in which there are overload conditions.

Another general comment we have concerns Section 6.2.3.2 on age-related deposition of particles. The individual studies described in this section provide a significant amount of data on age-related deposition of particles, but one has difficulty in extracting general conclusions on the effects of age. Inclusion of a figure that depicts the results from the various studies would assist in evaluating whether trends as a function of age can be determined for deposition as a function of particle size. Clearly, particle size is critical for whether or not there are age differences. This should be pointed out in section 6.2.3.2 and also carried forward in the Integrative Synthesis chapter. There also needs to be the connection between interindividual variability in deposition and susceptibility. One reason why individuals may be susceptible is that they obtain a higher dose for the same exposure.

It is the Panel's opinion that the discussion of clearance in the chapter is inadequate. There is considerable information available on clearance mechanisms, and it has been incorporated into the dosimetric models. Thus, there is adequate data to model clearance. There is also inadequate coverage of deposition hot spots and the fact that deposition is not uniform in most of the functional regions in the respiratory tract.

There are many instances of repeated references to the same work with repeated descriptions. The use of appendices with appropriate reference to the careful review of the papers being placed in the appendix would provide a smoother flow of information and an easier to read chapter.

The organization of Chapter 7 on Toxicology is not helpful to the flow of information. The current version focuses on particle types and their effects rather than highlighting the kinds of effects that can illuminate the epidemiology and go forward into the integrative summary to provide a picture of why particles cause death and disease. There should be care taken to indicate the lowest concentrations at which effects are observed as well as providing estimates of doses for each study.

An area that needs additional work is the section on cardiovascular and systemic effects of PM. The current chapter contains only two references, post-2000, for cardiovascular and systemic effects. The authors need to ensure via a comprehensive literature search that all relevant material on this topic has been included, particularly since the epidemiology studies are increasingly finding the potential for cardiovascular and cardiopulmonary effects from exposure to PM that relate to acute mortality and morbidity.

Now that the ACS cohort follow-up study (Pope et al., 2002) has shown excess lung cancer in relation to ambient average levels of $PM_{2.5}$, there will need to be some reintroduction of the toxicological data based on PM-induction or promotion of lung cancer. This need not, and should not, mean the restitution of most of the discussion of mutagenesis and high-dose cancer exposure studies from the 2nd draft, but rather a selective discussion of mechanisms (e.g., mitogenesis) that may account for PM associations with lung cancer in humans.

In all of these chapters, the current draft was a clear improvement over the prior draft. It appears that with careful attention to these major points and the individual comments provided in the appendix, it will be possible to come to closure on an acceptable CD at the next meeting assuming that we can come to agreement on the epidemiology and integrated summary chapters. Some comments were provided on these chapters and they are included in the appendices. As the epidemiological chapter is rewritten, it may be useful to have a teleconference to review the revised Chapter 8 so that the next draft version of this chapter will be sufficient to permit closure at the next Panel meeting when the revised Criteria Document will be reviewed.

With respect to Chapter 9, the NRC committee paradigm is presented, but the chapter is organized to address the PM Committee's questions rather than its paradigm. They are not necessarily the appropriate organizing theme for an integrated summary of what is known about the issue of PM and public health. The paradigm, however, is the relevant integrating framework. Thus, the chapter should follow the paradigm and not the task questions. However, since the PM committee's focus was on particulate matter and human health effects, the paradigm needs to be expanded to include an appropriate discussion of ecosystem effects and the potential basis for secondary standards to protect public welfare.

We commend the EPA staff for the effort and attention to detail in preparing the current draft Criteria Document. We appreciate the opportunity to review this document and provide our comments and recommendations. We look forward to your response to our advice.

Sincerely;

/ Signed /

Dr. Philip K. Hopke, Chair
Clean Air Scientific Advisory Committee

Appendix A - Final Comments from Individual Panel Members

Appendix B - Roster and Bios of the CASAC Particulate Matter Review Panel

Appendix A - Final Comments from Individual Panel Members

This appendix contains the final written comments of individual members of the Review Panel who submitted final electronic comments. The comments are included here to provide the all suggested edits, a full perspective, and range of individual views expressed by panelists during the review process. Preliminary comments provided by panelists are not included here. These comments do not represent the views of the Review Panel, the Clean Air Scientific Advisory Committee (CASAC), the EPA Science Advisory Board, nor the EPA itself. The consensus views of the Review Panel and the CASAC are contained in the text of the report to which this appendix is attached. Panelists providing comments are listed on the next page and their individual comments follow.

Dr. Philip Hopke

Chapter 2

Page 2-1, line 13: Mixture of mixtures concept: primary mixtures characterizing sources vs. secondary mixtures arising from atmospheric chemistry.

Page 2-2, line 25-26: From what we have seen so far from the single particle mass spectrometer work, most particles are part of external mixing. It is only when processed in fog, that primary mixtures are observed.

Page 2-3, line 2: Many brownie points from the chair !!!

Page 2-3, line 23: "...that of a unit density sphere": Normal definition of equivalent diameter is that a sphere of the same volume and same density as original. Aerodynamic diameter is for a sphere of unit density with same settling velocity.

Page 2-3, line 30: "...Friedlander [1977]": Use new version, 2000.

Page 2-6, line 4: "However, the nuclei mode...": True nuclei formed by homogeneous nucleation are 10 to 20 nm in size. We really should define this intermediate size as the Aitkin mode or not use "modes" when they are no agreed upon standard.

Page 2-7, line 2: "...have a small accumulation mode and no observable nuclei mode.": NOT TRUE! Aitkin mode particles have been seen in a number of rural settings like the Great Fell Dun. It would be good to get more up to date references here. Look at Kulmala's results for rural Finland or results at Mace Head.

Page 2-10, line 17: "...diameters below about 0.1 μm.": Too broad a definition - see earlier discussion.

Page 2-10, line 21: Delete "nuclei mode". Line should read "size range <0.1 μm. Aerosol physicists and material scientists tend to use the term".

Page 2-14, line 3: "...to collect the entire coarse mode...": Noll impactor?

Page 2-16, line 6: How about calling the PM (10-2.5) "thorasic coarse"? It is less awkward than PM (10-2.5).

Page 2-16, lines 13,15,17: Take out "nuclei mode" as equivalent to ultrafine.

Page 2-17, line 4: "vapor pressure above a flat surface...": at a specified temperature.

Page 2-17, line 22: "...of existing particles or can **nucleate to** form new particles." Add "nucleate to".

Page 2-17, line 25: "...available particle surface area is **generally** sufficient to...": insert "generally". Nucleation events have been seen in a number of cities including Helsinki, Pittsburgh and Rochester. It would be useful to put in a sentence or two on this.

Page 2-18, line 2: "(Clarke, 1992)" reference is again rather out of date. Look at recent work of Kulmala and O'Dowd, for example.

Page 2-18, lines 19-25: It is very weak. It requires at least ternary nucleation H_2SO_4, H_2O, NH_3 to produce sulfate nuclei and maybe something else (Kulmala's Nature Paper).

Page 2-19, lines 7-15: This is good, but needs references.

Page 2-20, line 7: change 1977 to **2000**

Page 2-20, line 25: "Some **primary** organic materials such as...": insert "primary"

Page 2-24, line 12: "...(. 40% RH)" should read 35 ± 5 , if I remember 40CFR50 correctly.

Page 2-27, line 16: Diffusion of NH_3 to droplets determines acidity. Need to begin making the case that we need to know about NH_3 sources.

Page 2-27, line 28: Where is the discussion of NO_3^- replacement as SO_2 controls come into place?

Page 2-31, line 28: "...PM monitoring may be anticipated".: I think there are already have been improvements including single particle mass spectrometry, steam-injection collection systems, differential TEOM systems, etc.

Page 2-34, line 1: "...designated RH (40%)": check 40CFR50 for specified RH.

Page 2-43, line 6: Differential TEOM Systems, Patashnick et al., Jan. 2000 issue of Aerosol Science and Technology.

Page 2-56, line 2: "Recent experiments...": Four years ago is not recent.

Page 2-75, lines 1-11: An important part of this discussion is missing - the potential chemical interactions between the typically acidic fine and basic coarse particles. It also does not provide a separately coarse sample for analysis.

Page 2-77: Virtual impactor provides an opportunity for development of a continuous monitor with all of its accompanying benefits.

Virtual or real impactors provide samples of coarse for chemical and biological characterization. Needs to be more clearly stated as an advantage.

Need to discuss slope of the separation curves. Virtual impactors are not as sharp**,** but have improved since original Loo design.

Page 2-78, line 22: What species?

Page 2-78, line 31: Not really 5040, since it has been modified to be done in a shorter time, etc. This should be spelled out as to just what the protocol is.

Chapter 3 - Concentrations, Sources, and Emissions of Atmospheric Particulate Matter

Page 3-4, line 27: "Et al., 2001)**, as discussed extensively in Chapter 2.**": add portion in bold.

Page 3-5, line 4: Soil OC will also be important. There will be humic material associated with wind blown soils, particularly in the east.

Page 3-5, line 8: There is apparently s recent Australian paper that suggests some pollens, when exposed to water, break into smaller (respirable) sizes. The guy who published this currently is with Rick Flagan. It would be useful to get this in here.

Page 3-12, line 1: "The first definition, **(Definition 1),** refers to the..."

Page 3-12, line 3: "Definition, **(Definition 2),** refers to the..."

Page 3-27, lines 8-12: The FRM is also subject to artifacts and their presence for continuous monitors should not be over-emphasized in this manner. We should use caution interpreting the FRM results if we care about what the actual PM concentrations are.

Page 3-28, line 6: One does not always have inversions, but we do generally have a lowering of the mixing height.

Page 3-33: There is a much richer set of Phoenix data from a dfpss that Gary Norris gave us for the UNMIX/PMF shootout 7900 samples.

Page 3-44, line 14: "...coefficient of divergence **(COD)** are used here...": inert bold text. It would be useful to provide the equation by which this is calculated here instead of page 3-50.

Page 3-50, lines 17-20: This stuff should be at the point where COD is first used.

Page 3-59, line 12: "...is secondary sulfate and nitrate formed...": Sulfate is higher in summer, but although more HNO_3 may be formed in the summer, we see ir peaking in winter when the temperature us conducive to forming NH_4NO_3.

Page 3-59, lines 28-29: "..larger fraction of PM mass than nitrate".: True in LA and the San Joachin Valley, but much less so throughout the rest of the west. Certainly not in eastern Washington.

Page 3-68, line 30: Is there a comparable set of criteria for Asian dust episodes?

Page 3-74, line 8: All ergenvector-based methods, including UNMIX, have this problem. There is no way to data point weight in an ergenvector analysis (Paatero and Tapper, 1993).

Page 3-80: There needs to be an emphasis on developing better organic sampling / analysis methods. We also need more source characterization. The chapter does nor adequately address how well we have or have not met Task 3 of the PM Committee's revised task (Burning Book 2) since summary chapter will be based on the NRC tasks, it needs to be addressed here.

Chapter 6 - Dosimetry of Particulate Matter

Page 6-3, lines 9-20: This is already in Chapter 2. Do not duplicate here. Refer to appropriate sections.

Page 6-3, line 18: Too simplistic since aerodynamic diameter is not meaningful for particles < 100 nm.

Page 6-3, line 25: "In linear form...": What is this supposed to mean??

Page 6-3, line 26: Replace **logarithm** with **geometric**

Page 6-4, line 5: Since it is individual particles that deposit one at a time, it really is more useful to discuss count distributions.

Page 6-11, line 7: Was anything new found that was not in earlier results of Christine Schiller? We do not need to keep repeating known results.

Page 6-22, line 5: In a truly compromised lung, morphology of the airways could be very different and this, these estimates are highly uncertain.

Mr. Richard Poirot

I wanted to submit some additional CASAC comments on the PM-CD, specifically relating to some of the issues Warren & I raised about the visibility section of chapter 4. I expect to have these done early next week, but also want to have Warren & a few other smart guys have a look before calling them "final".

Basically I think that - especially with the regional haze regulations now>in place to "protect" class 1 area visibility - there is strong justification for EPA to (at least) consider a secondary (short-term) PM-2.5 standard for visibility protection elsewhere (in urban areas, for example). With the class 1 areas covered by haze regs, and with a focus on short-term (24-hr) max, the traditional "East is way different from West" argument that has been used to avoid consideration of 2ndary PM standards before pretty much goes away (see attached).

To consider this, a number EPA needs, but have not provided "criteria" for in the CD, is the relationship between FRM-defined PM-2.5 mass and visibility or Bext. My best current (& somewhat flakey) approximation is 6.8 m2/g (based on comparison of past 5 yrs of all IMPROVE sites constructed extinction & measured fine mass - see attached), though that feels a bit high to me. Using 6.8 m2/g and a nice round number of 10 mile visibility (quite hazy, but the level above which NWS considers aviation visibility "unlimited"), would work out to a 24-hr PM-2.5 "urban visibility" 2ndary standard of about 35 ug/m3 - which might even pass the laugh test... Granted, it ain't our job to suggest a level of standard or even do the work upon which it might be based, but I'd like to have some feel for how the numbers might work out in advance.

General Comments

This review draft is to a large extent a “supplemental document”, providing additional (& considerable) detail in a number of specific focus areas that have been identified in previous reviews of earlier drafts. As such, it does not attempt to be comprehensive, and refers back on many occasions to earlier review drafts for details on other topics. Generally, the authors have been diligent in clearly identifying up front the specific areas covered (and not covered) here, and in adding frequent referrals back to sections of previous drafts. Nevertheless, the supplemental nature makes for an unavoidably awkward writing, reading and review process. This “iterative” CD process, in turn, adds complexity to the necessary linkages among the various sections – in the CD and also among the various groups and groups with expertise in measurement methods, air quality data analysis, emissions characterization, atmospheric chemistry & physics and the associated direct and indirect effects on human health, welfare and environment.

This leads to a second general comment which is that the different sections here are not always very well coordinated or linked together. To a large extent this may also be unavoidable – given for example the long time periods required for some of the effects analyses vs. the continually evolving measurement technologies. Today’s health study is based on yesterday’s state of the

science air quality measurements. Still, I think it would be useful to the CD document, the underlying comprehension process and the subsequent standard-setting process if more cross references among sections could be added (the chemistry & physics indicate these potential problems with measurements; the measurement constraints indicate limitations on identifying space, time and compositional patterns; the perceived exposure patterns indicate these limits on effects studies, etc.). Similarly, I think many inconsistencies among sections could be ameliorated if authors of one section were asked to read & review the other sections. We have already become too specialized, yet the problem grows…

Overall, I found the executive summary and first 4 chapters to be well organized, clearly written, comprehensive (for the focused topics they addressed) and accurately reflective of the current scientific understanding of these subject areas. In combination with additional details provided in the earlier drafts, this should provide a sound basis for development of primary and secondary standards for PM-2.5 and PM-10-2.5. All my comments are relatively minor, and would not require any major revisions.

E. Executive Summary

A good, balanced summary reflective of the chapter content and specific areas of focus in this CD revision/update. Clearly written, as are most sections, and overall requiring no major revisions I can think of.

E-3, line 28: suggest inserting "primarily" after "formed".
E-4, 8: suggest adding "and shape" after "density".
E-7, 2-5: suggest reversing the reasons, putting 'determine effects' before 'guide to attainment' before 'determine attainment'.
E-8, 6: "possible" could be deleted. Retaining some water is (certain and at least) just as likely as losing some OC…
E-9, 9: (see also general comments on chapter 3) A line could be added here like "Conversely, it can be observed that standard political geographical units like MSAs and counties often represent illogical definitions of coherent "communities" from an air quality perspective".
E-19, 25: Can we find a better word than "interesting"?
E-22, 8: "regional sulfate" is not really a "source category". Might add "(primarily from coal combustion)"
E-26, 10: Could modify to "Atmospheric deposition of particulate matter and its precursor gasses can alter…"
E-30, 8: "affects" should be "affect".
E-31, 22: I think you mean "high Al/Ca ratios" (not high Ca/Al).
E-32, 4: "naturally occurring cultural materials" means what? How about "building materials, historical artifacts and cultural works of art".
E-34, 4: Could add "the incident solar radiation" before "angle".
E-34, 16: No clue what "less of both types of information" refers to here.
E-35, 6: Could either delete "Oregon" or change "Pacific Northwest" to "Washington State".
E-35, 10-11: Could add "in rural areas" after "insignificant", and then add at end of sentence ", although larger proportionate contributions are likely in urban areas in all regions".

1. Introduction
Very logically organized and clearly written, this section presents a good review of the history and describes a clear, logical justification for the specific topics of focus in this "supplemental" CD. The direct responsiveness to previous CASAC, NRC and public comments on earlier drafts is excellent!

2. Physics, Chemistry and Measurement of Particulate Matter
Another excellent section with helpful diagrams, good detail, appropriate inferences and summarization.

This is a off-center point, but somewhere - maybe in or with reference to the health effects section(s) - I think it would be helpful to provide a clear explanation and justification for exactly why its important to exclude water from consideration of PM mass. Maybe this is quite obvious, but if it is, then a clear justification should be easy.

A somewhat awkward organizational concept (there are lots of these that are unavoidable with PM) is the relationship between "semivolatiles" and "sampling artifacts". Some discussion of negative artifacts clearly belongs in discussion of semivolatiles like NH4NO3 and OC. But positive artifacts are not, for the most part, a reverse of this. That is, they are typically more driven by chemical reactivity than physical volatility. Gaseous organics do not need to be "semi-volatile" or even "volatile" (as suggested on 2-41, 30) to react on quartz filter media. Various other positive artifacts such as from reactions of SO2, HNO3 or HONO on quartz or glass are also unrelated to volatility. Possibly it would be useful to have a specific section focused on sampling artifacts somewhere that was not constrained under the "semivolatile" heading, and in that section include some discussion of the (more chemistry than physics) positive artifacts. In that section (or somewhere), you might also include some discussion of positive NH4 artifacts (I don't see mentioned anywhere). Excepting the relatively few, short-term, geographically limited current or historical PM sampling programs (and certainly including the current EPA STN program which presumes incorrectly to quantify NH4) no routine measurement programs have taken adequate precautions to assure that ambient aerosol acidity is not neutralized by reactions with gaseous NH3 during or after sampling, and so there is nearly always some mass-contributing NH4 artifact on samples collected under conditions of strong ambient aerosol acidity.

Section 2.2.6 includes a concise summarization of elemental analysis techniques. However, there are few if any comments on the relationships between the desired sample type (TSP, PM10, PM2-5, etc.), specific analytical methods, the various filter media, filter physical characteristics, sample volumes and sample preparation techniques (aqueous acid extractions, etc.). I don't think a lot of detail is needed here and the ground is generally well covered in the (2-80, 30) cited Chow, 1995 paper. However some additional discussion is warranted (& might lend itself to a tabular summary). Otherwise the detail on the analysis methods alone is unnecessary/ unbalanced. Other good reasons are that selected analytical methods are often a matter of convenience rather than intelligent study design, and various mismatches persist (such as continuing attempts to quantify "toxic metals" on elementally filthy fiberglass or quartz filters).

2-1, 11 “particular” should be “particulate”.
2-2, 16 “hundreds” may be an understatement, since in studies where hundreds have been identified, these typically represent only about 10% of OC mass.
2-24, 18: Could insert “Other” before “semivolatile”. Water is also semivolatile.
2-26, 2: Could replace “may” with “can” or just leave it out. They do travel farther…
2-32, 12-19: A possible 7th design criteria could be “(7) Operation and maintenance procedures needed to sustain consistent measurements over time”. Also in line 13, “upper cut point” could be changed to “particle cut size characteristics”.
2-36, 15-16: Why is “low RH” a necessary condition for smaller “coarse mode” particles to be included as $PM_{2.5}$?
2-79, 1: “samples” should be “samplers”, and it might be useful to clarify what “cut point comparable to the WINS” means. How comparable? The 50% cut points are?
2-79, 19: PIXE is defined as “particle-induced…” rather than the “proton-induced” definition that is used more commonly throughout the document (on the next page for example). It would be good to be consistent and I recommend “particle”, as it (is safer) includes protons but not vice versa, and other particles (alpha for example) have occasionally been employed in some of the various “PIXE” measurement efforts you cite. Particle is also used in the IJPIXE Journal…
2-88, 8: “5% or less” of what?
2-97, 14 (or elsewhere in the discussion of “black carbon” techniques). It would be useful here to include some discussion of how filter loading or other characteristics of the sample can influence results from these methods. On a heavily loaded filter, some LAC may lie under / be masked by the surface deposits, etc.
2-99, 12: Could replace “or” with “and/or”. Naphion dryer and 30 degree C heater are (& should be) more commonly employed together than separately.

3. Concentrations, Sources and Emissions of Atmospheric Particulate Matter
Generally also a strong, coherent section, I think this chapter may appear “less than it is” since the general topic was relatively well covered in the ‘96 draft – from pre-existing, regionally relevant data - like IMPROVE. Conversely, there hasn’t been much time for published analyses of the more recently available (where “available” is a dubious concept) PM2.5 mass, continuous & speciation data from denser, more-urban networks.

Despite these reasonable excuses, a general criticism is that the information provided on space/time/ and relational patterns is relatively incoherent. An editorial comment, but one strongly felt, is that the funded efforts to analyze data collected in “routine” networks lag substantially (and increasingly) behind expansion of mandatory monitoring networks. I think this shows up clearly in the relatively limited discussions and absence of coherent summary information on the space/time & relational patterns discussed in this chapter. The “traditional “county maps (as in Figures 3-1 – 3-6) do not work for me. They convey little useful information, and regardless of whether they are “really easy to make”, have some (illogical) “regulatory context”, are used (illogically) in epi studies, are based on spatial units with other relevant information, or are “traditional”, they are still profoundly bad ways to convey spatial air pollution information. The concept that a neighborhood, city, MSA, county, state, etc. is (or is not) “represented” because there happens to be (or not) a sampler somewhere within the same political jurisdiction is wrong. Scientifically, our understanding of space/time patterns of most

PM metrics is substantially more advanced than what is conveyed here (see for example presentations from EPA's 12/3-5/01 Spatial Data Analysis & Technical Exchange Workshop, as well as the many, varied results from EPA in-house OAQPS, regional, state, local and contracted analysis efforts which have been developed in support of recent EPA-initiated discussions on "network-re-design" / "NCORE", etc.). We know better, and a sad irony is that "routine" labor-intensive, mandatory monitoring networks – justified primarily by an alleged need to determine compliance status, but more logically (and increasingly) justified by serving dozens of secondary important objectives (understanding "one atmosphere", support for future health studies, model validation, etc.) – these data accumulate and go largely un-analyzed. The ratio of resources expended to collect data vs. resources expended to analyze it is way out of balance and heading in the wrong direction. OK, off the soap box now, I don't think the above ranting represents a significant inadequacy in the CD – but more that a logical conclusion from the spatial pattern review is not that none exists, but rather that looking within political jurisdictions is a poor way to see it. Last point on this is that a seasonal stratification would have been useful in elucidating the spatial variance (i.e. consider the spatial & seasonal patterns together at some point, rather than separately).

3-4, 16-17: The indicated range of unidentified material – ranging from 23% in the East to 0% in the West – seems overly simplified. Was 23% the average in the East or the maximum? I assume 0 was the minimum and not the average for the West. Can these generalizations be more qualified ("at some sites in the East" or "at arid western sites" etc.)?
3-4, 21-22: Doesn't the 1.4 factor also include assumed hydrogen?
3-11, generally: It might be helpful to include here a brief summarization of what emission categories were controlled over the trend periods.
3-12, 3: I assume "only" could be moved for clarity to before "natural", otherwise the phrase "only within and outside North America" pretty much covers everything.
3-13, 25 (and following pages through 3/21). This discussion, largely based on the single Pinto (2002) analysis includes much interesting detail (maybe too much), but not much interpretation. One general suggestion is to employ consistent use of past tense in referring to how the data from these few years "were". It should not be implied that this indicates how the (current) concentration patterns "are".
3-18, 10: I'm not sure what it means (or if its true) that "wood burning… is practiced more widely in the Western United States than in the East". But in any event it might also be added that "prolonged local stagnation events are also more common during winter in Western mountain valleys than in most sections of the East. Hence winter maxima…"
3-21, 2: Are the negatives only evident in Detroit? If so, can some interpretation of likely causal factors be added?
3/22, 11: Lowest values after rain storms (but not during?). How about some interpretation here. Would be a nice link to the eco-effects section to follow.
3-28, 9: Some explanation of the meaning of the "98th percentile for positive and negative excursions" would help.
3-29, 21: I cant really "see in Table 3-1" that ratios exceed 1 for "a few hundred measurements". Maybe it would help if you indicated what %s of days were negatives in the regions.
3-33, table 3-3: It seems somewhat unusual that S is the largest mass contributor listed, but has one of the poorest correlations with mass. Presumably this is due to much higher OC or NO3

not included in table. Maybe worth a comment. Also in the coarse fraction, Al is > 3x Si, which seems strange (ratio is reversed and more logical in the PM-2.5). Could this be a typo?
3-36, 23,25: How can ammonium range from 0 to 9%, but contribute 33% at one of the sites?
3-37,18 (and following 10 pages): As in the discussion of seasonal patterns, much of this spatial variability discussion is based primarily on the one Pinto (2002) analysis which was an excellent work but also tightly focused in its methodological approach. As in the preceding section, there may be more (rambling) detail here than we really need, and less interpretation than we want. Generally, this analysis seems to shed much more light on spatial variability within the specific MSAs than it does on any regional differences (between MSAs). There are several cautions that MSAs with poorest CODs etc. often have sites way out of town, in different air sheds, etc., which begs the question of why they were included? A caution on the inconsistent use of AIRS land use codes among MSAs makes one wonder what their purpose is or how they could be improved. Given all the noise in these data, a conclusion might emphasize more the inappropriateness of the political MSA boundaries as organizational units, and less the concept that sites with different siting criteria can show different concentrations even over short distances. As with the seasonal section above, stratification of spatial analysis by season might have been very illuminating. Basically, I just don't believe that spatial correlation does not increase with decreasing distance between sites, and think that if your approach and metrics show otherwise, you need to rethink the approach to the query.
3-41, 14: Was glad to see these metrics expressed for collocated samplers, as the COD and other metrics used here are not intuitively obvious. Too bad though it was for only one site, Steubenville, with presumably high sulfate mass fraction not prone to sampling artifacts. It would be interesting to see more collocated results from other areas – especially those like LA where the apparent local spatial variability is greatest, and where high semivolatiles might lead to poorer sampling precision..
3-41, 26: reverse "r of".
3-47, 16-17: I think "flatter terrain in the East" is overly simplified explanation. Emissions density patterns, local stagnation, and much different chemical mixes of sources of PM, precursors and oxidants are also important.
3-50, 4-6: An explanation for this anomaly might be that coarse soil emissions increase with wind speed (and dryness), which tend to persist over fairly large spatial scales.
3-55, 18: What about OC:EC ratio as an indicator of potential secondary OC? Note also this indication that most OC in LA is primary; elsewhere we learn that OC is typically the largest mass-contributing species in urban areas; but in many other places in the CD we find unqualified generalizations that "PM-2.5 is a secondary pollutant".
3-56, 22: "By and large, however, most ambient PM-2.5 has been formed in the atmosphere" illustrates my point in preceding comment.
3-59, 11-12: "substantial fraction of the mass, especially during the warmer months, is sulfate and nitrate". Nitrate is very predominantly a fall-winter pollutant. Generally some of the writing at beginning of section 3.3 is a bit careless at times.
3-59, 21: These can & do also react with primary fine particle material too.
3-59, 29: Is the NH3 really higher in the West? I think its higher in the North Central (hog belt) region, but not particularly different between East & West.
3-69, 8,9: Its not obvious what spatial scales or averaging times the cited %s refer to.

3-73, 6: You could add "and eastern" after northwestern Canada (especially after last week!), and actually that area east of James Bay, Quebec is a chronic fire area – I can provide some references if needed.
3-76, 20: This seems to either emphasize the exception or to respond to a false or contrary premise that hasn't been stated. It might be better to indicate (if true) that "in some cases gasoline vehicles made the dominate contributions to PM-2.5, while in other cases diesel emissions appear to predominate". There are certainly other references that show a major contribution from diesels.
3-74, 3,4 (& following page): A nice concise summary of the PMF & UNMIX models here, but far as I can tell, no results from these models are cited. There have been several publications in the past year or 2.
3-80, 29: "22 studies" out of how many?
3-82, 4-18: This "short spikes are local" argument sounds logical, but I think the conceptual model is too simple. Other factors include the wind speed & persistence of the wind direction both at the source and at the receptor. I've often noted relatively short duration (few hour) spikes from relatively distant (> 500 km) sources under conditions of high wind speeds and rapid directional shifts.
3-84, 1: At some point, EPA needs to deal with this inconsistency in (fugitive dust) emissions and air quality. Dust raised by MV on paved & unpaved roads accounts for 60% of the emissions in the "fugitive" category. These do not necessarily require high wind speeds, but just cars (& especially trucks), which create their own "micro winds" that die off almost instantly. This emissions category would benefit greatly from development of adjustment factors to reflect that x % of these cold, street height emissions don't really go anywhere.
3-86, 4: not all oxidation of SO2 is "photochemical", and the term could be deleted.
3-87, table 3-13: It would help to tell what measurement (& emissions) data these % changes are based on. What do SO4/SO2 and NO3/NOx mean? Ratios or averages? Some discussion/explanation for why the SO4 & NO3 concentrations have improved faster than the emissions would be helpful. Maybe this is the logic behind Bush's clear skies plan – that the air will get better before we reduce the emissions…
3-88, 24: add "and" after cases.
3-91, 27,28: lower correlations of what? Spatial correlations?
3D-25, 18: Suggest adding "It should also be emphasized that these global estimates of fractional contributions from natural sources will tend to shift strongly toward anthropogenic sources over densely populated, industrialized areas like North America."

4. Environmental Effects of Particulate Matter
There are two general features of this chapter which I think can be emphasized as highly commendable efforts, but which also lead to problems. To a greater extent than most other chapters, there are many attempts here to link discussions of ecological effects to complex aspects of particulate matter that are treated in other chapters. For example, discussions of particle deposition are linked to details on particle chemical composition, size distributions, source characteristics, co-emitted or confounding pollutants and precursors, and emissions control technology effects on all of the above. Ecological response to particle-induced modifications of solar radiation are linked to discussions of human perception of visual air quality, etc. Unfortunately however, many of these cross linkages seem relatively independent

from, or ignorant of, more detailed information on these topics discussed in other sections. For example the discussion of visibility in the section on "Atmospheric Turbidity: Effects on Vegetative Processes" is especially weak and out of date (visibility trends are characterized by a paper published in the 1960s). Similarly, several discussions of the trace metal characteristics of different source types are inaccurate, outdated or both. I think these aberrations or inconsistencies are more embarrassing than harmful, and will not lead to illogical considerations of appropriate criteria for establishing secondary PM standards. In addition, much of the ecological effects discussion presented here could just as logically be included in subsequent criteria review for other pollutants such as sulfur or nitrogen oxides.
This last point leads to a second area where the chapter represents a commendable effort with associated problems. There seems to be some inconsistency or indecisiveness about whether a discussion of ecological (or welfare) effects of particulate matter needs to be limited to concentrations and deposition of particulate matter per se (i.e. will only consider "dry" particle deposition in particle form); or whether it can/should extend to other atmospheric removal mechanisms for PM (the component of wet and occult deposition which can be attributed only to pollutants which had been in particle phase); whether deposition or co-exposures to gaseous precursor pollutants should also be considered (SO2, NOx, HNO3, VOC); and finally whether confounding effects of other pollutants should also be considered (ozone may predispose plants to injury from acid deposition, acidification of surface waters increases Hg toxicity, PM influences on UV-B radiation or global climate change, etc.). There are inherent organizational difficulties associated both with treating the complex mixture called PM as a single pollutant, as well as with evaluating PM effects in isolation from those of their precursors or transformation products. These organizational issues are unavoidably magnified in a consideration of ecosystems - complex systems that are complexly affected by a cumulative integration of all of the above.

A key issue or question that needs to be addressed (and logically belongs at the beginning of chapter 4 to introduce consideration of criteria for establishing secondary standards) is the extent to which EPA can or should consider the benefits (or disbenefits) that any secondary standard for PM might have on effects from both precursor pollutants and transformation products. Arguably, nearly the same kinds of integrated considerations might belong equally well in criteria documents for sulfur oxides and nitrogen oxides. If the secondary standards were tuned just right and appropriate control measures were implemented to attain them, we could anticipate that similar improvements in concentrations of SO2, sulfate aerosol, PM-2.5, sulfur deposition and visibility might be achieved by secondary standards for either SO2, sulfate aerosol, PM-2.5, sulfur deposition, as well as by accelerated implementation of the regional haze regulations or by an additional roll back of allowances under the Title IV emissions cap and trade program. Possibly this last option would be the simplest, and most cost-efficient, but EPA lacks congressional authority for additional allowance reductions, nor is there any specific EPA group with affirmative responsibility to evaluate benefits of specific future emissions control strategies, unless such strategies are warranted by specific mandatory regulatory programs such as for the attainment and maintenance of air quality standards.

From a scientific perspective, these regulatory and policy constraints are silly, but more importantly they interfere with the ability to develop sound criteria upon which to base decisions

for secondary standards. Regardless of whether a comprehensive discussion of combined effects (and benefits of control) of SO2, NOx, VOC, PM-2.5 and their precursors or transformation products belongs most appropriately in this CD or others, it belongs somewhere. So I am pleases to see some consideration of these related pollutants and effects in this CD and encourage expansion of these more comprehensive criteria in the future.

4-1, 14: In light of above comments, I suggest adding ", and/or for assessing the ancillary benefits of control strategies developed to reduce adverse effects on health, welfare or environment."
4-2, 23 (or elsewhere early on): I suggest addition of a simple introductory paragraph like: "Airborne particles, their precursors and their transformation products are ultimately removed from the atmosphere primarily by wet and dry deposition processes. This atmospheric cleansing process (fortunately) prevents the long-term build-up of lethal concentrations of these pollutants in the air and moderates the direct human health effects associated with their direct inhalation. Unfortunately, these deposition process do not permanently remove these pollutants from the environment but merely transfer them to other environmental media, where they can and do have adverse effects on the structure, function, diversity, and sustainability of complex ecosystems."
4-5, 26 (or elsewhere): Though additional effects of gaseous precursors are directly considered in other sections of this chapter (4.4.1) or are unavoidably included in consideration of effects from wet deposition in various other sections, the dry deposition of gaseous precursor species, predominantly SO2, HNO3 and NH3, are deliberately avoided here. The detail on particle-only deposition included here is excellent, but I think, for completeness, it should be expanded to include the gaseous precursors. Any emissions control strategy to reduce PM-2.5 concentrations is likely to involve these precursor emissions, but these or other emissions control (or increase) strategies may also affect the transformation rates and consequently the gas/particle distributions and depositions of these pollutants and so should be addressed in the CD.
4-30, 16: Add "and particulate" after gaseous.
4-35, 5: Could add "or sea salt" after aerosols, or change to "coarse alkaline crustal material or sea salt". Sea salt isn't always alkaline & can become acidic at higher RH.
4-40, 25 (& elsewhere wherever trends are discussed): This (historical & continuing) response of deposition (or air quality) to emissions is a critically important concept and could be further emphasized wherever possible. Of particular interest is the linearity of the response (which I think has by and large been the case for S), although conditions and locations where the response is or may become non-linear should also be discussed.
4-40, 28: I think chemically, at least there has been some improvement observed more recently in some aquatic systems. Bourne Pond, in the Lye Brook (VT) Wilderness area was featured as showing some chemical (pH) improvement in a recent NAPAP report to Congress. Ironically, the Lye Brook IMPROVE site shows a regionally anomalous non-trend in sulfate aerosol concentration…
4-41, 7: "Regardless of the cause, …" I'd like to see someone develop a coherent explanation for the decline in base cation deposition, although maybe this CD is not the place. Also, I'm not sure the trend (cited here to Driscol et al., 1989) has continued through the present.
4-42, 17,18: Agreed, the lead decline is most impressive, especially from the first 2 years in the mid 70's. However, I dispute that "most of the heavy metal species have remained relatively constant over the 16-year period. If I divide these data into 2 roughly equal halves (1975-83 and

1984-91) and calculate the percent change from the first half to the second half, there are improvements in all species, including 35% (Mn), 41% (Ni), 44% (Cu), 61% (Zn), 70% (Fe), 71% (Cd) and 77% (Pb). Yes, lead is the winner, but no, the other species have not remained unchanged.
4-44, 21: With the few exceptions noted below, this is an especially strong section!
4-45, 30: Suggest adding "predominantly" before "primary", as coarse mode particles may originate from primary emissions, but in the ambient air they can/do become sites for formation of additional secondary material.
4-46, 8 (and as possible alternative to previous comment): Could add "Coarse-mode particle composition can be altered by chemical reactions and/or physical interactions with other gaseous particulate or liquid contaminants."
4-47, 30,31: Not clear what it means that "the net energy budget increased by 30%"?
4-49, 15,16: Could add that "while the particle size distribution shifts toward the fine fraction over longer inland transport distances." General this and the discussion of sea salt effects on coastal vegetation is interesting and useful detail. But I wonder rhetorically: Can effects from natural sea salt spray on coastal vegetation be considered "ecological damage" – or is it really just a component of the steady-state natural coastal ecosystem.
4-51, 20: Since there are clear indications of excess nitrate in sea salt following reactions with anthropogenic HNO3 resulting in coarse NaNO3 and additional N deposition, this point might be discussed here (along with eutrophic effects of additional N loading to coastal estuaries).
4-52, 19: Could add "in rural areas" after PM, since primary OC may be a significant contributor in cities.
4-52, 24: Should add after "areas" that ", or during late fall and winter in areas with high nitrate concentrations."
4-55, 8,9: I think this is an over-simplification, and suggest adding after "vegetation" "near sources", although this conversion also assures the longer-range transport of sulfate to and ultimate deposition in downwind areas such as the Adirondacks, Northern and Southern Appalachians and large sections of Eastern Canada with soils and associated ecosystems which are much less tolerant of excess sulfur deposition than those in the original source regions". See also following comment.
4-55, 14: This conclusion cited to a 1980 publication is entirely inconsistent with the more up to date discussion of effects of acid deposition on forest soils in a following section.
4-59, 3: I don't think this is especially accurate – unless this is intended to reflect only "heavy metals", in which case that should be stated. About 50 elements, including other metals, metalloids and transition elements that are potentially phytotoxic have lower boiling points than NI & Cr and are more likely than these to escape stacks in vapor phase.
4-79: Generally the brief discussion of visibility here is weak, and attributed to some very old references. It needs some work.
4-79, 6,7: 5 to 10 to 50% of what?
4-87, 22,23: Couldn't it be that the excess N is itself contributes to limiting biotic functions?
4-109, 20: "ration" should be "ratio". Also, I assume these Ca/Al ratios refer to soluble or bio-available concentrations and not to absolute elemental concentrations?
4-141,11: This section on visibility is quite good and (I think) appropriately concise, with references to more detailed sources of relevant information. There are also appropriate reference to the related recent regional haze regulatory program and subsequent illustrations of trends and

extinction budgets derived from the associated IMPROVE monitoring program. However, given that this a PM CD, and potential secondary fine particle standards are the specific basis for this chapter, I would like to see some added emphasis on three points:
The haze regulations pertain exclusively to specific Class 1 federal areas, and have no relevance (& offer no protection to) the other (what is it?) 98% of the country. This includes urban visibility and many widespread rural areas that are not Class 1 areas. True if responsible measures are eventually implemented to assure visibility improvement in Class I areas, we might expect ancillary visibility (& PM & deposition) benefits everywhere. However:
The haze regulations also have virtually no regulatory teeth (even more so given the recent DC Appeals Court BART decision), and there is nothing other than a vague "consultation" process to seek emissions reductions in one Regional Planning Organization (RPO) domain (the Midwest for example) in any other RPO Domain (the Northeast or Southeast for example). Furthermore, there is only 1 Class 1 area in the MWRPO (Boundary Waters) and it is largely unaffected by emissions within that jurisdiction.
The current Primary PM-2.5 (annual and) short term standards provide no protection against welfare effects on visibility. You can see through 65 ug/m3 PM-2.5 regardless of its composition or RH.
For these reasons, I think there some additional emphasis in this section on visibility in non-Class 1 areas, and a better set-up for consideration of visibility-improving secondary PM-2.5 standards, even if that option were eventually not chosen among the various alternative approaches. If such a standard were for example aimed at achieving a minimum level of visibility impairment in urban areas, it would likely be equally applicable to both Eastern & Western cities, and would likely be a useful supplement to the mild-mannered haze regulations.
4-156, 6,7: I don't have this reference handy just now, but think a likely causal mechanism for this was the increased water retention by the more acidic sulfate species that also correspond to the high sulfate concentrations.
4-159, 12-19: It might be appropriate here to slip in a comment about how much more useful the ASOS would be (and they would be extraordinarily useful) if they could be routinely obtained prior to the unfortunate censoring, binning & averaging of the raw sensor data.
4-160, 13,14: Adding IMPROVE data to AIRS helps nothing. AIRS doesn't work and the IMPROVE data are already accessible in a vastly superior system.
4-161, 28-30: The way this is phrased implies it's the higher summer RH rather than the higher summer sulfate. Suggest adding "higher summer sulfate concentrations and" after "because of" on line 29.
4-198, 28-30: Sulfate and ozone do not have different seasonal and spatial patterns in the eastern US.

Dr. Fred Miller

Chapter 6: Dosimetry of Particulate Matter
General Comments:
The chapter on the dosimetry of particulate matter is significantly improved compared to the second external review draft. The chapter contains adequate documentation of the factors affecting the deposition of particles in laboratory animals and humans. However, a major shortcoming is the lack of calculations presented that show dosimetric comparisons between

laboratory animals and humans. Previously, CASAC requested this information be included, and this review still maintains the need for such information to help put the animal toxicology studies in perspective to human exposure levels as part of judging the relevance of the animal results for potential risks to humans from exposure to PM.

The Summary section for the chapter needs additional work. The salient features to take forward to the integrative synthesis chapter should "jump out at the reader" in the Summary section. Currently, they do not do so; a suggestion of how to treat this is offered in the Specific Comments section below. Another general comment concerns Section 6.2.3.2 on age-related deposition of particles. The individual studies described in this section provide a significant amount of data on age-related deposition of particles, but one has difficulty in extracting general conclusions on the effects of age. Inclusion of a figure that depicts the results from the various studies would assist in evaluating whether trends as a function of age can be determined for deposition as a function of particle size. Clearly particle size is critical for whether or not there are age differences. This should be pointed out in section 6.2.3.2 and also carried forward in the integrative synthesis chapter. Specific comments are offered below that will strengthen the accuracy and quality of the chapter.

Specific Comments:

p. 6-7, l. 27	The comma should be after the word "particles" not before it.
p. 6-7, l. 28	Relative to the statement of charged particles having deposition 5–6 times that of particles having no charge and 2–3 times that of particles at Boltzman equilibrium, this statement needs to be put into perspective relative to actual urban aerosols. The influence of charge in urban aerosols should be minimal; clarification is needed that this would more likely relate to workplace exposures or indoor tobacco smoke environments.
p. 6-8. l. 29	The discussion of ET deposition should be corrected to indicate that somewhat higher total deposition occurs with nasal breathing for particles greater than 1 mm not as stated for mouth breathing.
p. 6-10, l. 1	Breathing pattern is listed as an important factor; clarify if breathing pattern is to be interpreted as tidal volume and breathing frequency or tidal volume, breathing frequency, and route of breathing.
p. 6-11, l. 5	The statement concerning very small ultrafine particles and gender differences is an overstatement. Only the 0.04 ：m particle had a significant gender difference; since particles smaller than 0.04 ：m were not used in the study, one cannot infer that there is a gender difference for particles < 0.04 ：m.
p. 6-12, l. 27	The sentence beginning, "Total deposition¼" needs to be reworded; in its current form, it is an awkward statement.
p. 6-15, l. 5	The discussion of the nasal passages being highly efficient collectors for ultrafine particles should be expanded to provide an orientation on the implications for toxicological studies. Specifically, the high ET deposition would lessen the probability that pulmonary deposition of ultrafine particles is an important factor in the toxicity of PM.

p. 6-16, l. 20	In the discussion of gender differences, a statement is made that for women TB deposition was greater by 21–47%, but in Fig. 6-7, the statement is made that the increase ranges from 26–53% greater. Which percent increase range is correct? Surely some of the differences in deposition between males and females are statistically significant. On page 6-3 a statement was made about *p* values being presented for tests compared to control, but this gender difference is one that is also worth testing, particularly since it would appear that there is a highly significant gender difference for coarse mode particles. This gender difference for the coarse mode should be brought forward as one of the potentially important results of the dosimetry chapter.
p. 6-20, l. 8	The description of the study by Kim and Jacques concerning the respiratory bolus technique is awkward. The discussion of this study and the representation of it is much more clearly given on page 6-25. The paragraph needs restatement as in the text it is indicated that particles were studied up to 0.1 ：m. Yet Fig. 6-8, which is referred to in the paragraph, shows deposition up to 5 ：m. The bottom line is that the paragraph ends up being confusing.
p. 6-20, l. 12	The statement that regional deposition varies widely along the depth of the lung is only correct for the ultrafine particles panel presented in Fig. 6-8. For particles 1 ：m and greater, this is far less pronounced if present at all. The paragraph should be clarified to convey this.
p. 6-24, l. 29	The statement concerning exercise as potentially increasing health risks from particles is a point that should be brought forward for the integrated synthesis chapter.
p. 6-25, l. 19	Section 6.2.3.2 on age presents a large amount of material from various studies. The presentation is restricted to a descriptive one. The authors should consider constructing a figure that is a composite of all the data in this section. Since there is a mix of particles and ages discussed in the section, a figure would be helpful for examining possible trends.
p. 6-25, l. 29	The statement that ET deposition in children would be generally higher than in adults is made. This statement is contradictory to the experimental data of Becquemin et al. (*Eur. Resp. J.* 4:694–702, 1991). Some discussion of this study should be made; this reviewer would give stronger credit to the experimental data than to the results of an unvalidated mathematical model.
p. 6-27,ll. 12	The synthesis statement from the Bennett study talks about ET deposition being age dependent and total deposition not, suggesting that ET region does a more effective job in children of filtering out particles that would otherwise reach the TB region. The question is since Bennett used only 4.5 ：m particles, is this statement restrictive to the results of Bennett et al. or can it be generalized from other data presented in the section? A figure would greatly assist in making this determination.

p. 6-29, l. 7	The sentence beginning “Total respiratory tract deposition¼” is awkward and needs rewording.
p. 6-35, l. 1	Inhalability is introduced as a topic in this paragraph. It would be good to point out the availability of equations in Ménache et al. (1995) that can be used to determine inhalability adjustments needed as a function of particle size to compare laboratory animal and human studies.
p. 6-38, l. 3	Concerning the results of Musante and Martonen (2000b) wherein statements are made about ventilatory characteristics having to be adjusted to provide for comparable regional deposition to that in humans, while correct, belies the more important following point. The results of these authors stress the need for dose-response studies and for models that are capable of adjusting for the dosimetric differences between species. The ability to adjust ventilatory drive of the rat trying to achieve comparability of deposition fraction to be similar to humans is not only fraught with difficulty but overlooks the fact that cell composition varies between laboratory animals and humans as a function of airway generation. This fact further illustrates why dose-response data are needed with dosimetric models being able to make interspecies adjustments for appropriate comparisons.
p. 6-45, l. 6	The Falk et al., 1997 study is described as showing that half of the particles that had not cleared within 24 h cleared with a half-time of 50 days. This reviewer questions this finding since the statement is made that these particles deposit in small airways; the question is how the authors could distinguish between tracheobronchial small airways and respiratory bronchioles such that the finding may represent part of alveolar clearance in combination with TB clearance.
p. 6-49, l. 11	The word “evaluated” should be replaced by the word “elevated.”
p. 6-54, l. 2	In this paragraph on particle overload, the argument is made for volumetric overloading based upon Morrow’s work in 1988. However, the more recent work of Driscoll shows that surface area of these poorly soluble particles is a strong predictor of their carcinogenicity over a wide range of exposures and types of PSPs. Moreover, various investigators have presented evidence of surface area being important for ultrafines and other particles relative to toxicity.
p. 6-55, l. 1	Section 6.5 on comparing inhalation and intratracheal instillation is improved from the previous external review draft but still does not make reference to the value of intratracheal instillation for mechanistic studies nor of making the clear statement that while IT can be used for mechanistic insights, inhalation studies are the appropriate route for risk assessment.
p. 6-63, l. 13	The wrong study is being described. The paper by Asgharian, Wood, and Schlesinger (1995) which covers empirical modeling in the alveolar region, used nomograms to estimate alveolar deposition fractions. The paragraph needs rewriting, with the removal of Anjilvel and Asgharian (1995) and Subramaniam et al. (1999).

p. 6-65, l. 28 This reviewer strongly disagrees with the statement that insufficient data were available to adequately model long term retention of particles deposited in the conducting airways of any mammalian species at the time of the previous Criteria Document and still as of this time. This is an overstatement of the ability of both the ICRP and the NCRP models to treat deposition and clearance of particles in humans. Why in this paragraph is the emphasis on tracheobronchial clearance and then on the next page the emphasis on alveolar clearance given that the section talks about models to estimate retained dose and doesn't distinguish between regions?

p. 6-65, l. 31 The word "properly" should appear in this sentence after the word "evaluate."

p. 6-66, l. 19 This paragraph concerns particle retention patterns in rats and humans and a comparison of the differences observed by Nikula et al. (2000). The human comparisons involved coal dust there is no discussion of the modeling results of Kuempel and colleagues for coal workers. These investigators have a number of relevant publications that should be cited.

p. 6-67, l. 6 The statement is made that various currently available models simulate particle behavior in an idealized respiratory system and can only predict average regional and total dosimetry in the lung. This is an overstatement and an incorrect representation of capabilities of stochastic lung models. These models can predict deposition in individual airways as well as the fact that they are clearly not idealized representations of the respiratory system.

p. 6-73, l. 13 Section 6.7 on Summary and Conclusions needs additional work. The salient points that will go forward to the synthesis chapter should "jump out at you" in the Summary section. This is not currently the case. One could consider ending with a bulletized list of points that you want the reader to have learned from reading the dosimetry chapter and that form the basis of some of the integrative synthesis chapter statements. An example of such a bulletized list would include the following:

- Gender differences for coarse mode particles
- Exercise increases respiratory dose
- Deposition patterns are similar between animals and humans but there are absolute differences in deposition fractions
- Clearance processes are similar in animals and humans but the clearance rate for particles is typically faster in laboratory animals
- With disease states such as COPD, there is increased deposition, and the deposition pattern is more central
- And the list goes on to identify the top points that the reader should glean from the chapter

Chapter 7: Toxicology of Particulate Matter in Humans and Laboratory Animals

General Comments:

A better balance in the treatment of topics has been achieved in Chapter 7 compared to the second external review draft. Similarly to Chapter 6, the Summary section for Chapter 7 fails to provide the salient points that the reader should be taking forward to the integrative synthesis chapter. Most of the current summary is subdivided according to particle type. While this is probably reasonable, the Summary section does not go far enough. It should end with the major points that have been discussed or established as potential mechanisms by which particles can exert effects on the cardiopulmonary and cardiovascular systems. Currently, Chapter 7 has a disconnect with Chapter 9 in that insights on mechanisms of action are downplayed in the Chapter 7 Summary and yet are phrased positively as supportive of epidemiological studies in the integrated synthesis chapter. So what is the bottom line here? While my specific comments note some points that should be included in the Summary section, they are by no means an exhaustive identification of potential points of importance.

Another area that needs additional work is the section on cardiovascular and systemic effects of PM. The current chapter contains only two references post-2000 for cardiovascular and systemic effects. The authors need to ensure via a comprehensive literature search that all relevant material on this topic has been included, particularly since the epidemiology studies are increasingly finding the potential for cardiovascular and cardiopulmonary effects from exposure to PM that relate to acute mortality and in some instances perhaps acute morbidity. Specific comments are provided below that are intended to strengthen the accuracy and quality of the chapter.

Specific Comments:

p. 7-18, l. 1	Relative to the Ghio et al. (2000a) study, the statement is made about a mild increase in neutrophils in the bronchiole and alveolar fractions of the BAL in human subjects. Are the authors indicating that this is an effect to follow relative to potential injury?
p. 7-22, l. 22	Table 1 should be referred to as Table 7-1.
p. 7-23, l. 1	The fact that the biological effects of ROFA in rats have been shown to depend on aqueous leechable chemical constituents of the particles is a point that should be brought forth to the Summary section of the chapter, particularly since depletion of these from the particles eliminates their pulmonary toxicity.
p. 7-23, l. 23	The Kodavanti et al. (1998a) study on ROFA makes the important contribution that different metals are responsible for different biological end point effects. This is a finding that should be reflected in the Summary section.
p. 7-27, l. 1	The sentence beginning with “Asthmatic subjects” and the next sentence beginning with “Brief exposures” should be deleted. They are repetitions of text already contained in the paragraph.
p. 7-31	How did Ghio and colleagues determine that only a small amount of the iron that was instilled in their human subjects was “active”? Please clarify.

p. 7-34, l. 18	The section on cardiovascular and systemic effects of PM in humans and laboratory animals contains only two references post-2000. Given the emerging evidence from epidemiological studies of cardiovascular and cardiopulmonary effects of PM, a concerted effort should be made to include all relevant animal toxicological and human controlled studies published in the last year and a half that relate to this topic. Alternatively stated, the authors should ensure that the literature has been thoroughly searched for this particular topic since it is emerging as a potential primary mode of action of PM in producing acute mortality and potentially acute morbidity effects in humans.
p. 7-39, l. 6	The articles by Kodavanti (1999) and Watkinson et al. (1998) using instillation and inhalation show major differences for ROFA and clearly demonstrate that IT is not equal to inhalation. This is an important finding given the magnitude of the exposures that were involved and should be brought forward to the Summary section.
p. 7-42, l. 23	Was the 5% increase in heart rate in the Godleski et al. (2000) study statistically significant?
7-69, l. 8	An important observation for the Utah Valley study was that similar quantities of Cu^{+2} could replicate the biological effects observed with the particles alone. This is an important observation and is worthy of notation in the Summary section.
p. 7-71, l. 19	In the Soukup and Becker (2001) study, the most potent fraction was the insoluble $PM_{10-2.5}$ fraction. This is an important point because it illustrates that coarse mode particles can't be ignored in the overall toxicity of PM.
p. 7-80, Section 7.5.3.2	A drawing of signaling pathways and an indication of which ones have been shown to be affected by PM would be useful to include in this section.
p. 7-88, l. 22	In the discussion of the effects of CB vs. ufCB, the point is made that PMN influx has CB producing a greater response than ufCB at all doses used in the study when the influx is expressed as a function of surface area. What is the implication of this finding that mass is not important and that surface area is the metric that should be used for assessing PM toxicity? Or is the finding in this study restricted to the particular end points addressed and the particular compound, namely carbon black?
p. 7-99, l. 26	Clarify what is meant by the statement that no newly published studies on the effects of PM on host susceptibility are available. If this is referring to a comparison for the 1996 review, the answer would be yes; if on the other hand it is relative to the second external review draft, then the answer would be no.
p. 7-104, l. 13	The statement about being able to extrapolate the results of the various studies by Calderon-Garciduenas were done in Mexico City to conditions in the United States should be worded in a way that the reader can understand that these results probably represent an upper bound on what might be the effects of PM in the United States.

p. 7-110, l. 14 Section 7.7.2 — This section comes across as no real insights have still been established on potential mechanisms of action. Given that exposures were extremely high compared to ambient levels, this may be why the authors believe no real insights have been obtained. However, Chapter 7 clearly establishes various points that should be brought forward as potential mechanisms of action that are being increasingly supported by experimental work. These mechanisms were identified throughout the presentation of the material in Chapter 7.

p. 7-105, l. 15 — The Summary chapter on toxicological effects in animals and humans should identify the salient points that the reader should extract. In large part the Summary section is devoted to types of particles. While this may be appropriate, the final summary for the chapter should integrate across particle types and provide a bulletized list of the points that are likely to be brought forward to the integrative synthesis chapter.

Chapter 9: Integrative Synthesis

General Comments:

The integrative synthesis chapter is uneven relative to the inclusion of references. Some sections provide numerous references to support statements while others have hardly any citations. Since many individuals will only read the integrative synthesis chapter, citations for the critical points being made should be provided. Given the need to reexamine the epidemiology chapter and the additional work needed on Chapters 6 and 7, CASAC will need to review a revised Chapter 9 before considering coming to closure on this chapter.

Included in my specific comments below are a couple of questions concerning Adult (Acute) Respiratory Distress Syndrome (ARDS) that potentially raise methodological questions in my mind at least for epidemiology studies of acute mortality in relationship to PM. One of the persons providing comments in the Public Comments session on July 18, 2002 at the CASAC review of the PM Criteria Document noted they had a data base of about 17,000 individuals for which they had acquired death certificates and codified cause of death. Subsequent inquiry of their data base shows that about 125 persons had ARDS listed as primary, secondary, or tertiary cause of death. Given the way the GAM model effect estimates are decreasing in the reanalyzes of various epi studies due to identifying statistical "glitches", I wonder what effect, if any, the inclusion of ARDS deaths may have on air pollution effect estimates and/or their level of statistical significance. Is a percentage of the ARDS deaths being incorrectly attributed to the excess risk for a 10 ：g/m^3 increase in PM? I feel strongly that pulmonary clinicians do not believe there is any way that ARDS is caused by air pollution. Yet these deaths are not excluded from the "all non-accidental causes of death" analyses. I encourage the Agency to assess whether there are potential ramifications of ARDS to PM acute (and for that matter chronic) mortality studies.

Specific Comments:

p. 9-7, l. 27 — The concept of respirable particles is introduced. They are defined to be a subset of thoracic particles that are more likely to reach the gas exchange region of the lung. This is somewhat superfluous since the

	particles that penetrate to the thorax all have a finite probability of lung deposition. What would be the particle size that would be the definition of respirable particles? Would it be 3 ：m, 5 ：m, or what? I believe the introduction of respirable particles, given its inability to have a very defined criteria, is not warranted and serves no purpose for the assessment of PM. My strong recommendation would be to delete the terminology, particularly since it only appears to be used in Section 9.2.3.
p. 9-51, l. 17	Strike the word "mostly."
p. 9-52, l. 26	In this paragraph, general statements are made concerning elevated deposition in the upper TB airways in women compared to men. The difference for coarse mode particles between male and female is most likely highly statistically significant and is 1) worth of notation as such, and 2) has clear implications for coarse mode particle potential effects.
p. 9-53, l. 1	The study by Becquemin et al. (1991) challenges that ET regional deposition decreases with age from 1 yr of age. This study should be evaluated and the statements such as appearing on this page should be modified accordingly.
p. 9-53, l. 16	This sentence is a good example of why citations should be provided in the integrated synthesis chapter. The finding about COPD subjects having greater deposition and the average deposition being 2.5 times higher under elevated rates is an extremely important observation but no citation is provided. Unless the reader goes back to Chapter 7, the support for this statement is not evident.
p. 9-56, l. 1	The statement is made that clearance of particles from the alveolar region is usually rapid and less than 24 h. This is an incorrect statement as the half-times for alveolar clearance are much greater than 24 h.
p. 9-56, l. 8	The description that particles < 2 ：m are deposited significantly more than particles > 5 ：m probably should be modified to indicate > 8–10 ：m because 5–6 ：m particles have depositions at least as great as 2 ：m particles in the alveolar region.
p. 9-56, l. 15	This sentence should be reworded to avoid the use of "COPD sufferers."
p. 9-57, l. 1 Section 9.8.3	A stronger conclusion is needed for this section to indicate that IT can be used for mechanistic studies but that inhalation must be the exposure route when effects are brought forward for risk assessment purposes.
p. 9-58, l.14 Section 9.8.5	Stronger statements are possible for this Summary section and are of the nature as this reviewer's comments already indicated for the Summary section of Chapter 6. Those Summary section comments should be brought forward and clearly identified in Section 9.8.5.
p. 9-68, l. 2	Strike the word "also."
p. 9-74, l. 25	Relative to recorded deaths caused by respiratory diseases, the statement is made that 44% result from acute infections. Acute infection is a major factor in many ARD cases, but ARDS typically is associated with traumas of a nature that would not have arisen from PM exposure. So the question arises as to whether ARDS-related deaths are excluded from air

	pollution effect analyses. They most certainly should be excluded. (Note: I believe current estimates indicate from 50,000 to 100,000 ARDS deaths per year. Moreover, the current mortality rate of about 40% is significantly lower than was the mortality rate in the 1980s and early 1990s.)
p. 9-76, l. 1	Statements are made about the immediate cause of death, as listed on death certificates, being evaluated in relationship to various PM indices in some epi studies. In ARDS, the immediate cause of death is probably most frequently cardiac arrest or respiratory infection with ARDS being listed later on the death certificate as a contributing factor. This brings into question misclassification of cause of death and relationship to air pollution since the secondary cause of ARDS in these cases is simply a notation of the fact that cardiac arrest or respiratory infection ensued. These individuals almost certainly did not die because of air pollution. An accompanying question is whether persons dying in the hospital are recorded on hospital records as having been discharged. It they are, hospital admissions and discharge analyses for air pollution effects should adjust for this fact.
p.. 9-78	In Table 9-13, asterisks appear by some of the numbers but there is no definition in the footnote to the table as to what the asterisks mean.
p. 9-78, l. 6	In this paragraph, there is a discussion about impairing normal repair processes in early postnatal development, but there is nothing on how the number of infections under age 5 can alter pulmonary function and other aspects of lung development. It would be worth looking at some of the publications by Collier and colleagues on the Frank Porter Graham School population to assess the magnitude of some of these early infections on lung development.
p. 9-83, l. 28	The sentence here on particles not naturally found in the air should be modified because in just a few sentences later the real world studies of Calderon-Garciduenas on Mexico City dogs is discussed.
p. 9-88, l. 30	Ozone should be eliminated from the examples given because it is a very poorly water soluble gas.
p. 9-147	For Figure 9-34, the legend should indicate 15 breaths a minute, not 15 breaths a second. Also, the legend should identify that MB and NB stand for mouth and nasal breathing, respectively.
p. 9-148, l. 5 Section 9.13.3	Apportioning Health Effects to PM (by size, chemical component, or source category) and Gaseous Co-Pollutants. This is an interesting section for statisticians but efforts should be made to write the section more in terms that a lay person could follow.
p. 9-150, l. 13	“even is” should be “even if.”

Dr. G. E. Taylor, Jr.

This draft has several "renovations" that provide a somewhat better foundation for assessing the impact of PM on the environment, and the staff are thanked for their efforts to address some of the shortcomings.

The comments below fall into three categories. The first is an overarching issue that continues to plaque the document. The second is a set of major concerns that individually and collectively are important for consideration in this and any future effort to address the risk to the environment of criteria pollutants. The third is a set of issues that are specific to the PM criteria document.

My personal assessment of PM effects on the environment places the risk at a very low level. The processes and pathways by which I arrived at that conclusion are not well known and not "documentable", particularly the quantitative aspects of my merger of the risk of exposure with the risk of response. The same conclusion - minimal risk - is offered in this CD for the environment; however, it is impossible to follow the development of the Agency's risk assessment and the degree to which quantitative methodologies were applied to arrive at that conclusion. It is important that this process be fully "laid out" so the assumptions and caveats are known to all. It is fortunate that the PM issue is a minor one with respect to the environment.

Overarching Issue

Coordinated and integrated framework for addressing the risk to the environment. This is the most significant problem with this CD and while the problem can not be rectified at this juncture there is no reason that the shortcoming should be perpetuated to future CD's for PM or any other criteria pollutant.

While the document has a clear framework for addressing the risk to human health and accomplishes that risk assessment in a very quantitative and rigorous manner, there is no parallel framework for addressing welfare effects. This is an unacceptable shortcoming in that the reader is not aware of the path to be followed in assessing the risk and how the disparate "pieces" will be folded into a scheme for assessment. In my judgment, the Agency does not have a framework in place to accomplish that objective.

It is strongly recommended that the Agency develop a framework that can be used for all pollutants and that allows the reader to know in advance how the effort will unfold. It needs to have a specific objective, explicit methodology that is quantitatively based, strong and deliberate effort to address exposure, strong a deliberate effort to address response, strong and deliberate effort to fold the two together, and a strong and deliberate effort to address economic issues linking together traditional methods as well as ecological economics.

In the absence of a clear framework for addressing the risk to welfare, there is no reason to perpetuate a chapter devoted to the environment.

Major Issues

Introduction and the Environment. The Introduction does an adequate job of casting the framework for the risk to human health. Unfortunately, there is no parallel effort to address the risk to the environment, which in large part is co-registered with the above concern regarding the absence of a framework for assessing risk. My sense is that the Agency has abdicated its mandated responsibility to address the risk to welfare.

Executive Summary. This section has a number of shortcomings with respect to the environment. The most striking is the absence of any quantitative assessment of the risk due to PM, and this is in marked contrast to the human health section. The discipline of ecology is rich in quantitative methods at a local, regional or global scale, but very little of that expertise is harnessed in the CD. The executive summary could benefit from having a framework (see above) around which to prioritize and organize the most important conclusions. Without a systematic framework for analyzing the welfare effects, the executive summary is chaotic and elementary.

An example of the disconnect between the rigor of the discipline of ecology and the CD is the first several bullets on the environmental effects of PM (E-26). The first set of conclusions is ones that one might expect from a junior high or high school classroom. Those "conclusions" have the unfortunate effect of diminishing the sophistication of the discipline of ecology.

In the future, it is recommended that a quantitative framework be developed (see above) and that the framework be a guide for offering conclusions for the executive summary.

Integrated Summary (Chapter 9). This chapter fails to address **any** issue on PM effects on the environment, and might be viewed as a "window" on the Agency's perspective on the risk to the environment. Whereas a risk assessment might lead one to conclude that there are no effects worthy of inclusion in an integrated summary, that conclusion has not been forthcoming in this CD.

I recommend that the integrated summary be re-cast to include a clear and definitive statement regarding the risk to welfare and include the rationale for making that conclusion.

Section 4-1 (lead section). Section 4.1 introduces the risk assessment of PM on the environment. This section should be followed with a statement of the objective and a framework for developing the data base and analyzing the information. While it is too late to do so herein, it is recommended that this quantitative framework be established for all future CD's.

Lead Paragraphs. On page 4-2 (first full paragraph) and page 4-3 (first full paragraph), these are important paragraphs that lead into the presentation. Both paragraphs are riddled with inaccuracies. For example, on 4-2 the statement is made that other than N and S there is no information on other chemical substances in PM; there is plenty of information on trace and heavy metals. On page 4-3, the entire paragraph offers caveats and statements that are not supported by the ecological literature. I would recommend that this entire paragraph be re-written.

Mechanisms of Deposition. The current version does a nice synthesis of the processes controlling deposition of PM to elements in terrestrial and aquatic landscapes. That addition addresses one of the shortcomings of previous versions.

Urban Ecosystems. The section on page 4-133 is a nice addition. The authors state that there are no references to PM and urban ecosystems in the literature. It is unclear why this restriction would be placed on this topic when the remainder of the CD paints a very broad brush (e.g., nitrogen, sulfur, UV-B effects). To the contrary, there are many articles on deposition and biogeochemistry of nitrogen, sulfur, trace and heavy (e.g., lead, cadmium) metals in urban ecosystems (principally forests and roadsides). It is unclear why the authors would impose this restriction on this topic and not all others. In my own assessment of the literature since the last meeting, in very short order I was able to identify immediately 25 articles of relevance and suspect that these would lead to at least twice that number. Most addressed trace and heavy metals, deposition, biogeochemical cycling, and effects (plants, soils, microarthropods in the soils, etc.).

Length. I question whether the length of this chapter, and the same issue was raised at the last review. If the risk to the environment (Chapter 4) garners no place "at the table" for the Integrated Summary, it is nonsensical to have 240 pages of material. Many of the topics in Chapter 4 could be eliminated or argued in a far shorter section. Again, this re-focusing is not appropriate at this late stage, but a framework for evaluating risk starting with a clear objective statement would help to place the effort in a context.

Co-occurrence of PM and other pollutants. The health effects section walks through a very specific accounting of the issue of co-occurrence of PM with other pollutants (e.g., ozone, sulfur oxides, etc.). In the section on the environment, there is never a mention of this association, implying that all effects are due to PM alone and its chemical constituents. This omission would not occur if there were a clear and systematic framework for assessing the risk to the environment.

Minor Issues

Page 4-66. The statement is made that ecosystem responses are initiated at the level of populations. I am not convinced that this is accurate. Since this same issue was raised before in my review, maybe a reference would suffice. However, I am of the sense that this conclusion is inaccurate.

Page 4-68. The following sentence is quoted herein and the same issue was raised before: "In contrast *(to natural stresses)*, anthropogenic stresses usually are severe, debilitating stresses." This statement is simply untrue. It also misrepresents the literature. Almost all anthropogenic stresses that are being addressed at the regional to global level involved chronic exposures and subtle effects that are manifest over time. The most obvious examples are carbon dioxide, ozone, mercury, climate change, habitat fragmentation, PCB's, endocrine disruptors, etc. This statement might have been appropriate decades ago (e.g., Copperhill Tennessee), but it certainly

misrepresents the ecological literature today. It also portrays the discipline of ecology in a very poor light.

Page 4-70. This following sentence was noted in the previous review as being problematic, and I re-surface it again; it also stands in marked contrast to the previous concern (No. 2): "Responses of ecosystems to stresses (unless severe or catastrophic) are difficult to determine because the changes are subtle. " Again, this is not an accurate reflection of the state of the science. It is quite clear that chronic sustained stresses have predictable effects and while the magnitude of those effects is not known with certainty, the community of ecologists is doing a very good job of honing in on the direction of change and even the magnitude of change in many cases. Again, the statement offered in the CD does not portray in a good light the discipline of ecology.

Dr. Frank Speizer

Executive Summary

This introductory section seems to catalog what comes next in the E4.3. x sections. As written, it is rather off-putting in that it really doesn't tell the reader much. It might be better to simply start with a list rather than a few sections for each bullet and then get right into the E.4.3. x sections.

Pg. E24, lines 8-9

Unnecessary comment on association vs causality

Pgs. E26 – E35, Sections E5 – 5.3

Seem out of place. Either they should come earlier or after a full discussion of health effects.

Abbreviations - A number of Abbreviations and Acronyms are duplicated, or some letters used to define different things, or are blank.

Chapter 5

Specific Comments:

Pg. 5.1, lines 10-11

This definition of exposure as the concentration in the "breathing zone" is far too limited. Exposure, in fact, is often estimated from a far more distant place. This definition is much closer to "dose."

This confusion is continued throughout the section that ends at line 11 on page 5.2.

There probably needs to be a caveat that the way exposure is being used here is with the knowledge that it is an estimate of exposure that is modeled from multiple sources. This is more or less what the last line of the section says, but it probably needs to come earlier.

Pg. 5.5, Section 5.3.1 Para beginning line 3.

This clearly makes the point I was trying to express above and confirms for me that the above point needs to be taken into account in the initial introduction to the chapter.

Pg. 5-8, Section 5.3.2.1.

Not clear that these sections should be here. They are previously in Chapter 2. They are not really reporting data but are discussing methods. On the other hand, the discussion on models that estimate concentrations of exposure surely belongs here.

Remainder of Section 5.3.

This represents a reasonable summary of the relatively simple methods for calculating total "personal exposure." However, I still believe that "personal exposure" needs to be further qualified to at least "estimated personal exposure." The sense of the variability and uncertainty in the measure is not captured as currently described.

Pg. 5-26-5-30, Table 5.

Results are not reported in a consistent manner. Some of the summary of measurements contain actual values of summary statistics, others contain notes/findings. Vice-versa for note/findings where some contain data, others contain comments. Should be consistent. Otherwise table quite useful.

Pg. 5.68 – 5.72

Suggest make Table listing and providing pros and cons for each of the source apportionment methods. (CMB, UNMIX, PMF, SRFA, etc.)

Pg. 5.82 lines 7-8.

This statement on lack of correlation of indoor/outdoor $PM_{2.5}$ seems inconsistent.

Pg. 5-86, line 14.

Take out word "very." In fact, there has an impressive amount of data produced in the last 5 years. Although I would agree that it is limited, we must recognize the progress made.

Pg. 5-87, line 28.

Probably worth spelling out all these abbreviations.

Pg. 5-87, line 4.

Change "errors."

Pg. 5-108, lines 1-4.

After reading the section preceding this conclusion, I believe more could be said. A lot of qualifiers have been offered and directionality of potential biases from measurement error discussed, rather than simply saying "could lead to bias in situation," need to add, "but likely to result in bias toward the null."

This would be more consistent with what follows on line 17-18 and lines 22-25 and 26-29.

General Comments:
The four major objectives in this chapter clearly seem to summarize existing and newly produced work. The discussion of using the ambient measures as surrogates for exposure is only moderately well developed. Issues of dose are only touched upon and maybe that is okay since the next chapter goes into detail. The place where these two chapters come together to convert exposure into dose, is not clear to me yet. I am commenting here only as a reminder if I do not see it by the end of the next chapter.

Chapter 6
General Comment:
I found this to be an exceedingly well organized and well written chapter that covers the field of particle distribution, deposition and clearance. Several new findings that have important influence on the thinking and understanding of the dynamics of exposure are well described. If I find any fault it is in the judgement to include the section on the mathematical and still theoretical discussion of a computational fluid dynamics model beginning in section 6.6.3. Although interesting and important from a theoretical perspective since many of the models have not been tested, it is hard to see the direct relevance to the criteria document. From a planning point of view I would suggest, since it is already here, it be included but a note be made that unless the models move on to validation and discussion of relevance, that the section be dropped from the next C.D. 5 years from now.

Specific Comments:
Pg. 6.6 - Line 13 Suggest reference to figure 6.1 here or define abbreviations better.

Pg. 6.6 - Lines 24-30 Although quite accurate is the criteria document, the right place to put a definitive Aparticle physics@ statement? This should be simplified.

Pg. 6.8 - Line 31 AED not defined and not in Abbreviation list.

Pg. 6.29 - Line 16-18 This statement seems unsubstantiated. Not clear that chronic bronchitis increases airspace more than emphysema.

Pg. 6.30 - Para beg. Line 3 Although may be accurate, it seems illogical. Children do not get lung cancer. Why develop a mathematical model for children in this setting? If paper is about different forms of obstruction and deposition in children, okay. If it really is about Atheoretical lung cancer@ in children, suggest it be left out!

Pg. 6.40 - Table 6.1 This table seem unnecessary. It simply outlines topics to be discussed.

Chapter 7

General Comment:

Although the review of the respiratory effects seems appropriate and describes studies that have been done since the previous C.D., much of the (limited amount) of work is not really new. In contrast, the cardiovascular effects work is new and seems to be where the major work has been done. (This is an observation and not a complaint.) In fact, this seems to be a direct effect of the perceived need for work in this field. What is most interesting (as an observation) is that cardiovascular effects are being recorded via inhalation with no perceived pulmonary effects.

Specific Comments:

Table 7.2 Further evidence that abbreviation list is incomplete. PEF,TV and LDH in first row of table not defined. Wouldn't it be better to organize the table by species, since it does not appear to be organized by any other category? Alternatively, it could be organized by Exposure Technique. (Sorry, I just figured out that it is organized alphabetically by first authors' last name, which seems to me the least valuable in using the table.) Ditto rest of tables.

Page 7.15, Line 29 "Cellular injury and (LDH.)" For anyone but the most sophisticated reader, equating these two terms is a big leap. At least spell out LDH.

Page 7.20, Line 20-21 Not clear what, "…effects…were not significantly attenuated" means.

Page 7.21, Line 5-6 This is a critically important finding and we must be sure it gets reported again later when dealing with system findings.

Page 7.27, Line 1-2 Something missing in test (see line 4.)

Page 7.27, Section 7.23
to end of this section There is a "hint" in the text that there is a potential for most of the experiments done with metal particles have used concentrations that were "too high." The results are generally interpreted as negative and therefore metals are not important. On the other hand, there is a potential that the "response mechanisms" present at more realistic doses (and even those are not so realistic at 1-5-10 x ambient) might be activated at these doses rather than at 50-100 times ambient when the mechanism may be "overwhelmed." I think the discussion needs to expand to take account of this possibility in greater detail.

Page 7.58, Line 26-27-28 Issue is that DPM may be coming down in terms of mass, but in terms of number, may be going up. Sentence should be qualified to take this into account.

Page 7.62, Line 20-26 Not clear why this methods caution need to be in this text.

Page 7.73, Lines 26, 27 PHS-1 & PHS-2 not in abbreviation list.

Page 7.88, Line 29 Sentence structure: take out first "old."

Page 7.110, Line 10 A slightly overstated exaggeration of sample size needs for morbidity and mortality.

Dr. Sverre Vedal

Chapter 8 (Epidemiology)

1. Tone of chapter. I continue to be concerned about the tone of the chapter as reflecting an attempt to convince the reader of a point of view as opposed to being an objective appraisal of the evidence. While this is related to the issue of the mechanism for selecting studies (point 2 below), it goes deeper than that. Typical of this tone is the statement beginning as early as the first paragraph of the chapter: "The numerous more recent epidemiologic studies reviewed in this chapter generally identify more cities where ambient PM relationships with morbidity and mortality have been found…". This statement is a half-truth, and half-truths pervade the chapter. In this instance, while it is true that many new studies do indeed identify more cities, it ignores the fact that many studies do not. Further, and perhaps most importantly, it seems to ignore one of the most important recent observations in PM epidemiology, namely, that in NMMAPS II there is remarkable heterogeneity in estimates of PM effect from city to city. This observation provides one with a vastly different impression than is conveyed by the above quoted sentence.

2. Study selection. The ad hoc and biased nature of the selection and consideration of studies in the chapter undermines the credibility of the points being argued and supports the impression that a point of view is being presented rather than the chapter being a credible appraisal. Until an explicit mechanism is utilized for selecting studies for inclusion, the chapter will continue to be legitimately criticized as being biased.
I will present an example from a study of my own, which is something I genenerally prefer not to do. However, it is very illustrative of the problem, and I know the paper well. This was a study of symptoms and lung function in asthmatic and non-asthmatic children (Vedal, 1998). The principal findings were that adverse PM10 effects were observed in the asthmatic children, but not in the children without asthma. The study is cited in the chapter sections dealing with symptoms and lung function in asthma, since in the study an effect was observed in the children with asthma. The study is not cited in sections dealing with symptoms and lung function in subjects without asthma; no effect was seen in the non-asthmatic children. Since the study was considered adequate for providing information on asthma, it would seem that it would also have been adequate for providing information on normal subjects. I present this as but one example of selective, and apparently biased, reporting of study findings. Since there are many such examples, I am left wondering how far this bias extends in reporting findings from studies with which I am less familiar.

3. Confounding. An extended and largely new section is devoted to a conceptual presentation of confounding. While this is informative, the form of the presentation is clearly guided by a view to addressing the issue of confounding by the gaseous co-pollutants (see #3 below). First, there are some mistakes regarding confounding. A confounder needs to be related to exposure (that is, to PM); this is not the same as stating that there needs to be exposure to the confounder (in this

case, a gaseous co-pollutant) as stated (p.9, lines 26-28). Second, several criteria for confounding are presented (p.181, lines 13-17). The following are criteria for confounding: 1) the confounder needs to be related to the exposure, 2) the confounder needs to be a cause of the outcome independent of its association with the exposure, and 3) the confounder cannot be part of the causal pathway from the exposure to the outcome. Any other criteria are ad hoc. The additional criteria for confounding itemized in this section and presented below are meant purely to shore up an argument that gaseous pollutants cannot confound the association and are not accepted criteria for confounding: 1) personal exposure is correlated with ambient concentrations of both PM and the gases (the confounder), and 2) personal exposure to PM and the gases (the confounder) are correlated.

4. Gaseous co-pollutants. This issue of potential confounding by the gaseous co-pollutants is complex. While I am sympathetic to the view that the measures of the gaseous pollutants are likely serving as surrogate measures, it is not clear that they are surrogate measures of PM. The question is not whether the gases themselves meet criteria for confounding, but rather whether the factors that the gases are in fact measuring (as surrogate measures) meet these criteria. This cannot be answered well, since we don't know what the gaseous pollutants are in fact measuring. It is likely, however, that they are measuring a combination of pollutant source emissions and meteorological factors that largely determine ambient concentrations and that are in fact true confounders. Since these factors also determine the PM concentrations, there is a problem with including multiple measures of the same phenomenon, albeit reflecting somewhat different aspects of the phenomenon, in the same regression models.
Further, measures of PM are also surrogate measures of something, and do not necessarily have primacy over the gaseous pollutants in this regard. That is, PM measures are not really measures of PM exposure in much the same way that measures of the gases are not really measures of exposure to the gases. An argument can be made that ambient PM concentrations are qualitatively different from ambient gaseous concentrations in that they are better measures of PM exposure than the gases are of exposure to the gases. While this is likely the case in some settings, it is not true in others, further complicating the picture. The bottom line is that the attempt in the chapter to demote the gaseous pollutants from the status of confounders to that of surrogates for PM is not successful. That is, the interpretation of coefficients from single-pollutant PM models, as well as those from multi-pollutant models, remains problematic.

5. "Chronic" studies. The recent developments regarding temporal smoothing and modeling of meteorological covariates in the time-series studies (GAMs issues, etc, see below.) have introduced more uncertainty into the time-series findings. As a result, even more emphasis will be placed on the findings of the cohort studies of chronic PM exposure. As a bottom line, the chapter essentially dispenses with the findings of the AHSMOG and Veterans cohort studies, findings which are in many ways at odds with those of the Six Cities and ACS studies. Although there may be good reasons for downplaying the findings of these studies, if this is going to be the stance taken in the future, better arguments will need to be put forward.

6. The GAMs issue for the next chapter 8 revision: The two effects of using GAMs are: 1) inadequate convergence criteria result generally in underestimation of the effect estimates, and 2) the standard errors are underestimated. The motivation for using GAMs initially was sound.

The problems with GAMs are not conveyed well by either of two extreme views: 1) that they only affect a very small part of the evidence regarding PM health effects, and the statistical "fix" should allow even that problem to be resolved, versus 2) that the PM-health effects field is now in total disarray. The GAMs "problem" has uncovered several issues. First, how tightly to model the temporal and meteorological terms (the confounding variables is time series studies) is still problematic. Second, the pollutant-meteorology effect/interaction is very complex and is probably not well-modeled. Although we have learned much from the uncovering of this issue, and this knowledge will certainly advance the field, it is indisputable that there is now more uncertainty. This uncertainty should be reflected in the next revision.

7. Model selection. The work on model selection (M. Clyde, pp.175-176)) should not be left without further discussion. Although the approach used may not be fully justified, it raises the important issue of how investigators select statistical models in analyzing the PM effect, including selection and specification of variables. Analysts in the PM-health effects arena make many decisions, often with little justification and typically with little knowledge about the process being modeled. This adds another element of uncertainty to reported findings. Although uncertainty is unavoidable, in a situation where estimates of effect are very small, this element of uncertainty should also be fully acknowledged rather than downplayed.

Minor comments:
1. p. 237. Attempts to justify the use of "best" lags are ill-advised.
2. p. 285. The use of combined estimates from NMMAPS, given the heterogeneity in individual estimates of effect, needs to be justified.

Chapter 7 (Toxicolology)
The summary section is refreshing in appropriately qualifying the conclusions given some of the limitations in the studies reviewed. The summary gets distracted in going down the list of particle components and loses what is, for better or worse, the primary goal of toxicology at this point in time which is to provide biological plausibility for the observational findings (dose-response and mechanisms are, for now, secondary issues). While the summary list is helpful, the chapter needs to "cut bait" on the issue as to whether plausibility has in fact been enhanced by the recent findings. I believe that it has.

Chapter 5 (Exposure)
An important point is made in the bullet points of the summary section (p.108): "Multi-pollutant personal exposure studies have suggested that ambient concentrations of gaseous co-pollutants serve as surrogates of personal exposure to particles rather than as confounders." Since this is an important, and controversial, point, the bullet should refer to a fuller discussion of this issue in chapter 8 (see point 4 in chapter 8 critique, above). The current chapter is not able to do justice to (nor should it attempt to) the implications of the statement.

Dr. Barbara Zielinska

Chapter 2: Physics, Chemistry, and Measurement of Particulate Matter

In my opinion, this chapter represents a significant improvement over the previous version. Overall, the authors have organized the discussion in a more coherent manner, removed the repetitions and referenced several recent papers relevant to this chapter. I have only a few minor specific comments:

There are still some repetitions throughout the chapter. For example, the discussion about organic and elemental carbon measurement methods (Section 2.2.7) on page 2-88 to 2-97 describes the TOT and TOR methods first on page 2-89-90 and then again on 2-91-92. This should be consolidated and clarified. These two methods are the most commonly used in the US, so it is important that the differences between them are highlighted and made clear to the reader. The implications for data analysis, mentioned on page 2-97 (line 24-27), are very significant and should be emphasized in the summary section.

The authors discuss the sources of nuclei-mode particles on page 2-18 and mention secondary organic compounds as the only source of organic nuclei mode particle. I think it would be worthwhile to mention the recent results of Kittelson and co-workers (reported during several recent conferences) indicating that heavy hydrocarbons originating from lubricating oil and/or diesel fuel form nano-particle when directly emitted in vehicle exhaust.

Section 2.2.2.3, page 2-35, line 6: Fine mode particles are not necessary smooth droplets (for example, diesel particles).

Page 2-41, line 10-22. Although mass balance (comparison of the gravimetrically determined mass with the mass calculated from the sum of the major chemical components) is a useful validation technique, it has a major uncertainty due to the conversion factor from OC to organic mass, assumed to be 1.4. In addition, OC/EC is determined from a quartz filter that is prone to a positive sampling artifact (adsorption of organic gases during sample collection due to the large specific surface area), and gravimetric mass is usually measured using a Teflon filter, which has much smaller exposed surface area and is thought not to adsorb organic gases.

There is a difference between the "adsorption" and "absorption" term. The first relates to the physical adhering of gases to active surfaces, whereas the other involves sorption of gases into a liquid or amorphous surface layer (see, for example, Pankow, 1994). It would be desirable if the authors use these terms accordingly in the text, not just randomly.

Page 2-50, line 19-21. This sentence is not clear – what the authors mean "by this process"? Any references supporting this statement?

Page 2-57, line 5-10. This sentence should be corrected, I think the authors mean: … (NOx, HNO_3, etc.) by reaction or adsorption (but not absorption). Also, is the charcoal denuder effective in eliminating NO_2?

The minor editorial corrections are as follows:

Page 2-3, line 14, "a sensing volume"?

Page 2-66, line 30: cut, not "out"

Page 2-71, line 13: "impactor"

Page 2-78, line 29-30: remove "and (4)" and add "by"

Page 2-89, line 10: remove "and"

Page 2-90, line 31: "CC" or rather OC?

Page 2-93, line 4: it should be "nonoxidizing atmosphere"
Page 2-107, line 25: it should be 40 ngm^{-3}.
Page 2-109, line 20: "using"

References:
Pankow, J.F., 1994. *Atmospheric Environment*, **28**, 185-188

Chapter 3: Concentrations, Sources, and Emissions of Atmospheric Particulate Matter

Although this chapter represents a significant improvement over the previous version, there are some general issues that need to be addressed, as follows:

There are some ambiguous statements in this chapter concerning primary versus secondary $PM_{2.5}$. The authors state in Section 3.3, page 3-56 "...most of the ambient $PM_{2.5}$ has been formed in the atmosphere…" and in the Summary (page 3-92) "Depending on the origin of OC in ambient samples, $PM_{2.5}$, on average, may also be dominated by secondary components throughout the rest of the United States". Whereas this is certainly true for sulfates and nitrates, it has not been proven for organic carbon. Although various environmental chamber studies undoubtedly showed the formation of secondary aerosol from primary organic compounds (as the authors discuss in Section 3.3.1), the relative importance of these processes in ambient air, especially in different urban environments is not yet clear. Numerous SOPM compounds have been identified in laboratory studies but only a few of them (mostly biogenics) have been identified in ambient air. Some studies (for example NFRAQS, Watson et al., 1998, or Kao and Friedlander, 1995, and Pinto et al, 1995, discussed on page 3-55) showed that almost all urban OC could be accounted for by primary emissions. Some others (like Schauer et al, 1996, 2000, cited in this chapter) assigned the upper limit to secondary organic aerosol based on the remaining OC that has not been apportioned to the primary sources.

The authors include a brief discussion about secondary nitrate and sulfate formation in Section 3.3.1. It would be nice to cite some references (for a potentially interested reader) that discuss this subject in more details, including the latest papers.

Section 3.2 discusses briefly definitions of background $PM_{2.5}$ concentrations; however, there is no conclusion which definition is recommended by the authors for the average background concentrations required for PM risk analysis.

Specific minor comments include:
Page 3-41, line 26: values of r range….
Page 3-43, line 24: contribution from local sources…
Page 3-44, line 29: remove "to"
Page 3-58, line 19: "…can be viewed..."
Page 3-61, line 13: …with two double bonds (not bounds) or cyclic olefins…
Page 3-66, line 1-2: The statement that the reactions of organic compounds either in particle or on the surface of particles have only recently come under study is not correct. There was a lot of work done in 1980s (by such researches as Hites, Kamens, Nilsen, Pitts and other) and 1990s (Jang and McDow, 1995; McDow et al., 1994; Odum et al., 1994).
Page 3-66, line 22: octanal or hexanal, but not octanol.
Page 3-67 line 24-25: wall reactions are not the only uncertainties of smog chamber results. The others include sampling artifacts, unrealistically high concentrations of reactants, etc.

Page 3-68, line 1-8: It has been known since some time that a "rough" handling of conifers induces higher terpene emissions (see, for example, Juuti et al., 1990; Corchnoy et al., 1992).
Page 3-80, line 1 -2: what is #5 source types? This sentence is not clear.
Appendix 3C: The title of Table 3C-1 says that study published after 1995 are cited, but the table still include the older references
Appendix 3D: Footnote to Table 3D-7: Both papers reported OC and EC.

Some references:

Corchnoy et al., 1992: *Atmospheric Environment*, **26B**, 339
Juuti et al., 1990: *J. Geophys. Res*., **95,** 7515
Jang and. McDow,1995. *Environ. Sci. Technol*, **29**, 2654.
McDow, et al.,1994 *Environ. Sci. Technol*., **28**, 2147.
Odum et al, 1994 *Environ. Sci. Technol*, **28**, 1285.

Chapter 5: Human Exposure to Particulate Matter and Its Constituents

This chapter presents a comprehensive review of the indoor and outdoor exposure studies and discusses the important limitation in the data interpretation. However, I have some general comments concerning several issues discussed in this chapter:

Some of the conclusions in the Summary Section do not really follow from the discussion in the remaining part of this chapter. For example, on page 5-107, lines 5-11, the authors state that PM mass concentrations, especially fine PM are typically distributed uniformly in most metropolitan areas, thus reducing the potential for exposure misclassification. However, Section 5.4.3.2.1 (page 5-53) acknowledges that data presented in Chapter 3 (Section 3.3.3) indicate that ambient gradients of PM and its constituents exist in urban areas.

The authors argue that the non-ambient portion of personal exposure to PM is not expected to contribute to the relative risk estimates determined by regression (in a non-threshold linear model) of health response to ambient PM concentration (page 5-107, lines 29-31, page 5-93, line 1-7). However, is this assumption also correct if the threshold is present? What if the non-ambient portion of personal PM exposure is significantly more toxic than ambient portion?

I'm not convinced that indoor sulfates could be used as a surrogate for outdoor PM concentration in indoor environment (Section 5.3.2.3.2). The indoor sulfates may also originate from water (used in ambient humidifiers or dispersed during showering).

A general comment regarding Section 5.6 that discusses the potential sources of errors: are the recent findings regarding errors in the statistical programs used for epidemiological studies going to affect the discussion in this section?

In addition, I have a few specific minor comments, as follows:
Page 5-2, line 21: change the word order "…those chemical constituents from various sources that are responsible…"
Page 5-23, Table 5-4: is the study period reported by Janssen et al., (1999a), really completely unknown?
Page 5-27, Table 5-5: last "summary of measurements" should read $PM_{2.5}$.
Page 5-43, line 11, Amsterdam is in Holland, not in Finland.

Page 5-44, line 11-12: based on the data in Table 5-8, Fresno outdoor $PM_{2.5}$ concentrations are lower in the spring than in the winter. Also, why "Winter" is capitalized?
Page 5-45, line 6: "…have been influenced…"
Page 5-59, line 2: during fall?
Page 5-65, line 1: by a factor of 3?
Page 5-81, line 3: it should be NH_4^+ and NO_3^-
Page 5-93, line 6: "health"
Page 5-93, line 29: "…it would then be…"

Dr. Jane Koenig

Chapter 8

My comments are based on the chapter as written and on the discussions on July 18/19.

My overall assessment of Ch 8 is that, while it certainly is comprehensive and reflects a tremendous amount of effort, it is simply too long to serve as a useful document. Perhaps the length of the CD was discussed after I left the meeting (3 pm on Friday). With respect to this and since the committee decided to select 20 articles reporting PM exposure and mortality as representative of the literature, I suggest selecting a similar number of articles reporting PM exposure and indicators of morbidity. As I stated at the meeting, it is my opinion that PM effects on morbidity, along with the cohort and case-crossover mortality studies, are sufficient to support a NAAQS based on PM2.5. In terms of the public health impact, PM-induced morbidity, due to sheer numbers of individuals involved, presents a greater threat than PM-induced mortality. I can understand the emphasis on death, however I believe the NAAQS for ozone is based solely on morbidity outcomes. Hence here is my summary of important morbidity studies along with the PM2.5 concentration associated with them. (PM2.5 often must be estimated from PM10 due to lack of monitors.) I also have some general comments.

Cardiac Effects

Hospital admissions

The CD cites no studies of associations between daily PM2.5 and hospital admissions that are significant. Table 8-17 lists 6 studies that show significant increases in hospital admissions associated with daily PM10. In most of these cities, I judge that PM10 would be dominated by PM2.5. If one accepts a conservative ratio of 2.5/10 of 50%, the studies in this table show effects in the range of estimated PM2.5 equal to 11-23 mg/m3, well below the current 24 hr PM2.5 standard. The percent increases in admissions given in Table 8-17 range from 3.3 to 6.1%, thus it is predicted that reanalyzing with GAM using the strict convergence command that they would remain positive. These studies are important and should be highlighted as well as the Lippmann (2000) described on page 8-116.

Heart rate variability

Studies that were omitted from this section:

Magari et al. (May 2000) . The association between personal measurements of environmental exposure to particulates and HRV. Epidemiology 13: 305-310, 2002./ Study included continuous exposure and cardiac measures. 1.4% decrease in 5 min SDNN in healthy subjects for a 3 hr average PM2.5.

Sullivan et al (April 2001, abstract) Seattle panel study. A 15% decrease in log-transformed high frequency (HF) power for a 10 ug/m3 increase in PM2.5 in healthy subject over 65 years of age. Am J Respir Crit Care Med 163: A236, 2001. Also Sullivan et at (April 2002, abstract) reported a 25% decrease in HF power for a 10 ug/m3 increase in PM2.5 measured outside the home in subjects with cardiovascular disease (Am J Respir Crit Care Med 165: A304, 2002). Along with the Liao, Pope and Gold studies cited in the CD, it is my opinion that there appears to be a relationship between HRV and PM2.5 at levels in the range of 32 mg/m3 or less.

Blood biomarkers of inflammation or thrombosis
The CD cites several reports of increases in these markers. Although there are not numerous studies, investigators have only recently realized the importance of these cardiac outcomes. Increases in fibrinogen associated with PM exposure has been reported by Prescott et al (2000), Pekkanen et al (2000), Peters et al (2001), and Schwartz et al (2001). Increases in C-reactive protein associated with PM have been reported by Seaton et al (2001) and found in the Seattle panel study (unpublished data).

Section 8.3.1.4.
The first sentence is misleading. Only the hospital admits studies are ecological (and perhaps some of the blood work). The HRV and some blood studies are panel studies usually analyzed using mixed effects models with random intercept.
Actually a summary of the subject characteristics of responders and nonresponders in these varied studies would be very useful. I realize that this is beyond the scope of the CD but perhaps EPA could initiate such an effort.

Respiratory Effects

<u>Hospital admissions</u>

The CD does cite studies of PM2.5/hospital admissions. The summary table on page 151 should be moved to page 139 or so, otherwise the reader will stumble through until page 151 without it. . Studies in both Seattle and Brisbane reported significant associations with fine particles. Sheppard et al (1999) reported associations with PM10. PM2.5 and CF lagged 1 day (4-5 % increase for IQR of 19, 11.8 and 9.3 ug/m3 respectively). Moolgavkar et al (2000) also reported associations between PM2.5 and hospital admissions for respiratory disease in Seattle (data given in the CD). Interestingly, Brisbane is similar to Seattle in terms of meteorology - according to David Bates. Lippmann et al (2000) reported associations with admissions for pneumonia and PM2.5 for lag days 1, 2, and 3 suggesting a robust relationship. COPD admissions were significant only on lag day 3. (This study supports the hypothesis that respiratory disease has longer lags from air pollution exposure than CV disease.)

I realize that most of these studies were analyzed using GAM methods and therefore may overstate the risks. However, we have found in our analyses of mortality in Phoenix that the difference in RR calculated with GAM or GLM range from none to very small. See attached Table.

Medial visit Studies

I rate these studies as important for consideration of a 24 hr standard. Visits to ED that were significantly associated with PM2.5 are summarized in section 8.3.2.4. Significant effects were seen at mean PM2.5 concentrations of 12 ug/m3 (yearly) for Seattle and 12.2 for Montreal.

The comment on page 8-152, line 3, regarding the underestimation of asthma aggravation when relying on hospital data, is very important.

Lung function and Respiratory symptoms

No US studies are listed in Table 8-23. Unfortunately PEF measurements are being used less do to the quality assurance problems with uncoached pulmonary function measurements. I would think that the Vedal (1998) study would be listed here as children with asthma were studies. The decrement in PEF associated with PM10 was significant.

Discussion

This section is generally well written and it is obvious a tremendous amount of effort was expended on it.

However, regarding this section, I think we need to review the purpose of the CD. I thought it was basically a summary of the relevant literature. I thought didactic material and editorial interpretation were reserved for the Staff Paper.

P 8-174 The list of concerns that the reader should attempt to follow and glean information from is useful but I find this lengthy section very repetitive and judgmental. I think a summary of new research in each area--confounding, lags, etc should be 1-2 pages at best. (and of course some of this material, if left in the CD, would need to be updated re Splus).

8-188 figures need more complete legends. Are the upper and lower lines in each case the CI. If so should be dashed as in figure 22. I don't know if these impart enough information to warrant their inclusion.

In Seattle ozone is only measured from May to Sept so I don't know how the plots were generated for that city.

line 1. The studies do not need to be described here. That description is included in the earlier pages. The document really needs to cut repetitive and duplicative material. This may be a problem of multiple authors but it makes reading very tedious.

Do we really have enough confidence in MP models to develop this precise modeling? Or is it more informative to compare cities with different pollution mixes or changes in pollution over time. Eg Schwartz Occup Environ Med 2000; 57: 692-697 for SO2 and PM in Philadelphia.

I don't think Table 8-34 is useful.

8-4.2.2 Assessment of MP models. What were the last 20 some pages?!!

Pages 8-173 to 196 apparently are considered an Introduction to the following material. This document really needs to be pared down into a concise document that that can be accessed easily.

8-197 and 198
ozone in Samet 2000b is described on line 27 (197) as showing slight trend to increasing the variance and on line 11 has being responsible for a considerable reduction in effect size. Is that consistent?
Table 8-35. Some New ….
In my opinion, it is not appropriate to add new data to a discussion section. And these studies aren't new anyway?
8-200 line 21. Should say It is unlikely that the measured gaseous …
line 23. Why has the document switched to the use of "circulatory" mortality? It had been described as cardiovascular. We must be consistent.
Bottom of the page. More discussion of Samet 2000b. Can't the discussion of each study be consolidated?
The Janssen morbidity study is included in the middle of a discussion of mortality. I think this is confusing.
too much detail. I suggest just stating there is uncertainly but it is balanced by coherence among many health endpoints.
Isn't the Sarnet work presented earlier in Ch 5?? I would cut out the description, refer to ch5 and just make the points you wish to make. 1-2 pages
Proximity to sources isn't really relevant to ecological studies--
Figure 8-27 Label X axis.
this discussion is good. Succinct!!
role of components. There has been discussion of this. Need to consolidate
I would work this into previous section

line 14 ff Mar has been discussed several times already, cant' there be a back referencing system.
This page is duplicative
Already have a CV and respiratory mortality section. Don't duplicate
If lung cancer is to be discussed, I think there should be a data section earlier. Same for Reproductive effects.
line 23-24 Add "were mostly limited to **lung function changes.** I think a phrase has been deleted. Also on line 26--add hospital admission studies.
Ff this entire section needs to be reduced to one page or less. Just state what is important--details have been given earlier.

As I stated this is a comprehensive and thorough summary of a wealth of material. I really do believe that tightening the document up would serve several purposes. It would force the authors to emphasis the most important material and it would give the reader a document that could be comprehended. Four areas of extreme importance are the role of gaseous confounders, the evidence for cancer and reproductive effects, the form of the standard--especially the averaging time, and the role of bioaerosols.

More specific comments
Lippmann (2000) study not listed in Table 8-17.

line 20. List the biomarkers and give data for CRP.
Line 28 give the data
line 6-7 Give data

line 14 It appears that CV -PM lag days are often 0 or 1, whereas respiratory-PM lags are longer (2,3 and 4). This could be discussed.
lines 17-23, cite Mar et al. Laden et al and any other pertinent studies.

list years of study. Helps reader interpret the PM means and daily maxs listed.

8-138 section beginning on line 10. Why not have a table of hospital admits for respiratory as was done for CV. The Sheppard study reported asthma hospitalization associated withPM10; PM2.5; and PMcf.

Important error. The Norris study found a relative risk of 1. 15 for an 11 ug/m3 increase in PM2.5 in the Seattle ED study (1999). This error showed up in the 2nd draft. I thought I had included it in my remarks last time, but apparently I did not. The RR needs to be corrected in Figure 9-28 as well. Also needs correction in Table 8-22 and Figure 8-13.

143 line 23. I agree that statistical significance is over used. In terms of biology, there must be very little difference of effects describes as a risk of 1.1 (1.05 -3.0) versus 1.1 (0.98- 2.5). I think consistent trends on 2 or 3 lag days is more educational about effects than the absolute risk calculations.

Table 1. Analysis of mortality and air pollution in Phoenix--unpublished data. A comparison of GLM and GAM with strict convergence. Therese Mar, Tim Larson, and Jane Koenig, University of Washington

pollutant	lag	B	SE	T	75th	25th	inter range	RR	LCI	UCI
PM10(GLM)										
	0	0.001	0.001	1.408	56.509	32.546	23.963	1.025	0.990	1.062
	1	0.002	0.001	2.323	56.509	32.546	23.963	1.041	1.006	1.076
	2	0.001	0.001	1.516	56.509	32.546	23.963	1.026	0.992	1.061
	3	0.000	0.001	-0.525	56.509	32.546	23.963	0.991	0.958	1.025
	4	0.000	0.001	0.192	56.509	32.546	23.963	1.003	0.971	1.037
	5	0.001	0.001	0.816	56.509	32.546	23.963	1.014	0.981	1.048
PM10 (GAM)	0	0.001	0.001	1.925	56.509	32.546	23.963	1.028	0.999	1.057
	1	0.002	0.001	2.992	56.509	32.546	23.963	1.042	1.014	1.071
	2	0.001	0.001	1.572	56.509	32.546	23.963	1.022	0.995	1.050
	3	0.000	0.001	-0.349	56.509	32.546	23.963	0.995	0.968	1.023
	4	0.000	0.001	-0.411	56.509	32.546	23.963	0.994	0.967	1.022
	5	0.000	0.001	0.554	56.509	32.546	23.963	1.008	0.980	1.036

PM2.5(GLM)	0	0.001	0.002	0.669	17.328	8.494	8.834	1.012	0.977	1.048
	1	0.004	0.002	2.123	17.328	8.494	8.834	1.039	1.003	1.076
	2	0.003	0.002	1.637	17.328	8.494	8.834	1.030	0.994	1.068
	3	0.000	0.002	0.074	17.328	8.494	8.834	1.001	0.966	1.038
	4	-0.001	0.002	-0.335	17.328	8.494	8.834	0.994	0.959	1.030
	5	0.001	0.002	0.484	17.328	8.494	8.834	1.009	0.973	1.046
PM2.5(GAM)		0.002	0.002	1.210	17.328	8.494	8.834	1.018	0.989	1.047
		0.004	0.002	2.664	17.328	8.494	8.834	1.039	1.010	1.068
		0.003	0.002	1.764	17.328	8.494	8.834	1.026	0.997	1.055
		0.001	0.002	0.898	17.328	8.494	8.834	1.013	0.985	1.042
		-0.001	0.002	-0.361	17.328	8.494	8.834	0.995	0.967	1.023
PMCF(GLM)	0	0.001	0.001	1.467	40.364	23.236	17.128	1.025	0.992	1.060
	1	0.002	0.001	2.186	40.364	23.236	17.128	1.036	1.004	1.070
	2	0.001	0.001	1.355	40.364	23.236	17.128	1.022	0.990	1.055
	3	-0.001	0.001	-0.803	40.364	23.236	17.128	0.987	0.956	1.019
	4	0.000	0.001	0.398	40.364	23.236	17.128	1.006	0.976	1.038
	5	0.001	0.001	0.591	40.364	23.236	17.128	1.009	0.979	1.041
PMCF(GAM)	0	0.002	0.001	1.901	40.364	23.236	17.128	1.027	0.999	1.056
	1	0.002	0.001	2.743	40.364	23.236	17.128	1.038	1.011	1.066
	2	0.001	0.001	1.481	40.364	23.236	17.128	1.020	0.993	1.048
	3	-0.001	0.001	-0.853	40.364	23.236	17.128	0.988	0.962	1.015
	4	0.000	0.001	-0.232	40.364	23.236	17.128	0.997	0.970	1.024
	5	0.000	0.001	0.144	40.364	23.236	17.128	1.002	0.975	1.030
diesel (GLM)	0	0.000	0.000	-1.515	2858.879	616.958	2241.921	0.973	0.939	1.008
	1	0.000	0.000	0.118	2858.879	616.958	2241.921	1.002	0.967	1.038
	2	0.000	0.000	-0.956	2858.879	616.958	2241.921	0.982	0.947	1.019
	3	0.000	0.000	-0.863	2858.879	616.958	2241.921	0.984	0.949	1.020
	4	0.000	0.000	2.078	2858.879	616.958	2241.921	1.037	1.002	1.074
	5	0.000	0.000	-0.092	2858.879	616.958	2241.921	0.998	0.964	1.034
veg burning (GLM)	0	0.000	0.000	1.497	1596.329	323.730	1272.599	1.020	0.994	1.047
	1	0.000	0.000	1.152	1596.329	323.730	1272.599	1.016	0.989	1.044
	2	0.000	0.000	0.361	1596.329	323.730	1272.599	1.005	0.978	1.033
	3	0.000	0.000	1.443	1596.329	323.730	1272.599	1.019	0.993	1.045
	4	0.000	0.000	-0.478	1596.329	323.730	1272.599	0.994	0.967	1.020
	5	0.000	0.000	1.139	1596.329	323.730	1272.599	1.015	0.989	1.043

secondary (GLM)	0	0.000	0.000	1.139	3272.503	1205.754	2066.749	1.025	0.982	1.070
	1	0.000	0.000	0.888	3272.503	1205.754	2066.749	1.022	0.974	1.072
	2	0.000	0.000	0.613	3272.503	1205.754	2066.749	1.015	0.968	1.063
	3	0.000	0.000	-0.492	3272.503	1205.754	2066.749	0.989	0.944	1.035
	4	0.000	0.000	-0.551	3272.503	1205.754	2066.749	0.987	0.944	1.033
	5	0.000	0.000	-1.938	3272.503	1205.754	2066.749	0.956	0.914	1.001
motor (GLM)	0	0.000	0.000	-0.424	5759.407	1312.399	4447.008	0.989	0.941	1.040
	1	0.000	0.000	1.324	5759.407	1312.399	4447.008	1.034	0.984	1.086
	2	0.000	0.000	2.118	5759.407	1312.399	4447.008	1.054	1.004	1.107
	3	0.000	0.000	0.382	5759.407	1312.399	4447.008	1.010	0.961	1.061
	4	0.000	0.000	-0.282	5759.407	1312.399	4447.008	0.993	0.944	1.044
	5	0.000	0.000	-0.195	5759.407	1312.399	4447.008	0.995	0.946	1.047
crustal (GLM)	0	0.000	0.000	-1.334	4097.649	1099.493	2998.156	0.968	0.922	1.016
	1	0.000	0.000	-0.551	4097.649	1099.493	2998.156	0.987	0.941	1.035
	2	0.000	0.000	0.178	4097.649	1099.493	2998.156	1.004	0.960	1.050
	3	0.000	0.000	-2.199	4097.649	1099.493	2998.156	0.952	0.912	0.995
	4	0.000	0.000	-1.345	4097.649	1099.493	2998.156	0.971	0.930	1.014
	5	0.000	0.000	-0.493	4097.649	1099.493	2998.156	0.989	0.948	1.032
EC (GLM)	0	-0.015	0.018	-0.814	1.747	0.579	1.168	0.983	0.943	1.024
	1	0.009	0.018	0.482	1.747	0.579	1.168	1.010	0.969	1.053
	2	0.024	0.018	1.299	1.747	0.579	1.168	1.028	0.986	1.072
	3	-0.004	0.019	-0.234	1.747	0.579	1.168	0.995	0.952	1.039
	4	-0.001	0.019	-0.069	1.747	0.579	1.168	0.999	0.957	1.042
	5	-0.012	0.019	-0.649	1.747	0.579	1.168	0.986	0.945	1.029
motor+diesel (GLM)	0	0.000	0.000	-1.123	8647.420	2418.142	6229.278	0.971	0.921	1.023
	1	0.000	0.000	1.063	8647.420	2418.142	6229.278	1.029	0.976	1.085
	2	0.000	0.000	1.137	8647.420	2418.142	6229.278	1.032	0.978	1.089
	3	0.000	0.000	-0.175	8647.420	2418.142	6229.278	0.995	0.943	1.051
	4	0.000	0.000	0.910	8647.420	2418.142	6229.278	1.025	0.972	1.081
	5	0.000	0.000	-0.198	8647.420	2418.142	6229.278	0.995	0.943	1.049

Chapter 9
Generally, Ch 9 is well written and easy to read.

I am glad to see that the category of biomass burning has been added to Table 9-2.

useful discussion of receptor models

requirements that must be met in order for a gaseous co-pollutant to be a contributor to health effects; Not certain that this statement is correct; gaseous co pollutant must be correlated with personal exposures to that gaseous pollutant.

NO2 as a co-pollutant. Not measured carefully enough to determine this yet.

Add oxidants to Table 9-7.

line 11. Data -verb tense error

Norris 2000 data are not accurate. That study found PM10 and ED asthma visits associated with a RR of 1.14 (1.05-1.23).

9-125 Probably should not give a value for asthma reaction lag times. I don't think it is known with any degree of accuracy.

Dr. Allan Legge

CHAPTER 3. Concentrations, Sources, and Emissions of Atmospheric Particulate Matter
Overall Comment:

It is clear from this Chapter that despite massive efforts that the measurement of PM size fractions on a national scale to permit calculation of national trends is still a work in progress. While it is true that significant progress has been made in the measurement of $PM_{2.5}$, the measurement of $PM_{10-2.5}$ has been limited. Given the uncertainties associated with the measurement of PM_{10} and $PM_{2.5}$, one must strongly question the advisability of calculating $PM_{10-2.5}$ mass as the difference between PM_{10} and $PM_{2.5}$ mass and then use these data to try and draw meaningful conclusions regarding potential adverse health outcomes. It is also apparent that the chemical characterization of PM is still limited especially for organic compounds. Although not stated, one point is clear and that is that not all PM mass of a given size fraction is created equally. It is clear that chemical speciation is required.

It is strongly recommended that 'future research needs' be identified while still fresh in the minds of the authors to aid in the research planning process.

Specific Comments:
1. Pages 3-11 to 3-12 Background $PM_{2.5}$ Concentrations

Given the variability which has been found for 'background' $PM_{2.5}$ and PM_{10} concentrations regardless as to how 'background' has been defined, the use of an 'annual

average natural background concentration' is questionable. It would be more appropriate to use a seasonally adjusted 'background'.

2. Page 3-13, line 25.

Has 'Pinto et al.,2002' been accepted for publication. It is currently listed as 'submitted'.

3. Page 3-56, lines 4-6 and Page 3-75, Table 3-8.

It is noted that the major sources of each constituent in Table 3-8 are shown in boldface type. This is not the case.

4. Page 3-59,lines 16-18.

It is noted that 'At night, NO_2 also is oxidized to nitric acid by a sequence of reactions initiated by O_3 ----.' While this can be true, O_3 is commonly lower at night and HONO usually dominates.

5. Page 3-68, lines 1-8.

This paragraph needs to be rethought in the light of the paper by Geron et al. (2001). They showed that isoprene emission rates are 2-28 times higher than previously reported for selected North American tree species. Their approach was to measure 'isoprene emission capacity' under standard conditions. Since Khalil and Rasmussen (1992) did not follow this approach this could explain their lower emission rates. Given the importance of the isoprene emission inventory, it is critical that the role of trees be accurately portrayed.

Geron, C.,Harley, P. and Guenther,A. 2001.
Isoprene emission capacity for US tree species.
Atmospheric Environment 35: 3341-3352.

6. Page 3-75, line 25.

Should read ' published that provide ----- receptor models than'

7. Pages 3-78 to 3-79, Table 3-10.

Footnote notations for y and z are missing but are noted with first and second Sample Sites. Notations l, p and q are missing from the body of the Table.

8. Page 3-80, lines 1-2.

Reference is made to #5 source type in Table 3-9 and 3-10. The source types are not numbered in either table.

9. Page 3-80, lines 17-18.

It is noted that 'In Table 3-10, primary motor vehicle exhaust contributions account for up to 40% of average PM_{10} at many sampling sites.' Longbeach, CA and Follansbee, WV are 44.5% and 53% respectively while most other values are well below 40%. The sentence needs to be reworded.

10. Pages 3-91 to 3-94, 3.4 Summary and Conclusions

The matter of uncertainties in 'emission inventories' is missing.

11. Appendix 3B

i) Page 3B-1, lines 7-9 and page 3B-2, Table 3B-1.

It is noted that sampling lasted from 'February through July, 2000' in the text but Table 3B-1 indicates February through June 2000. Which is correct?

ii) Page 3B-2, Table 3B-1.

The cities of Phoenix and Portland are missing.

iii) Why is the MDL in many places in the 'Speciation Tables' higher than the Min?

Chapter 4. Environmental Effects of Particulate Matter

Overall Comment:

This Chapter reflects substantial improvement over the Second draft of the PM CD. While there are elements of repetition in the text, this does not detract from the content which is thorough and well balanced. It is clear from the text that PM size fraction mass is of little use when trying to assess potential impact of PM on vegetation and/or ecosystems. Chemical speciation is required. The recognition that PM can be a complex chemical mixture and that this mixture can interact with other air pollutants including 'organics' (i.e. multiple stresses) in potentially causing vegetation and/or ecosystem stress is important. One needs to view total exposure and not just exposure to a given pollutant and/or agent in isolation. The notion that the impact of PM on vegetation and/or ecosystems is cumulative in nature is also most important. This has a major bearing on the potential adverse consequences to both above and below ground ecosystems. There is thus merit in exploring the European concept of critical loads for application to PM in the future. The key will be determining the appropriate chemical end points and/or biological indicators. The following are a few references which will provide some guidance:

(1) Bull, K.R. Bull 1992.
An introduction to critical loads.
Environmental Pollution 77: 173-176.

(2) Bull, K.R. 1995.
Critical loads - possibilities and constraints.
Water, Air and Soil Pollution 85: 201-212.

(3) Posch,M.,de Smet,P.A.M., Herrelingh,J.-P. and Downing, R.J. (Editors). 2001.
Modelling and Mapping of Critical Thresholds in Europe: Status Report 2001. Working Group on Effects of the Convention on Long-range Transboundary Air Pollution, Coordination Center for Effects, National Institute for Public Health and the Environment, Bilthoven, The Netherlands, RIVM Report No. 259101010, ISBN No. 96-9690-092-7. 188pp.

(4) de Vries,W.,Posch,M.,Oja,T.,van Oene,H.,Kros, H.,Warfvinge,P. and Arp,P.A. 1995.
Modelling critical loads for the Solling spruce site.
Ecological Modelling 83: 283-293.

(5) Arp,P.A., Oja,T. And Marsh, M. 1996.
Calculating critical S and N loads and current exceedences for upland forests in southern Ontario, Canada.
Canadian Journal Forest Research 26: 696-709.

(6) Gunderson,P., Callesen, I. And de Vries, W. 1996. 1998.
Nitrate leaching in forest ecosystems is related to forest floor C/N ratios.
Environmental Pollution 102,S1:403-407.

(7) Smith,V.A., Tilman, G.D. and Nekola, J.C. 1999.
Eutrophication: impacts of excess nutrient inputs on freshwater, marine, and terrestrial ecosystems.
Environmental Pollution 100: 179-196.

(8) Strickland,T.C.,Holden Jr.,G.R.,Ringold, P.L.,Bernhard,D.,Smythe and Fallon,W. 1993.

A national critical loads framework for atmospheric deposition effects assessment: I. Method summary.
Environmental Management 17(3): 329-334.
(9) Hunsaker,C.,Graham,R.,Turner,R.S.,Ringold,P.L.,Holden Jr.,G.R. and Strickland,T.C. 1993.
A national critical loads framework for atmospheric deposition effects assessment: II. Defining assessment endpoints, indicators, and functional subregions.
Environmental Management 17(3): 335-341.
(10) Hicks,B., McMillen,R., Turner,R.S., Holden Jr., G.R. and Strickland,T.C. 1993.
A national critical loads framework for atmospheric deposition effects assessment: Deposition characterization.
Environmental Management 17(3): 343-353.
(11) Holden Jr.,G.R., Strickland, T.C., Cosby,B.J., Marmorek,D., Bernhard, D., Santore,R., Driscoll,C.T., Pardo,L., Hunsaker,C., Turner,R.S. 1993.
A national critical loads framework for atmospheric deposition effects assessment: Model selection, applications, and critical loads mapping.
Environmental Management 17(3): 355-363.

It is strongly recommended that 'future research needs' be identified while still in the minds of the authors to aid in the research planning process.

Specific Comments:

1) Page 4-22, lines 6-18.

The issue of forest edges as efficient sinks for dry deposition is discussed. An additional supporting recent reference is Weathers et al. (2001). The citation is as follows:

Weathers, K.C.,Cadenasso and Pickett, S.T.A. 2001.
Forest edges as nutrient and pollutant concentrators: potential synergisms between fragmentation, forest canopies, and the atmosphere.
Conservation Biology 15(6): 1506-1514.

2) Page 4-42, lines 1-2.

It is stated that 'Preferential association of heavy metals with fine particles allows these particles to escape emission controls.' While this is true to a point, it is important to note that the issue is rather one of efficiency of control. One does not mean to imply that fine particle emissions are uncontrolled.

3) Page 4-57, lines 11-12.

It is stated that ' —, acid rain causes not only the aging of needles (which in northern conditions normally lasts from 11 to 14 years) to be shortened,----.' It is unclear what this means. It is suspected that the authors are referring to 'needle retention'.

4) Page 4-63, line 27.

The word 'Indirect' should be dropped from the section title to be consistent with the text.

Dr. Morton Lippmann

SUPPLEMENTAL REVIEW COMMENTS

Epidemiology

1. HEI's plans for extending the re-examination of the NMMAPS studies' concentration-response (CR) coefficients due to recently discovered problems in S-Plus Software and the expression of confidence intervals to other HEI-sponsored time-series research studies are to be commended. Time-series studies supported by other research sponsors that used S-Plus Software for GAM modelling can be expected to have similar problems, and the feasibility of the option of arranging for HEI to provide guidance for uniform protocols for recomputing the results of these other time-series studies whose initial findings were compromised by S-Plus and confidence interval errors should be vigorously examined. If the PI's of these other studies follows the HEI protocols for their re-analyses, and if HEI can attest that these re-analyses did, in fact, use their protocols, it would provide EPA with the best possible opportunity for evaluating the merits and utility of these results as a suitable scientific database for use in their determination of the need for changes in the 24-hr $PM_{2.5}$ NAAQS in a credible and timely manner.

2. Since the cohort mortality studies are not affected by the GAM problems, there is no need to delay the completion of PM CD's discussion of the annual mortality data and their implications to the adequacy to support retention and/or change in the annual $PM_{2.5}$ NAAQS. In completing its review and discussion of the new data, the CD should expand its discussion of the Pope *et al*. (2002) report of the significant association of excess lung cancer with $PM_{2.5}$.

Dosimetry

The discussion of non-uniform airway surface deposition of inhaled particles in Chapter 6 is limited to the tracheobronchial tree. The discussion needs to be expanded to the gas-exchange airways. In particular, the studies of Brody and Roe,((((1) Brody *et al.*,(2) and Warheit and Hartsky(3) should be cited that show deposition of the fine particles that penetrate to this lung region is highly concentrated on airway bifurcations and diminishes rapidly with airway generation, consistent with the concentration of flow streamlines near the bifurcations and the penetration depth of convective tidal flow.

These factors should be reiterated in the discussion of the differences in particle penetration to small airways and alveolar sacs between: 1) inhaled particles; and 2) instilled particle suspensions.

((((1) Brody, A.R., and M.W. Roe. 1983. Deposition pattern of inorganic particles at the alveolar level in the lungs of rats and mice. *Am. Rev. Respir. Dis.* 128:724-729.

(2) Brody, A.R., L.H. Hill, B. Adkins Jr., and R.W. O'Connor. 1981. Chrysotile asbestos inhalation in rats: Deposition pattern and reaction of alveolar epithelium and pulmonary macrophages. *Am. Rev. Respir. Dis*. 123:670-679.

(3) Warheit, D.B., and M.A. Hartsky. 1990. Species comparisons of alveolar deposition patterns of inhaled particles. *Exp. Lung Res.* 16:83-99.

Topics not Receiving Adequate Attention Throughout the PM CD

It must be remembered that the PM CD will need to provide the best possible coverage of scientific underpinnings for both $PM_{2.5}$ and $PM_{10-2.5}$ NAAQS decisions for both acute and chronic health endpoints. In this regard, there is insufficient discussion of scientific knowledge related to $PM_{10-2.5}$ for sources, ambient levels, measurement techniques, exposure assessment, and health effects in contrast to the wealth of relevant information on $PM_{2.5}$. With respect to the information needs for reconsideration of the 24-hour and annual PM NAAQS, the contrasts are less stark, but there is clearly much more information on, and discussion of short-term exposures and their acute health effects than on more cumulative exposures on the health effects associated with them. This disparity is unfortunate, since the current annual $PM_{2.5}$ NAAQS is the controlling limit in the vast majority of communities believed to be out of PM NAAQS compliance. Thus, it is important, during the completion of the PM CD, to more explicitly address both what is known and what remains unknown concerning long-term average ambient PM sources, concentrations, options for measurements of ambient concentrations, long-term particle retention and dosimetry, and health effects.

Chapter 1. Introduction

General Comment

This chapter provides a good description of the purpose of a PM CD, and the process by which it serves the development of a PM NAAQS.

Specific Comments

1. p. 1-2, line 25: insert "terms of mass concentrations into" before "two".
2. p. 1-2, line 26: insert "in aerodynamic diameter" after "2.5 ：m", and "aerodynamic" before "diameter".
3. p. 1-2, line 31: replace "of" with "determined by the standard TSP sampler's configuration as well as the wind speed and direction, which varied from".
4. p. 1-3, line 5: delete "inhalable".
5. p. 1-5, line 27: after "groups" insert "and through the coordination of EPA's own laboratories (NERL and NHEERL) PM research with those in the five academically-based Centers for PM Health Effects Research that are supported by EPA (SAB, 2002)".
6. p. 1-15, line 13: insert "PM concentrations measured using" before "stationary".
7. p. 1-16, line 4: change "the generation of" to "components that generate".

Chapter 2. Physics, Chemistry, and Measurement of PM

General Comments

This is an excellent chapter overall. It provides a thorough and clearly presented summary of the background on PM characteristics and methods of measurement for concentration by particle size category and chemical composition. The only suggestion that I offer for change relates to the comprehensive nature of the highly detailed discussions in Section 2.2.6 (Inorganic Elemental Analyses) and Section 2.2.9 (Continuous Monitoring). The main chapter text for these sections could be greatly condensed, and be supplemented by tables outlining the specific attributes of each method. The detailed text could then be included as Appendices to this

chapter. Much of this detail is not of critical importance to this round of review of the PM NAAQS.

Specific Comments

1. p. 2-18, lines 19-24: This description should be expended to acknowledge the ammonia neutralization of the acid.
2. p. 2-19, line 12: change "ammonia" to "ammonium ions".
3. p. 2-28, line 26: replace "and" with a comma, and insert "and for epidemiological studies" after "patterns".
4. p. 2-31, line 7: change "integrate" to "permit integration".
5. p. 2-31, line 9: insert "change in the atmosphere or" after "temperature".
6. p. 2-32, line 1: change "to" to ", and added" after "PM_{10}".
7. p. 2-32, line 2: insert "thoracic" before "PM".
8. p. 2-34, line 13: change "wind speed and direction" to "direction and relatively independent of wind speed".
9. p. 2-34, lines 25 & 26: change "upper part of the human respiratory system" to "human upper respiratory tract".
10. p. 2-35, line 8: insert "and retention" after "deposition".
11. p. 2-35, line 13: insert "and peroxides," before "while".
12. p. 2-38, line 13: insert "the" before "sampling".
13. p. 2-39, line 2: change "personal activity particles" to "particles originating from personal activity".
14. p. 2-39, line 8: insert "refinements of" before "older".
15. p. 2-43, line 13: change "affect" to "effect".
16. p. 2-77, lines 1-6: This discussion is much too brief, especially in relation to most of the other discussion items in this chapter. There is a lot more that could and should be said here about the current state-of-the-art of virtual impactors, and their potential application to the measurement of $PM_{10-2.5}$.
17. p. 2-79, line 3: What is the difference between 46.2 and 47 mm filters?
18. p. 2-91, line 1: delete "Recently".
19. p. 2-115, lines 7 & 8: change "in this size range" to "below 50 nm

Chapter 5. Human Exposure to PM and its Constituents

General Comments

This chapter provides a comprehensive and generally well balanced overview of human exposures to PM and its numerous components. However, the introduction and summary sections need to be better organized in recognition of the primary purpose of the CD, i.e., to establish a scientific literature base to be used in the establishment of a NAAQS for PM in ambient air. For example, the paragraph on p. 5-2, lines 3-11, should be moved to line 7 on p. 5-1. In Chapter 7 on epidemiology, most of the studies have utilized data relating community levels of airborne pollutants to population-based health statistics. They have had, of necessity, to rely on some simplifying assumptions regarding exposures. In this regard, it has generally been assumed that air pollutant concentrations measured at population oriented monitoring sites can serve as a surrogate index of the average personal exposure of the population, or that it can be related to the average exposures of selected target populations of interest by suitable adjustment factors such as percentage of times indoors and outdoors, penetration of outdoor pollutants into

indoor air, and residence time in indoor spaces.
A paragraph summarizing the above should follow the transposed paragraph that I want moved to the beginning of the Introduction.

Specific Comments

1. p. 5-1, line 10: insert "ideally" before "measured".
2. p. 5-1, line 14: insert "estimates based on" before "atmospheric"; "transport" before "models"; replace "and" with "combined with"; and insert "and clearance" before "models".
3. p. 5-1, line 20: delete "the" and change "link" to "links".
4. p. 5-2, line 17: insert "air" after "ambient".
5. p. 5-2, line 18: change "nonambient sources also" to "other microenvironments".
6. p. 5-3, line 25; and p. 5-4, line 1: insert "mass" after "PM".
7. p. 5-3, line 30; and p. 5-4, line 19: insert "air" after "ambient".
8. p. 5-5, line 28: change "particulate matter" to "PM mass".
9. p. 5-51, line 1; and p. 5-77, line 20: change "PM" to "PM_{10}".
10. p. 5-78, line 7: does this refer to $PM_{2.5}$ or PM_{10}?
11. p. 5-88, line 3: insert "If it is assumed that total PM exposure is responsible for the effects, then the" before "use".
12. p. 5-88, line 14: insert "total" before "PM".
13. p. 5-90, line 10: delete "Clearly".
14. p. 5-90, line 12: insert "may" before "provide".
15. p. 5-95, line 5: insert "such as $PM_{10-2.5}$," after "PM,".
16. p. 5-99, line 5: delete "ultrafine particle measures", since it is a highly time-dependent measure.
17. p. 5-102, line 10: transpose "PM" and "toxic".

Chapter 6. Dosimetry of Particulate Matter

General Comments

This chapter is much improved in comparison to the previous draft. It provides a thorough and dispassionate review of a complex literature review, and it is encouraging that much of the recent research cited is addressing critical unknowns with state-of-the-art measurement technologies and modelling approaches.

There are some redundancies, especially when some of the papers discussed apply to more than one subtopic in the chapter. Some of it can and should be reduced by cross-referencing the previous citation of the work rather than by repeating the description of what was done.

Specific Comments

1. p. 6-2, line 7: insert "within the respiratory tract" after "dosimetry".
2. p. 6-2, line 11: insert "in airway fluids and cellular components" after "solubility".
3. P. 6-2, line 22: insert "of particle suspensions" after "instillation".
4. p. 6-2, line 28: insert "component regions of" after "in".
5. p. 6-3, line 23: change "a size distribution parameter" to "size distribution parameters".
6. p. 6-4, line 22: insert "dichotomous" before "branching".
7. p. 6-4, line 24: insert "gas-exchange region, consisting of" before "respiratory".
8. p. 6-4, line 29: change "cells of" to "distribution of cells lining the".
9. p. 6-6, line 15: change "1 ：m" to "2 ：m".
10. p. 6-7, line 13: insert "highly" before "charged", and "initial" before "charge".

11. p. 6-8, line 26: insert "variability in air path dimensions and airway branching" after "interindividual".
12. p. 6-9, line 5: insert additional sentences: "Deposition never reaches zero because of mixing between particle-rich tidal air and nearly particle-free residual lung air. The particles in the tidal air remaining in the deep lung are gradually deposited".
13. p. 6-12, line 17: insert "as well as the cross-sectional area of the flow path" after "(Figure 6-14)".
14. p. 6-12, line 22: "in-vivo" should be italicized here and elsewhere (p. 6-30, l. 24).
15. p. 6-13, line 1: insert "of particle deposition" after "distribution".
16. p. 6-14, line 23: insert "hollow airway" before "cast".
17. p. 6-22, line 12: insert "thoracic" before "particles".
18. p. 6-23, line 1: delete "will".
19. p. 6-23, line 9: change "are" to "have been".
20. p. 6-23, line 19: insert ", conductive airway size," after "body size".
21. p. 6-24, line 11: insert "size" after "particle".
22. p. 6-26, line 16: change "but" to "and".
23. p. 6-31, lines 23-27: Was ethnicity itself a factor?
24. p. 6-32, lines 12-15: What was the message from this citation?
25. p. 6-33, line 7: transpose "compare" and "systemically".
26. p. 6-49, line 1: What was the particle size used in the Nemmar et al. 2002 paper?
27. p. 6-49, line 18: "clearance" is misspelled".
28. p. 6-49, line 22: What was the particle size?
29. p. 6-53, line 20: insert "and cigarette smoke" after "irritants".
30. p. 6-61, line 12: insert "which is equivalent to the ICRP (1994) model's ET region;" after "region".
31. p. 6-61, line 13: insert "which is equivalent to the ICRP (1994) model's A region." after "region".
32. p. 6-66, line 24: change "2 ：g" to "2 mg", and change "< 10 ：g" to "2 to 10 mg".
33. p. 6-67, line 1: insert "matter" after "particulate".

Chapter 7. Toxicology of PM in Humans and Lab Animals

General Comments

The text of the 3rd draft is much improved over the 2nd draft. However, now that the ACS cohort follow-up study (Pope et al., 2002) has shown excess lung cancer in relation to ambient average levels of PM 2.5, there will need to be some re-introduction of the toxicological data base on PM-induction or promotion of lung cancer. This need not, and should not, mean the restitution of most of the discussion of mutagenesis and high dose cancer exposure studies from the 2nd draft, but rather a selective discussion of mechanisms (e.g., mitogenesis) that may account for PM associations with lung cancer in humans.

Specific Comments

1. All of the numerous uses of "*in vitro*," "*in vivo*," "*ex vivo*," "*a priori*," should be italicized.
2. The "Lippmann, 2000" reference (p. 7-3, line 16) does not address "silica."
3. p. 7-3, line 28: insert "pure" before "sulfuric acid".

4. p. 7-3, line 29: change "emission bag room " to "emission source bag filters".
5. p. 7-5, Table 7-1, Col. 3: change "Niagra" to "Niagara".
6. p. 7-7, line 10: insert "and storage" after "deposition".
7. p. 7-16, line 18: insert "fibrous" before "glass".
8. p. 7-18, line 27: insert "additional" before data".
9. p. 7-19, line 6: "DPM" was not previously defined.
10. p. 7-27, lines 1-4: Contains repeated sentences from p. 7-25, lines 30 and 31.
11. p. 7-28, line 3: "causal components" for what?
12. p. 7-34, line 28: change "related to" to "that followed".
13. p. 7-38, line 15: change "ROFA studies" to "studies using ROFA".
14. p. 7-40, line 11: What does "greater than" refer to?
15. p. 7-40, line 29: Is the implication here that inhalation exposures at 15 mg/m^3 are "realistic"?
16. p. 7-47, line 16: insert "effects seen in" before "these".
17. p. 7-75, line 10: What about "zinc"?
18. p. 7-83, line 5: "Ottawa" was misspelled.
19. p. 7-84, line 4: change "particulates" to "particles".
20. p. 7-87, lines 13-14: The differences could also result from particle surface activity.
21. p. 7-88, lines 28-30: Why introduce the topic by identifying eight different UFP and then not discuss differential responses related to particle type?
22. p. 7-94, line 11: change "direct" to "consistent".
23. p. 7-96, line 13: change first "that" to "whose surface layers".
24. p. 7-104, line 1: suggest deleting "urban". The human effects of fine particles are certainly not limited to urban residents.
25. p. 7-105, line 2: The O_3 exposure used (120 ppb for 2 hrs) was not particularly high (especially for an exposure at rest).
26. p. 7-105, line 6: 1 ppm O_3 is definitely not a low concentration.
27. p. 7-107, line 10: What "one" was that?
28. p. 7-107, line 21: change "absorbed" to "adsorbed".
29. p. 7-108, lines 3 and 4: change "sulfuric acid ultrafine metal oxide" to "ultrafine sulfuric acid and metal oxide".
30. p. 7-110, line 6: change "problems" to "greater uncertainty to the interpretation of the findings".

Chapter 8. Epidemiology of Human Health Effects

General Comments

Recognizing that Chapter 8 will not be formally reviewed by CASAC before revisions are made to reflect anticipated revisions of many recent time-series analyses, I herewith offer some editorial suggestions and requests for clarifications noted in my initial reading and review of this chapter.

I also offer the general impression that this chapter needs considerable tightening. It gives much too much space to detailed reviews of papers that contribute little to the issues being addressed. It also contains redundant descriptions of papers that address more than one issue or topic. There should also be a more distinct separation and/or identification of the review of peer-reviewed scientific findings from the CD authors' interpretations, synthesis, and analyses.

Specific Comments

1. p. 8-2, line 21: change "is" to "are".
2. p. 8-3, line 8: insert "it may" after "as".
3. p. 8-3, lines 29-30: put "unlike a time-series analysis" within parentheses.
4. p. 8-4, line 13: change" gaseous criteria" to "other".
5. p. 8-5, line 5: insert "at least some" before "adverse".
6. p. 8-11, line 28: "1999 PM AQCD"?
7. p. 8-14, line 23: citation to a 1998 paper seems inappropriate in this section.
8. p. 8-22, Gwynn et al. (2000) Comments: The CoH exception should be noted.
9. p. 8-84, line 3: change "2000" to "2002".
10. p. 8-113, line 12: insert "%" after "6.8".
11. p. 8-117, line 13: change "0_3" to "O_3".
12. p. 8-117, line 16: insert "et al. 2000" after "Lippmann".
13. p. 8-129, line 6: What does "18[7]" signify?
14. p. 8-137, lines 1-2: What relation do the stated effect sizes have to Table 8-18?
15. p. 8-145, line 25: What are "asthma concentrations"?
16. p. 8-149, line 5: change "$N0_2$" to "NO_2".
17. p. 8-171, lines 28-29: clarify the nature of the "inconsistent results", especially in relation to the much greater power of the CHS study results.
18. p. 8-173, lines 1-6: ignoring the CHS findings in this paragraph is incomprehensible.
19. p. 8-182, line 1: change "below" to "Figures 8-16 through 8-20".
20. p. 8-188, Fig. 8-21: The figure legend is inadequate.
21. p. 8-208, line 23: here and on the following pages, who is "we"?
22. p. 8-210, line 26: insert "in" after "lived".
23. p. 8-216, Fig. 8-27: There is no scale on the abscissa".
24. p. 8-219, line 13: change "section" to "sections".
25. p. 8-221, line 3: insert "and the extended analyses of the ACS Study" before "substantiate".
26. p. 8-227, line 27: transpose "well" and "substantiate".
27. p. 8-229, line 15: change "15" to "16".
28. p. 8-236, line 28: shouldn't this line continue with a discussion of the CHS papers on this topic (Gauderman et al. and Avol et al.)?
29. p. 8-264, line 25: insert ", 2002" after "1995".
30. p. 8-264, line 26: change "provided" to "provide".
31. p. 8-265, line 22: insert "than" after "rather".
32. p. 8-276, line 20: change "$S0_2$" to "SO_2".
33. p. 8-277, line 8: Section 8.4.9.3, which continues to the end of p. 8-282, is a really excellent discussion. It should come earlier in the chapter, and be carried over into the synthesis and Executive Summary.
34. p. 8-281, lines 19-20: RSP, with a broad cut-size characteristic @ 4 ：m in aerodynamic diameter, is <u>not</u> equivalent to PM_{10}. It should be described as an internationally agreed convention for particles that penetrate to the gas exchange region of the thorax, and being intermediate between $PM_{2.5}$ and PM_{10}.
35. p. 8-283, line 12: change "exposures" to "and $PM_{2.5}$ concentrations".
36. p. 8-283, line 19: insert "short-term" after "observed".

37. p. 8-285, line 28: Finding (9) should be broken into two parts. Up through "conditions" on p. 286, line 2, it focuses on short-term effects, while the balance refers to longer-term effects.
38. p. 8-286, line 12: change "may not yet be" to "are not yet".
39. p. 8-286, line 26: insert "to PM of outdoor origin" after "exposure".
40. p. 8-288, line 18: insert "mild" before "asthma". More severe cases of asthma must be considered to be "serious".
An important new paper, not cited in the 3rd draft is:

Lin M, Chen y, Burnett RT, Villeuve, Krewski D. The influence of ambient coarse particulate matter on asthma hospitalization in children: Case-crossover and time series analyses. Env. Health Perspect. 110(6): 575-581, 2002.

It was accepted for publication 11/13/01; therefore meeting the April 2002 cutoff.

Chapter 9. Integrative Synthesis

General Comments

While recognizing that this chapter will not be formally reviewed by CASAC before revisions are made that account for re-analysis of a number of recent time-series studies, there are some review suggestions made herein that are likely to be relevant to finalization of the CD, and warrant consideration for inclusion in the next draft.

Specific Comments

1. p. 9-2, line 10: continue the sentence with "on the basis of the consistency and weight of the evidence".
2. p. 9-3, line 3: change "coarse-fraction" to "the thoracic particle fraction of the coarse particle mass mode".
3. p. 9-3-, line 22: insert "and catalytic" before "oxidation," and "industrial effluents" after ", precursors".
4. p. 9-3, line 23: insert "earth" before "crustal".
5. p. 9-4, line 3: insert "also" before "depends".
6. p. 9-4, line 9: change "several" to "at least four".
7. p. 9-7, line 18: change "rejected" to "passed through".
8. p. 9-12, line 10: insert "and catalytic" before "reactions".
9. p. 9-16, line 17: change "reduce" to "control".
10. p. 9-17, line 24: it should be noted there that nearly all of the IMPROVE sites are intended to monitor background levels.
11. p. 9-51, line 16: change "1 ：m" to "2 ：m", and insert "smaller" before "particles".
12. p. 9-51, line 18: change "the same" to "this", and delete "and".
13. p. 9-51, line 23 and page 9-52, line 3: change "0.3 and 0.5" to "0.2 and 1.0".
14. p. 9-51, line 28: insert ", number of charges" after "size".
15. p. 9-51, line 29: insert "for an aged aerosol" after "small".
16. p. 9-51, line 30: change "one recent study found it to be" to "it can"
17. p. 9-51, line 31: insert "and A" before "region".
18. p. 9-52, line 1: change "an efficient" to "a".
19. p. 9-52, line 22: insert "in the A region" after "dose".
20. p. 9-55, line 9: "mucociliary" is misspelled.

21. p. 9-55, line 12: change "epithelial cells interstitial, and" to "retention in epithelial and interstitial cells, and gradual".
22. p. 9-55, line 15: change "ET" to "ciliated passages of the nose".
23. p. 9-55, lines 19 and 20: change "upward" to "proximal".
24. p. 9-55, line 24: change "upwards of 40 to 50% of deposited 6-10 ：m" to "some".
25. p. 9-56, line 1: change "Clearance" to "Ingestion".
26. p. 9-56, line 2: insert "migration of particle laden macrophages to the mucociliary escalator takes place over several weeks" before "penetration".
27. p. 9-58, line 29: insert "with diameters < 30 nm" after "particles".
28. p. 9-59, line 20: insert "particles < 30 nm".
29. p. 9-63, line 9: insert "number concentration" after "PM".
30. p. 9-64, line 9: insert "larger conductive" before "airways".
31. p. 9-64, line 15: insert "non-volatile" before "metals".
32. p. 9-64, line 24: insert "that volatilize at combustion temperatures" after "metals".
33. p. 9-76, line 8: delete "all".
34. p. 9-83, line 3: "Ottawa" is misspelled.
35. p. 9-118, Figure 9-27: define HF and IHD in figure caption.
36. p. 9-121, lines 15 and 16: note here that the lags are in days.
37. p. 9-126, line 12: change "this" to "these".

Dr. Joe L. Mauderly

Chapter 5: Human Exposure to Particulate Matter and its Constituents
This comment pertains generally to the issue of the uniformity of fine PM distribution across metropolitan areas, and especially to the third "bullet" on page 5-107:

It is repeatedly asserted that fine PM is distributed sufficiently uniformly across metropolitan areas to mitigate concerns about exposure misclassification when area monitor data are used in epidemiology studies. At the same time, there is ample evidence that proximity to traffic emissions is associated spatially with differences in respiratory health outcomes. This spatial difference has been invoked, for example, as evidence that diesel particles affect the health of children. In other instances, exposures to emissions in microenvironments such as city bus stops are invoked to portray the importance of higher environmental exposures to health risks.

The Agency can't have it both ways. Either PM exposure gradients exist within metropolitan areas or they don't. The existence of the spatial variations can't depend on the argument the Agency is making at the time. In truth, spatial differences in PM exposures within metropolitan areas do exist, and are or are not important depending on the question that is being asked by the study. For example, spatial variations are likely to be more important for short-term outcomes than for long-term outcomes.

The point is that there are indeed sizable localized differences in PM concentrations within metropolitan areas, even though we don't have as many data as we would like to adequately characterize the prevalence and magnitude of the differences. These differences are likely important to certain types of health outcomes. Generalized statements that there are not such

differences, or that the differences are not important, should be qualified in regard to the specific research issue under discussion.

Chapter 6: Dosimetry of Particulate Matter

General Comments

The chapter is in better shape than the last draft, but still needs some work to accomplish its purpose of providing an adequate dosimetric background for subsequent chapters. The only real utility of this chapter in the context of the Criteria Document and subsequent Staff Paper is the degree to which it lays the foundation for interpreting the toxicology and epidemiology information that follows. The chief needs are to place the doses used in the toxicology studies in perspective regarding human exposure, and to place the epidemiological studies in context in regard to the likely doses of PM that are received from environmental exposures. The present chapter serves as a tutorial on particle dosimetry, but falls short of accomplishing these key purposes.

As recommended in the last review, the utility of the chapter in setting the stage for subsequent chapters would be greatly enhanced by the addition of: 1) a figure showing regional deposition across a broad range of PM sizes; and 2) one or more tables listing comparative values for deposition among species. A good example of the former is figure 9.1 on page 155 if NRCP report No. 125. This single figure readily communicates more useful concepts at once than the reader is challenged to gain from synthesizing information from several of the present figures. The latter might, for example, list deposited dose parameters (e.g., PM mass per unit surface, PM number per alveolus, PM number per macrophage, etc.) for rats and humans under a selected range of PM particle sizes, exposure concentrations, and exposure times (e.g., 0.1 and 2.0 ：m, 10 and 100 ：g/m3, and 1 and 24 hrs) using whatever model the authors believe best at this time, or perhaps two common models (one of which must be a multiple-path model). The addition of these two items to the chapter will make it at least doubly useful to the reader as he/she tries to digest the toxicology and epidemiology chapters. Indeed, without this kind of information, the chapter's value is very limited.

The section on models is a *tour de force* of the different models, but leaves the reader with little concept of: a) key advances since the last CD; 2) key conceptual differences among the models; and c) advances in our understanding of how the dosimetry of PM affects its toxicity. Listing the models and giving a paragraph on each is only the first step. There is insufficient synthesis of the information.

The summary does not adequately describe the two most important advances in the field of PM dosimetry: 1) a much better understanding of the differences in total deposition between normal and diseased lungs; and 2) an improved understanding of interspecies differences in total and regional PM dose. Although both of these issues have very important implications for subsequent chapters, neither are explicitly listed in the summary. That is astonishing.

The summary does not call attention to the utter lack of information on dosimetry in our present animal models of presumed susceptible subpopulations. This need was raised in the NRC PM Research Committee's report, but is not pointed out as a gap in this chapter.

Detailed Comments

P 6-1, L 15: Dosimetry pertains to both the sites and the amounts of PM deposition or retention.

P 6-4, L 29: The claim that cell types in the three regions are distinctly different is not explicitly true. There is respiratory epithelium in both the ET and TB regions.

P 6-5, Figure 6-1: The turbinate area of the ET_2 region is covered with respiratory epithelium. This isn't portrayed in the figure.

P 6-6, L 11: It should read "—velocity may cause some—".

P 6-16, L 11, 13, 14 and 18: This technique does not "measure" deposition in a region, it estimates it based on assumptions about the nature of the flow front and thus the distance of penetration of the bolus. It does not take into account non-plug flow or differences in airway generations and lengths to alveoli. Whenever results from the bolus technique are described, they should be called "estimates".

P 6-17, Legends for Figures 6-5 and 6-6: Data are only shown in these figures for PM over 1.0 micron in fig. 6-5 and over 0.1 micron in fig. 6-6. They do not show data for the PM size range indicated in the legends.

P 6-18, Figure 6-7 legend: It should read "—regions estimated by the –".

P 6-8, L 8: "Stokes number" should be defined here, because it is the first use in the chapter.

P 6-20, L 8: Again, "measure" should be "estimate".

P 6-22, L 12-13: Is it conceivable that there are geographical areas in which coarse PM are not present? The statement is misleading by implying that there are.

P 6-24, L 10: I think you mean "total fractional deposition" here. There's a difference between total deposition and total fractional deposition.

P 6-24, L 24: Again, I think you mean "total fractional deposition".

P 6-25, L 4 and 6: Do you really mean total deposition in these lines, or do you mean total fractional deposition.

P 6-26, L 7, 8, 9, 20, 21, and 22, and P 6-27, L 20, 22: Are you certain that you don't mean "deposition fraction" or "fractional deposition" in these places?

P 6-30, L 23-24: The sentence does not make clear the value of the citations. It simply states that something was studied, but makes no conclusions or statements about findings.

P 6-39, L 1-2: This statement doesn't make sense any way you look at it. If by "deposition" you mean total deposition, then it couldn't be similar in rats and humans. You must mean fractional deposition. If so, then it's not the smaller lung of the rat that would yield a higher deposition density, its their higher metabolic rate. Rats breathe more per unit of body size than humans. If the lung surface is scaled to the minute volume, then the deposition density would be similar in the two species. If the lung surface is less per unit of ventilation in rats than in humans, then the lung deposition density would be greater in rats. In order for the sentence to make sense, the author needs to do some re-wording.

P 6-40, table 6-1: In the next to last row, "interstitial" is not a mechanism, it's a location. The table is supposed to be about mechanisms.

P 6-43, L 15: Why is the "retention of ambient particles" here any more related to deposition than the retention described above this sentence? The point isn't clear. The amount particles of any type observed in the lung at any given time is related to both deposition and retention.

P 6-45, L 2: The meaning of this sentence is not clear. What does "retained mass arising from the periphery of the TB" mean? What is the "periphery of the TB"? Isn't the point that differences in path length and number of generations to alveoli means that some material probably reaches alveoli even if the bolus is shallow? That is, the apparent long-term component of TB can arise because in fact it is not only the TB region that is being exposed. If that's the point, then the wording needs to be changed to make that clear.

P 6-48, L 24: "Outcome" should be "outcomes".

P 6-49, L 11: It should be either "found" or "evaluated", but not both.

P 6-49- L 14: Is it true that particles were observed in the blood, or is that just inferred because they were found in systemic organs? Did this study confirm the portion of Ag that was particulate vs. solubilized Ag?

P 6-58, L 18: Reynolds number should be defined, or at least described, the first time it is used in the chapter. Is this the first time?

Chapter 7: Toxicology of Particulate Matter in Humans and Laboratory Animals

General Comments

This chapter is improved from the last draft, but still falls short in some important respects. The value of this chapter lies in understanding the plausibility of the causal association between PM exposures and the health effects associated statistically with PM. The toxicology data are very unlikely to be used directly in setting the standard. Plausibility has at least two important dimensions: 1) plausibility of mechanism; and 2) plausibility of dose. The finding of an effect at an absurd dose does not support plausibility of causation at low environmental doses in a different species. This chapter falls short, as the dosimetry chapter before it, in not portraying the relationship between doses in toxicological and epidemiological studies. To do that depends on using the advances in interspecies dosimetric modeling to estimate the human doses

equivalent to those which were the lowest at which the various toxicological effects have been observed. For some effects, this comparison will be convincing that the toxicology supports the epidemiology very well. For others, it will demonstrate that the toxicology has not yet demonstrated effects at relevant doses.

The information conveyed in this chapter is seriously short-changed by ignoring the large body of information on combustion PM and PM together with co-pollutants that exists from the work on engine emissions. The fact that there is a health assessment (not yet published) on diesel emissions is not an acceptable excuse. You can't adequately discuss our knowledge of PM by only citing a few selected issues (e.g., adjuvancy) from the diesel literature and studiously ignoring the rest. This is particularly egregious considering the Agency's repeated assertion that diesel emissions in general, and diesel PM in particular, is one of its greatest air pollution health concerns! This separation is transparently arbitrary and ultimately counterproductive.

The chapter still suffers from insufficient attention to listing exposures or doses for each study cited. Mechanisms and outcomes are really not very important unless you can put the exposure into context, and unless the exposure is within the realm of credibility. The lowest dose yielding a significant effect should be listed for each major category of effect, or for each major category of PM component.

Detailed Comments

P 7-3, L 3: It should read "—encompasses myriad physical—".

P 7- 3, L 22: It should read "—orders of magnitude fewer—".

P 7-4, table 7-1: It is not clear what the purpose of this table is. Don't you describe the PM used in individual studies in subsequent tables? If this table is important, why is it so incomplete? For example, surely you can get a description of the PM samples obtained from NIST. They know where they got them and what they are. Why use both "DEP" and "DPM" for diesel particles? Are you trying to say that the two terms distinguish the particles somehow? How is Mt. St. Helen's ash a "laboratory-derived surrogate PM"? Overall, this table either needs to be deleted, or it needs to be shaped up.

P 7-6, L 17: Why are you only worried about acute exposure here?

P 7-15, L 24: Do you really mean "specific ambient sources" here, or do you mean specific ambient locations?

P 7- 19, L 21: Be certain that when you are referring to exposures to whole diesel exhaust, you don't make the mistake of calling it an exposure to DPM. There is a difference between exposures to diesel particles and exposures to diesel exhaust. Both are useful, but they are far from the same thing. Distinguish between them throughout the chapter.

P 7-20, L 22-27: First, give a specific reference when you are referring to a specific study (i.e., filtered diesel exhaust). Second, there are other references (animal studies) supporting the

finding that filtered diesel exhaust expressed most of the effects of whole exhaust. This is a very important point to make clear; i.e., that one can't equate the effects of diesel exhaust to the effects of diesel particles (although the particles do have effects).

P 7-22, Section 7.2.1.3: Diesel particles certainly are also "complex combustion-related particles". Why apply that term only to ROFA?

P 7-23, L 13: Give exposure concentration and time.

P 7-23, L 22: The quantities given are "doses", not "concentrations".

P 7-25, L 17: In this summary, it would be useful to indicate the lowest dose of, or exposure to, ROFA that has caused a statistically significant effect.

P 7-26, table 7-5, last citation: Did this citation also include rabbits? The next page indicates that it did.

P 7-27, L 1: There is something missing on the end of this sentence.

P 7-27, L 2-5: This material is repeated.

P 7-27, L 9-12: First, what do you mean by "previously described"? These findings weren't previously described in this section. Second, the Frampton work is not included in the table. Third, the Zelikof rabbit work is not included in the table.

P 7-27, L 26: Do you really mean "up to 15 ：g/m^3" or do you mean "as low as 15 ：g/m^3"?

P 7-28, L 5: The statement that "only limited studies" have been performed seems to conflict with the large number of studies in the table. I'd say there have been lots of studies of PM other than acid aerosols.

P 7-31, L 8: What was the dose?

P 7-33, table 7-7: There is endotoxin on some ambient PM, but it is not clear that endotoxin is an "ambient bioaerosol" per se. This section should note the lack of information on most bioaerosols, seeing that nearly all information pertains to only the presence of endotoxin. That hardly covers the waterfront of bioaerosols.

P 7-37, table 7-8: The particle sizes listed for the Watkinson study are all identical, and are that of the ROFA. Not all of the materials had the same particle size. The sizes of the other materials should also be listed.

P 7-38, L 29: The materials were instilled in 0.3 ml saline, not in 3.0 ml as written.

P 7-38, L 31: The word “serious” has little meaning here. How did the authors know that the arrhythmias were “serious”; indeed, what is the difference between a “serious” and “non-serious” arrhythmia? The relationship of arrhythmias in rats and humans hasn’t been established.

P 7-39, L 2: The listing of deaths is incorrect. There were “1, 3, and 2 deaths” in these groups, not “1, 2, and 3 deaths”.

P 7-44, L 20-21: While it is true that current data have not demonstrated effects below “high concentrations”, it is not true that they have proven that there are no effects at lower doses. The present wording indicates that we have initial evidence that only high concentrations “can” have effects. It may seem a small difference, but it would seem more accurate to state that “-- studies to date have only provided evidence that high concentrations –“. This suggests more accurately that, while studies do date have not shown effects at lower doses, this does not preclude the possibility that there may be effects at lower doses.

P 7-43, L 15: “—if unclear--” should be “—is unclear—“.

P 7-44, L 18: it is not clear how the increased fibrinogen “corresponded” to the inability to find the other things. What does “correspond” mean here? Are you suggesting cause and effect?

P 7-47, L 14-16: How does this make sense? You are comparing two studies using two different kinds of rats exposed to CAPs at two different times and in two different locations. The studies can’t possibly be repeated. What is your point?

P 7-47, L 24-27: What is the lowest exposure (concentration and time) to CAPs that has produced a significant effect?

P 7-48, L 1: Delete “tests”. The PM might have affected pulmonary function, but I doubt that it affected the pulmonary function tests.

P 7-49, L 1-20: If these findings are worth half a page, they are worth noting the doses.

P 7-50, L 10: What are the references for this “considerable evidence”?

P 7- 50, L 14: What was the dose?

P 7-50, L 21-22: The contrast you are trying to make here is not clear. What’s the difference between “sporadic incidence of focal alveolar fibrosis” and “only modest increase in trichrome staining”? If these are indicating the same things, then are you saying that “sporadic” is different from “modest”? Which is worse?

P 7-51, L 18: What does “macrophage dysfunction phenotypes” mean?

P 7-53, L 15: What does "all" mean here? Do you really know that "all particles exert an adjuvant effect"? That may well be true, but do you know that?

P 7-58, L 29-30: What is the basis for the statement that "improvements in diesel engine design have brought about a significant decrease in particle size"? What is your reference for this statement? If it is true that particle size has been reduced (which I question), then what is your information that shows that it was due to engine design (in contrast to fuels, after-treatment, etc.)?

P 7-59, L 19-20: It seems peculiar that you quote 10 mg/m^3 and then state that 20 mg/m^3 is a "very high" concentration. Isn't 10 mg/m^3 ridiculously high as well? Where is your break point for "very high"?

P 7-61, table 7-9: Volatile organics are by definition not particulate; thus, they are not a characteristic of particulate matter. Perhaps you mean "semi-volatile organics".

P 7-63, table 7-10, third listing: Where is "DEF" defined? What is it?

P 7-72, L 21: How is ROFA "surrogate combustion particles"? I thought they were combustion particles. There are many different kinds of combustion particles, and ROFA certainly isn't a "surrogate" for anything except ROFA. What's the point?

P 7-77, L 19: What would you mean by the response increasing "with washed particles? Don't you mean that the response decreased when the particles were washed?

P 7-79, L 17: The wording here is confusing. I doubt that the material was collected on two sides of the same segmental bronchus. I'd guess that you mean it was collected from segmental bronchi on both sides; i.e. from one on the right side and from one on the left side.

P 7-80, L 8-11: Is this the only conclusion that can be drawn from the section? Of course the response is heavily dependent on the composition of PM – that's not news.

P 7-81, L 7: Is "—a unique type of programmed cell injury—" a reasonable definition of apoptosis? I thought it was programmed cell death.

P 7- 81, L 17: Do you mean over and between the epithelial cells? What would "between the lining" mean?

P 7-81, L 18-91: The thoughts aren't clear here. Are epithelial cells actually "targets" of the nerve endings? In what sense are they "targets"? Certainly, the trachea is not the first innervated epithelium to be encountered by PM, as implied here.

P 7-81, L 26-28: Overall, I can't make sense of this statement. The point about distinguishing genetic and "non-genetic" factors isn't clear. Is an argument being made for "genetic factors" somewhere?

P 7-83, L 5: "Ottowa" should be "Ottawa".

P 7-83, L 11: I think "phasphatase" should be "phosphatase".

P 7-83, L 20: Are the other approaches "non-technical"? What is the point of the word "technical" here?

P 7-85, L 31: "Stimulated" should be "stimulate"

P 7-86, L 18: What particles are "used" in "occupational studies"? Do you mean particles occurring in occupational exposures? If so, the statement is arguable; lots of occupational exposures certainly involve particles less than 0.1 ：m in size (for example, all combustion emissions).

P 7-86, L 26: You should list organics as well as metals in regard to surface releases. Organics are arguably more prevalent on surfaces than metals.

P 7-88 and 7-89: Lots of detail given here, but what's the point? This section seems to lack a punch line. Lots of the information is given without mentioning doses or dosing technique. Unless you put it in perspective, the information is not of practical interest.

P 7-88, L 10: But of course, the composition of ultrafine and larger particles is not generally the same.

P 7-88, L 14: Are you talking about doses in the plausible environmental range? If not, then this information isn't very relevant. In line 17, you quote a dose of 125 ：g. This would be about 15 days of continuous exposure of a rat at 100 ：g/m^3. This section doesn't quite get around to putting the information into context regarding ambient exposures.

P 7-89, L 5: How were the explants dosed?

P 7- 90, L 15: What is "ROI"? Is it defined somewhere?

P 7- 91, L 11-23: What are you defining as "lung injury"? In line 19, what were the exposure times you are quoting?

P 7-91, L 28: How long were the exposures?

P 7-92, L 22: What was the dose that caused this effect?

P 7- 93, L 6-20: First, are we really interested in effects observed at an exposure concentration of 15 mg/m^3? Second, what was the exposure or dose in the dog study you quote in line 14?

P 7-93, L 28 and L 30: What were the exposures or doses?

P 7-94, L 9: What were the exposures or doses in the Gordon study?

P 7-95, L 1: What do you mean that it's not clear whether particles enter the blood? What part of distribution to other organs is not clear?

P 7- 95, L 27: This is a very short list of non-PM components of the ambient air. These are the gases that are routinely measured, but it's myopic to imply that PM only co-exists with these. It should be made clear that, unfortunately, we only have information for a very tiny portion of the non-PM components, and that's because others are not routinely measured.

P 7-96, L 29: This statement borders on the absurd. We have a huge database on PM in mixtures other than acid sulfates. What about all the studies of diesel emissions, as only one example? This whole section is sadly short-changed by intentionally omitting all the information on combustion emissions. That's where most of our present information lies.

P 7-100, L 15-16: Care to give us a "so what" here?

P 7-100 and 7-101: Exposure concentrations or doses are lacking in several places.

P 7-102, L 13: It would be useful to list the major reaction products or classes involved.

P 7-104, L 14-15: First, what's "unique" about Mexico City, other than perhaps its higher concentration of pollutants on many days? Does Mexico City air contain contaminants that aren't found elsewhere? Second, are you suggesting that the exposures in cities other than Mexico City are "controlled", and that the nutritional factors are "known"?

P 7- 104, L 21: It's not efficient to define an abbreviation for a term that's only used in one paragraph (BAUS).

P 7-105, L 7: It should read "—concentrations of O_3—".

P 7- 105, L 11-12: If you have no other information on the PM than it was "collected from gas stoves", then the information here is not useful.

P 7- 105, L 20: To be "biologically plausible", the knowledge has to include both mechanisms and dose. The dose component is short-changed in this chapter. Indeed, the value of the chapter could be tripled by simply including a table showing the lowest exposures or doses that have been shown to cause the different effects described in the chapter.

P 7- 107, L 7: Why would we assume that any single PM component is the "primary toxic component"? If we've learned anything over the last 5 years, it's that there is no "primary" toxic component – there are many.

P 7-107, L 20-21: Citing the teflon story is certainly an odd place to begin a summary of ultrafines! This was neither our first information on ultrafines, nor was it ever important regarding ambient PM.

P 107, L 29: It should read "—more inflammation per unit of mass—". That's the whole point.

P 7-108, L 11: Whether or not bioaerosols would "account for the reported health effects of PM" is not the point. The point is whether or not bioaerosols contribute to the health effects of PM. Moreover, "bioaerosols" is not the same as endotoxin. There are lots of bioaerosols. Endotoxin contamination of PM is only one form of the issue. The chapter is remiss in not conveying that idea by explicitly pointing out that we have very little information on bioaerosols except for endotoxin. The chapter is also remiss in not conveying the importance of pollens and other bioaerosol allergens. We actually have quite a bit of knowledge of their importance and prevalence, but somehow this chapter chose to ignore them as if they were not contained in ambient PM exposures.

P 7-110, L 23-25: It should be added that findings since 1996 have certainly provided evidence supporting many hypotheses for PM effects, and that this body of evidence has grown substantially.

Dr. Roger O. McClellan

Overall Comments

The third draft of the Criteria Document is significantly improved over the earlier drafts. However, some specific revisions are required before it is viewed as providing a scientifically sound review of current knowledge of Particulate Matter and, especially, for the establishment of NAAQSs for Particulate Matter. I will enumerate the recommended changes in my comments.

With only a few exceptions, the document reviews all of the appropriate literature. However, the individual chapters still fall short in their synthesis of the available literature and identification of key information required for setting the NAAQS for PM. In addition, this key information is not adequately integrated in Chapter 9 nor in the Executive Summary. The individual chapters, the integrated summary and the executive summary would benefit from having a much stronger orientation to how the information may be used in establishing the four key elements of a NAAQS. Specifically, how will the information reviewed inform decisions on establishing (a) the indicator (PM_{10}, PM_{10-25}, $PM_{2.5}$ or others), (b) the averaging time (24 hr or annual), (c) numerical level in ：g/m^3 and (d) statistical form.

No matter how polished a scholarly review the CD might be if it fails to adequately relate the information to the four key elements above, the document will be inadequate. Some individuals might argue that this infringes on the role of the Staff Position Paper. I do not feel that is the case but rather feel that improved presentation and integration of information in the CD relative to the four key elements will facilitate the preparation of an improved science-based Staff Paper and, ultimately, a science-based NAAQS for PM.

In presenting information that will inform decisions on the four elements of the NAAQS for PM, it is important that the CD take a neutral stance with regard to the final decisions on the elements. The present document does not always convey a neutral tone. This is especially the case in the Executive Summary.

At the time the 1996 CD was prepared, the available information on $PM_{2.5}$ measurements was limited. As a result, the 1996 CD included analyses based on extrapolated $PM_{2.5}$ values. Other analyses used other extrapolated values. It is no longer necessary to use analyses based on imputed values and if cited in the document their shortcomings for decision-making should be noted.

It is now apparent that many of the time-series studies published in the peer-reviewed literature involve flawed analyses. These studies must be identified in the CD and more appropriate conclusions based on revised calculations included. The finding of serious flaws in papers such as those from the National Morbidity and Mortality-Air Pollution study, despite its being subjected to extensive, critical peer review by the Health Effects Institute has, in my opinion, sobering ramifications with regard to the peer review process in general and especially as related to complex computer-based statistical analysis of large data sets.

In my opinion, when the results of such analyses have a central role in decisions on the NAAQS for PM, such analyses should be subjected to rigorous independent evaluation. In many cases, this may involve replicative analysis by an independent group and the publication of much more details of the analytical methods than has been typically done in the past.

Appropriate attention has been directed at the GAMs based analyses and the issues of convergence criteria and overstatement of statistical significance. However, I am concerned that the same kind of critical evaluation has not been directed at the other statistical analyses reported in the CD. The problems discovered with the GAMs based analyses emphasize the importance of the Agency and CASAC conducting a more critical review of the findings cited in the CD. It is especially important to recognize that the CD goes beyond assessing whether PM is hazardous and must include quantitative data that are necessary for the agency to arrive at numerical levels, averaging times, and statistical forms that address the issue of how low is low enough to satisfy the Clean Air Act's requirement for setting a NAAQS with an adequate margin of safety.

An example of the need for more critical review is the analysis used in NMMAPS to arrive at a central estimate of the PM_{10} mortality risk for the 90 (88) cities. The analysis uses a Bayesan approach that is very dependent on the choice of priors as related by Ann Smith in her presentation to CASAC.

A serious deficiency in the present CD is the failure to adequately place the health risks of PM in perspective relative to co-pollutants and other factors influencing health risks. In my opinion, the present CD understates the difficulties in reliably estimating the health risks of individual pollutants. Since the CD at hand addresses PM, there is a tendency to overstate its effects and down play the health effects of other pollutants. One approach to helping gain a balanced perspective is to require that all major epidemiological studies used in the setting of the NAAQS provide concentration-response coefficients for all of the pollutants, not just PM, and when feasible provide calculations that apportion the "excess air pollution risk" among the several pollutants. I personally find it very useful when a study report includes information that allows one to gauge the impact of individual pollutants and other factors such as barometric pressure changes, temperature, educational level, etc. An understanding of the relative importance of the several pollutants can provide valuable inputs for decisions on control strategies.

In summary, the third draft CD is improved over the previous drafts. However, much remains to be done to ensure that all the cited literature represents analyses that were

appropriately performed and that all information in the document is synthesized in a manner that will inform decisions on the key elements of the NAAQS for PM.

Chapter 6: Dosimetry of Particulate Matter

A. General Comments

This chapter is a scholarly review of the subject. All of the relevant literature have been considered.

Unfortunately, the chapter has three major weaknesses:

1. The chapter is excessively long and turgid with details. As an aside, the authors could reduce the length by at least a page by giving a single referral to the 1996 CD in the first paragraph.
2. The chapter would be substantially improved and shortened if more emphasis were given to synthesis of information and presentation of basic concepts.
3. The chapter is poorly linked to the rest of the CD. In part, this may be the case because very little of the information on dosimetry has any bearing on the establishment of the NAAQS for PM. If the authors disagree with my assertion, I challenge them to highlight in the chapter the specific information that impacts on the setting of the NAAQS for PM. This exercise will aid the authors in identifying the contents of a substantially shorter revised chapter.

A key component of the shorter, revised chapter should be a brief discussion (perhaps including a summary table) of certain aspects of the dosimetry of key components of PM. This would include consideration of particle size and mass and chemical components such as the carbonaceous fraction, sulfates, nitrates, and trace metals that have received consideration as putative toxic agents that may have a special role in the toxicity of PM. A table might be created showing for a "typical" subject the estimated amounts of each PM constituent deposited per day for one or more "typical" aerosols. The table(s) should complement similar tables in other chapters, especially the toxicology chapter (both for animal studies and controlled exposure human studies) and the epidemiology chapter. The data on specific constituents will help provide perspective for the quantities used in the human and laboratory animal toxicity studies reported in Chapter 7. And most importantly, this set of tables will be useful in developing a more informative integrative summary chapter.

B. Specific Comments

1. Create a shortened, concise introduction out of the present first 3 paragraphs.
2. Provide a single referral to the 1996 CD as a complementary document. It is not necessary to repeatedly refer the reader to the 1996 CD.
3. Where information is especially relevant to assessing the hazards of PM, make note of the situation. For example, on pgs 6-12, the topic of hygroscopicity is considered. The paragraph provides the reader no clues as to whether this is important relative to PM composition in any part of the country.
4. On all figures and tables (for example, Figs. 6-8), clearly note whether the material presented represents measurements (in people), calculations or whatever.
5. Pgs 6-35: Inhalability is introduced but not defined here or on pgs 6-61.
6. Pgs 6-42, line 19: The basis of the 5% figure is unclear. Is this 5% of all alveolar cells in a given volume of alveolar tissue or 5% of free alveolar cells? It should be supported with a reference or deleted.

7. Pgs 6-48, line 23 and on: The authors are trying to make a linkage from inhaled PM to health effects in extrapulmonary tissues such as the heart. The section needs to be rewritten and some quantitation introduced. It should start with a brief description of the several ways that have been hypothesized for PM to have extrapulmonary effects: (a) direct translocation of PM or constituents, and (b) release of cytokinetics from respiratory tract with translocation to the heart. To the extent possible, the emphasis should be on a quantitative presentation.

8. The 15 pages on modeling needs to be condensed to no more than 5 pages.

9. The summary and conclusions need to be rewritten to provide a linkage to the rest of the CD and the setting of the NAAQS for PM.

Chapter 7: Toxicology of Particulate Matter in Humans and Laboratory Animals

A. General Comments

This is a voluminous compilation of research findings on the health effects of PM observed in studies of human subjects and laboratory animals exposed to PM under controlled exposure conditions, generally to concentrations of PM substantially greater than found in ambient air in the U.S. Although the introductory paragraph introduces the concept of "various research approaches targeted to test hypotheses," the concept of hypothesis testing is not present in the rest of the chapter.

By and large, the chapter meets the author's billing – "it may fail to adequately convey the extensive and intricate linkages among the pulmonary, cardiac, and nervous systems, all of which may be involved individually and in concert to represent the effects of exposure to PM." I urge that the chapter be revised, shortened and organized in a manner that more clearly links the chapter to other chapters in the CD and the establishment of the NAAQS for PM.

It would be useful for the chapter, after a brief introductory paragraph, to include three brief sections that link to the rest of the CD. The first of these sections would relate what is typically found in PM and, thus, forms the basis for hypotheses as to how PM may produce health effects. This should include consideration of all of the major constituents such as carbon, sulfate, nitrates and trace metals linked to the dosimetry chapter. The second section would briefly relate the health effects found to be statistically associated with increased PM as a basis for testing hypotheses related to how PM might produce their effects. And the third section would relate the approaches available for testing hypotheses – i.e., controlled human exposures, laboratory animal studies and in vitro approaches. For each approach, the strengths and weaknesses from biochemical indicators should be described. This section needs to describe the challenge faced by the experimentalist trying to obtain data on hypotheses that must ultimately be linked to statistical associations between increased levels of PM, increased rates of adverse health outcomes characterized on the order of 1% or less increase in effect per 10 : g/m^3 by studying millions of people over periods of years.

A serious deficiency in the present Chapter 7 is the failure to provide a brief summary of what is known concerning the health effects of various kinds of PM in occupationally exposed populations. This might take the form of a few summary references and perhaps a table of the various agents and levels of exposure producing disease. Some might argue that occupational populations are healthy individuals and the exposure levels are much higher than ambient PM concentrations. I would agree, but note that the information is still very useful to provide perspective in considering the health effects of PM.

An additional serious deficiency in the present Chapter 7 is the inadequate coverage of chronic exposure studies previously conducted in laboratory animals with particulate matter. I suggest the authors include a brief section, including summary table, that reviews our existing knowledge from chronic inhalation studies conducted in laboratory animals. One group of studies are the multiple exposure level studies conducted with diesel exhaust. Some of these studies, especially those conducted by the Lovelace organization, evaluated a broad range of responses from biochemical indicators to the life span of populations. In addition, chronic inhalation studies conducted by the National Toxicology Program, although small in number, are worthy of consideration in Chapter 7. A few other studies may be identified by considering data bases such as those of NIOSH, ACGIH and ATSDR.

It is recognized that the studies noted above generally involved "pure" compounds and, thus, do not exactly mimic typical ambient PM. In addition, the studies typically started with young health animals and followed them for 2 years or, in a few cases, longer. Obviously, near the end of the studies the animals were aged. These provide a substantial data base for extrapolation to ambient PM and human populations.

The rest of the chapter needs to be more clearly organized around (a) PM constituents, (b) type of adverse outcome, and (c) approach used (human studies, laboratory animal studies and in vitro.) Whatever the organization, the matrix of information should remain clear.

The chapter requires a revised summary and conclusions section that more clearly links to the rest of the CD and especially the epidemiology chapter and the Integrated Synthesis. This might be achieved by developing several tables that summarize the evidence as related to a, b, and c above. The total chapter, and especially the summary and conclusions, needs to have a "neutral" tone in describing evidence for PM, and specific constituents, at ambient concentrations causing adverse health effects.

B. Specific Comments

1. The present format of the chapter which might be described as a "dump" of data is not conducive to review. Beyond being revised to provide an orderly, organized exposition of information synthesized around concepts (hypotheses), there is a critical need to review all material to determine if it is factually correct. For example, I note (pg 7-19, line 15) reference to metabolites of metal components of the particles is a discussion of diesel particles. This strange statement needs to be supported with a reference as to the metal content of diesel particles and the identified "metabolites."

2. Pg 7-22: Section 7.2.1.3 should be retitled if it included in any form – "Residual Oil Fly Ash" and the introduction to the section should make clear that this material is not representative of most combustion-related particles. The unique nature of this test material is such that the information should be summarized in less than one page.

3. Pg 7-25. Acid Aerosols. In view of the extent to which control strategies for PM focus on SO_2, sulfate and acidity, it is important that this section be revised and separate sub-sections developed on each of these categories of constituents. This should include a critical review of the 1989 Acid Aerosol Issue Paper and the extraction of key concepts and summary information. The resulting discussion in the CD should stand alone without reference to the issue paper.

In the presentations on specific constituents (that are present in substantial concentrations in typical ambient PM) care should be taken to clearly delineate both what has been studied and not studied. In short, it is important to clearly delineate what is unknown

because it has not been studied versus positive results based on experimentation versus negative results based on experimentation (with the experimental conditions carefully described).

4. Pg 7-28: The brash statement "that particle-associated metals are among the potential causal components of PM" needs qualification. If the statement remains, the EPA owes it to the readers to provide a side-by-side comparison of the metal content of ambient PM, estimated levels deposited in the respiratory tract of humans and the levels used in the experimental studies.

. The biases of the authors at trying to find and ascribe effects irrespective of relevance of the evidence is apparent in the last paragraph on pg 7-31. The study of Lay et al (2001) is described with results that fail to support a hypothesis for a role of Fe even at concentrations more than 1000 times that of ambient air is followed by a case report concerning oil fly ash and health effects that I would argue that is of dubious value relative to ambient PM.

Chapter 8: Epidemiology

General Comments

This chapter provides the information that is most critical to the establishment of NAAQS's for PM. The quantitative information in the chapter plays a central role in the risk analyses conducted to inform the numerical values in the Staff Paper and ultimately in the establishment of NAAQS's for PM.

To state the obvious, we must have confidence in the quantitative results from the epidemiological studies. The discovery of problems with the time-series studies is a sobering reminder that even rigorous peer review can miss problems. It is especially important that all of the key studies be reviewed again to make certain the analytical methods are as sound as they can be. In particular, attention should be directed to the review of any options in the programs that can be altered at the discretion of the investigators.

For selected studies it may be desirable to have re-analyses conducted by independent teams such as the Krewski et al re-analysis of the ACS and Harvard Six Cities data. Re-analyses are valuable in having a "second set of eyes" review data and conduct analyses affirming the results for use in making important societal decisions.

Chapter 9: Integrative Summary

This is the most important chapter in the document in that it provides a linkage to the Staff Position Paper and, ultimately, promulgation of the next NAAQS for PM. As such, the chapter needs to draw on information presented in other chapters of the CD and synthesize the information that has bearing on the setting of a NAAQS for PM. This places the highest priority on information that will inform decisions on the four elements of the NAAQS: (a) indicators, (b) numerical level, (c) averaging time, and (d) statistical form.

The present chapter is not adequate. A strength of the present chapter is its use of the source to response paradigm advocated by the National Research Council's Committee on Research priorities for Airborne Particulate Matter. However, the chapter does adequately synthesize information to inform decisions on the four elements of the NAAQS noted above.

In creating a revised chapter, greater emphasis should be placed on linkages to other chapters in the CD. Elsewhere in my comments I suggested that in the toxicology and epidemiology chapters, summary information be developed, preferably in tabular form, that can be brought forward into the synthesis chapter. In some cases, this will mean synthesizing substantial information and drawing some overall conclusions of both a qualitative and

quantitative nature. In other cases, limited or no information will be available and this should be documented.

In my view, a serious deficiency in the total document, and especially in Chapter 9, is its failure to appropriately acknowledge that many aspects of the health impacts of air pollution are of a multi-pollutant nature. This issue deserves special emphasis in consideration of the epidemiological studies. For all of the epidemiological studies cited in Chapter 9, care should be taken to identify the extent to which the study considered other pollutants. In addition, for key studies the concentration-response coefficients for the other pollutants should be given. The magnitude of the influence of other confounders should also be provided. For example, I found the statement by the NMMAPS investigator that the air pollution signal was an order of magnitude less than the confounder effect. Statements of this kind are useful to help place the effects of air pollution in general and specifically, particulate matter, in perspective.

The Particulate Matter metrics used over the years have changed as emphasis has shifted from indirect measures to TSP to PM_{10} to $PM_{2.5}$ and, to a lesser extent, $PM_{10-2.5}$. The amount of information on the latter metrics is still very limited. As such, it is no longer necessary or appropriate to use findings from epidemiological studies based on estimated or imputed $PM_{2.5}$ values. Because of major uncertainties associated with $PM_{10-2.5}$ values developed based on differences between measured PM_{10} and $PM_{2.5}$, it is not appropriate to give very much weight to epidemiological studies based on $PM_{10-2.5}$ (PM coarse values) arrived at by difference.

Based on the recent finding of major statistical issues in the analysis of the time-series studies, it will be important to include in Chapter 9, the results of studies using updated statistical methodology. Any studies that used GAMs methodology and not subjected to re-evaluation should be clearly identified as not being suitable for use in the Staff Paper.

Dr. George T. Wolff

Chapter 3

p. 3-4, line 21 – 22 – The factor of 1.4 also includes hydrogen.

p 3-6, Figures 3-1a & b – Why were these concentration cut points chosen? One cut point should be the NAAQS so that one can readily judge the degree of the problem in terms of attainment/nonattainment. Therefore, the top map should have a cut point at ³ 50 and the lower one a cut point at ³ 150.

p. 3-9, Figures 3-4a &b – Here cut points of ³ 15 in the top map and ³ 65 in the lower map should be used. Also, "for counties with $PM_{2.5}$ monitors" should be added to the figure captions.

p 3-12 – Background discussion – Using the lowest 5% of the concentrations likely underestimates the background because it will include days with below background concentrations due to precipitation scavenging. It would be more meaningful to estimate the distribution of background concentrations which will be dominated by meteorology and an occasional event like a wildfire or duststorm.

p 3-16 – The concentration units and the year of collection are not identified in figure 3-7a -d.

Figure 3-8 – Year is missing.

Figure 3-9a-c – See comment 5.

Figures 3-9 (and others) use median values, figure 3-10 (and others) report geometric mean, and the annual standard is an arithmetic mean. Either the same measure should be used or all three of them should be reported.

Figures 3-13, 14 & 16 are missing axis labels.

P 3-27 lines 14-23. Are these urban sites? Why are California sites unheated?

Table 3-3, 1st column – "It" should be Ti.

p. 3-35, line 20 – "It" should be Ti.

p 3-37, line 27 – COD should be defined here and its significance explained here.

p 3-57, Table 3-8 – Construction and demolition are missing as anthropogenic sources of mineral. Also, pollen, insect and vegetation parts are sources of coarse natural organic carbon.

p. 3-59, line 13 – I suggest adding (which is a liquid particle) after H_2SO_4.

p. 3-59, line 14 – Other heavy metals as well as soot have been observed to catalyze the reaction in addition to Fe and Mn.

p. 3-59, line 16 – I suggest adding "gaseous" before HNO_3.

p 3-59, line 21- I suggest adding "alkaline" between coarse and particles.

p. 3-72, figure 3-24 – This is not 24-hour data as the caption says.

p. 3-75, lines 8-17 – If the Norris et al time-series analysis relied on the GAM and/or S Plus, it probably should be deleted until the S Plus issue is resolved.

p. 3-88, lines 5-6 – What is the source of the transported sulfate?

Summary and Conclusions – A summary of the background concentration discussion should be included.

24. p 3B-1 – Something should be said about the types of samplers used.

Chapter 4
This chapter is a collection of unrelated topics whose only common thread is that they are welfare effect issues. My comments are limited to those areas where I have some expertise – namely the chemical and physical aspects.

p 4-15 – Ambient Concentration section – It makes more sense to reference the wealth of nationwide data from chapter 3 than to cite a few obscure, isolated sampling studies.

p 4-15, line 21 – sulfite??? – There is no important sulfite species. The sentence would be accurate if it said: A significant fraction of sulfur dioxide emissions are oxidized to sulfates.

p 4-15, line 28 – I don't understand what is meant by the phrase "surface reactive material (NO,SO_2) of secondary particles."

p. 4-16, lines 20-23 – There is something missing from this discussion. In a marine environment, coarse nitrate is readily formed by the reaction of nitric acid with sea salt to form sodium nitrate. Sulfate is a component of sea salt which will be aerosolized by the surf.

p 4-45, line 5 – I do not know what is meant by the sentence; "Fine particles have a greater distribution."

p 4-47 – Physical Effects – Radiation – What is missing from this discussion is information on the PM concentration needed to cause these changes. Without it, this information is not very informative.

p 4-55, lines 2-3 – Sulfur dioxide is oxidized to sulfate not sulfite and bisulfite.

p 4-61, lines 25-27 – As written, this sentence is incorrect. It should read: "Volatile organic compounds in the atmosphere are partitioned between the gas and particle phases……"

p 4-61, line 31 to 4-62, line 3 – Organic carbon particle are only emitted from the tail pipe and through tire ware.

p 4-63, line 13 – Delete "transferred."

p 4-74, line 17 – It is not just the secondary compounds. It is primary organic compounds as well.

p 4-78, line 13 to 4-81, line 13 – Atmospheric Turbidity section – The utility of this section is questionable. It contains dated references, inaccuracies, and it does not convince the reader that this is a concern for vegetation.

p 4-81 – Altered Radiative Flux section – I also question the utility of this section without quantitative information which shows this is a concern at ambient PM concentration.

p 4-83 – In my opinion, the section on UV-B is irrelevant to the issue at hand and should be deleted.

p 4-148, Figure 4-25 – This figure would be much more useful and understandable it the size parameter was converted to particle size (diameter).

p 4-179, line 4 – The unstated, but assumed notion that reducing present concentrations of fine PM to levels that meet a NAAQS will measurably increase UV-B and all the effects subsequently discussed is not supported by any data or logic. Thus, statement (1) and all subsequent discussion including the entire section 4.5.1 should be deleted.

p 4-179, line 8 – Change "reflect" to "scatter."

p 4-179, line 11 – Delete "and leads to ……..environment." In its place add: "The scattering of solar radiation by particles has the opposite affect – there is a net decrease in temperature. Enhanced cloud formation due to particle also leads to a net cooling effect."

p 4-179, line 26 – Delete "and (b) metals…….processes." Metals from these sources are a trace constituent.

Section 4.5.2 – This is a very controversial issue and, despite the claims of some, there is no scientific consensus. As a result, it needs to be presented in an objective, non-controversial way. The easiest way to do this is to just discuss the potential and observed impact of particles on weather and climate, and avoid controversial subjects like whether it is presently the warmest it has been in the last 1000 or 2000 years, the accuracies (or inaccuracies) of climate models, the magnitude and nature of the water vapor feedback, whether the scenarios used in the US National Assessment bear any resemblance to reality, etc. Therefore, I strongly recommend deleting section 4.5.2.1. If you do not, it needs abundant line-by-line corrections.

Section 4.5.2.2 – This section should be retained but not in its present form as it is outdated. There needs to be 3 sections: 1) the role of scattering particles, 2) the role of absorbing particles (principally black carbon), and 3) the role of particles in enhancing cloud formation. The IPCC TAR provides a reasonable "state-of-the-science" discussion on 1) and 3), but most of what we have learned about the potential important role of black carbon in climate change has appeared in the literature since the TAR was drafted. In fact, even Figure 4-32, which was taken from the TAR, is outdated. Consequently, this section requires major revisions.

Chapter 8

My comments on this chapter are limited to two issues: the heterogeneity of the short-term study results and the interpretation of the long-term studies.

The heterogeneity issue is not just limited to NMMAPS. It also applies to all of the other time series studies when viewed collectively. Although some produce similar results, others produce different results. They use different models, different lags, and identify different pollutants for

different health end points. Therefore any discussion of heterogeneity should be focused on all the studies. To me, heterogeneity implies the results are inconsistent from study to study.

Long Term Studies. Page 8-95, lines 2 to 9 – To me, the lack of consistent findings in the AHSMOG study and the negative effects in the VA study do cast doubt on the results of the other two studies given the fact that there are some results from the reanalysis study that just do not make sense. These results are summarized below.

A number of concerns were raised with the data interpretation. In the Reanalysis of both the ACS and H6CS, the coefficients for all mortality categories are statistically significant for those with less than a high school education. For those who went to high school, many of the categories become non-significant, and for those with more than a high school education, almost all become insignificant. This clearly suggests that we are missing some socioeconomic or demographic variable(s), which might explain the mortality differences.

Both the ACS and H6CS reanalyses show a negative relationship between PM and respiratory disease mortality. If any disease category was expected to be associated with air pollution, it would be respiratory disease, but it is not. This is not logical.

SO_2 is implicated as being as important as PM in determining mortality. Given the low ambient concentrations of SO_2 and the even lower indoor concentrations, this seems like an implausible finding.

Disease latency was not considered in the Reanalysis. The air quality data used were coincident with the periods of follow-up thus neither study allowed for latency or disease induction. If previous exposures were considered, the regression coefficients would be much less because the PM levels have been decreasing.

Prior disease is protective. When risks in ACS were estimated by prior disease status, they found that those with no disease tended to die 4 years earlier then those with prior disease. This is contrary to logic.

Previous exposure to pollution does not matter - Including residence duration as a predictor variable did not change the PM association in the 6-city study. This implies that previous exposures had no effect which is contrary to logic.

Many potentially important ecological variables were ignored even though members of the HEI Advisory Board kept reminding the investigators that such variables should be included. These variables include but are not limited to: population density, diet, exercise habits etc.

Finally, it should be pointed out that any analysis involving ozone is uninterpretable because both used the wrong measure for ozone. The Reanalysis Team used the annual average O_3 for the H6CS reanalysis. Although they claim to have used the annual mean of the daily 1-hour maxima for the ACS reanalysis, they apparently used the annual average as well because the ozone values presented in appendix G (of the HEI report) are much too low to be the average of the 1-hour maxima.

Cleaner areas and rural areas generally have higher annual average ozone values than more polluted urban areas. Consequently, we are concerned that the results involving O_3 are not meaningful. Another problem with computing an annual mean for ozone is that many locations only measure ozone during the ozone season, which has a different definition depending upon the local climatology. In Michigan, which is typical of northern states, ozone is only measured from April 1 to September 31, whereas in Southern California, it is measured year round. Comparing 5 months of Michigan data to 12 months of Southern California data is like comparing apples and oranges.

Mr. Ronald H. White

Chapter 8

General Comments

This chapter is a comprehensive review of the epidemiological evidence in the scientific literature since 1996, and is a substantial improvement over the previous draft. The inclusion of study selection criteria and the use of summary tables to compile key information on the mortality and morbidity studies are important additions to this draft of Chapter 8. However, EPA should consider whether sufficient need exists for two sets of tables to summarize the same studies, one shorter set on outcomes in the chapter main body and the more detailed tables (that also include outcome information) in the Appendices. Combing the outcomes information in the shorter tables into the more detailed tables in the Appendices would substantially reduce the length and improve the readability of the unwieldy main body of Chapter 8.

Some discussion of less than 24-hour averaging time exposures to PM (as discussed in Chapter 5) and their relationship to potential observed health effects should be added to this chapter.

Specific Comments

Page 8-85, lines 9-13: While this is probably a useful recommendation for future analysis of the ACS data set, I question whether study-specific future research recommendations are appropriate for inclusion in the Criteria Document.

Page 8-85, lines 17-22: It is unclear what the point is for noting the findings regarding the relative risk of $PM_{2.5}$ in comparison to cigarette smoking and obesity.

Page 8-95, line 4: “late” should be “later” studies.

Page 8-97: Table 8-14 should be updated with the data from Pope et al. 2002.

Section 8.2.3.4: The studies of infant mortality and low birth weight discussed in this section should be summarized in a table format in the text and/or in the Appendices. The relationship of low birth weight to infant mortality should receive explicit discussion.

Page 8-107: Include Krewski et al. 2000 Figure 6 in text since it is specifically referred to in the discussion of “Salient Points.”

Page 8-117: Discussion of Moolgavkar (200b) omits discussion of potential correlation between PM_{10} and CO and NO_2 as an issue related to study findings.

Page 8-132, line 25: Provide study citation.

Page 8-171: Except for Peters (1999), studies are generally consistent in finding a PM_{10} effect on respiratory endpoints.

Page 8-172: No support for the statement that Dockery (1996) and Raizenne (1996) are the "most credible" cross-sectional studies. Provide justification for the statement.

Page 8-200, line 5: rewrite as "Samet et al. (2000b)".

Page 8-205, line 7: Provide citation for the Port Alberni study (Vedal et al. 1998).

Page 8-207, Section 8.4.2.5.1: Isn't this section a review of Sarnat et al. (2000) not the 2001 study?

Page 8-209, lines 5 and 14: Eliminate the "we note" and "we believe".

Page 8-211, line 3: Identify that it is the Sarnat et al. (2001) study that is referenced..

Page 8-220, lines 22-25: Acknowledge limited evidence of possible coarse fraction mortality and morbidity effects, as noted on page 8-225, lines 23-25 and 8-233 lines 3-18 (noting Mar et al. 2000; Lippmann et al. 2000; Burnett et al. 1997). Also see page 8-235, lines 4-7 noting the "relative role for coarse PM mass fraction in respiratory hospital admissions.

Chapter 9

General Comments

The organization of this chapter to follow the risk assessment paradigm and the key questions identified by the NRC PM Research Committee, while a significant improvement from the previous draft, still fails to provide a true integration of the key new information contained in the CD and its relationship to the PM NAAQS. The discussion under each of the key NRC Committee questions still generally reads like a sequential reiteration of the chapter summaries rather than an integrated discussion. An alternative approach to this chapter is to extract the key salient points from the findings in improved chapter summaries and organizing them in an interdisciplinary manner to address the major new findings of the post-1996 information.

Specific Comments

Page 9-74, line 30: "disease" is missing after "cardiovascular".

Page 9-92,line 31: Summary of Previous Findings notes "limited evidence for harvesting". However, there is no subsequent discussion of the more recent data discussed in Chapter 8 demonstrating little evidence of a harvesting effect later in the next section on Update Findings.

Page 9-135, line 25: Provide citations for " A few studies".

Dr. Warren H. White

The EPA as a whole, and NCEA in particular, is underfunded and understaffed for the myriad responsibilities it is charged with. It has nonetheless produced, in this third draft, a generally comprehensive and well-written document containing much valuable new material, both literature reviews and original analyses. My comments are offered with genuine respect for the effort and care that have gone into the draft CD.

My primary concern is that the sheer bulk – 1700 pages – of the draft CD seriously undercuts its value to the Agency. When no one reader can hope to digest the whole thing, who can confidently pick out the critical issues on which the Agency should focus its limited resources? Who can be sure that data analyses by one author have accounted for significant measurement problems noted by another, or that malleable terms like "elemental carbon" or "combustion nuclei" mean the same things in different chapters? How can overarching feedback (such as this) be addressed, when it arrives without the page/line numbers that allow it to be assigned to a specific workstation on the document assembly line?

Distillation is hard work and takes time, but using time pressure as a reason to skip it is ultimately self-defeating. The longer the CD gets, the longer it takes to finish; the longer it takes to finish, the more new stuff there is to include. Remember when the Charleston PM2000 Conference was going to be absolutely the last chance to get new work into the CD, via "expedited peer review"? It may seem unreasonable to request major changes when we are already behind schedule, but note that I have <u>always</u> urged setting a hard and fast cutoff date for considering publications, after which all effort could focus on assimilating what was already in a given review cycle's digestive tract.

Particle size and "mode" terminology

Chapters 1 and 2 have somewhat obscure discussions of terminological distinctions concerning particle size. (The re-cap in Chapter 9 is clearer, but retains some of the confusion.) The clarity of these distinctions is impaired by framing them exclusively in terms of particle size. The Agency's present focus on size as the essential regulatory distinction would more easily be justified in the context of the associated chemistry and formation mechanisms.

Consider the concept of fine and/or sub-2.5 μm particles. We can identify at least three different particle populations:

i) Particles with aerodynamic diameters (AD) less than 2.5 μm.
ii) Particles penetrating the FRM impactor. Because penetration curves are not perfectly sharp, this will include some particles with ADs greater than 2.5 μm and exclude some particles with ADs less than 2.5 μm.
iii) Fine-mode particles. This may include some secondary organic, sulfate, or nitrate droplets greater than 2.5 μm AD that the FRM impactor rejects, and exclude some clay particles less than 2.5 μm AD that the FRM impactor accepts.

The ideal size-cut (population i), which one might call $tPM_{2.5}$ (for "true" $PM_{2.5}$), is never clearly mentioned in the CD, but I think it is probably what is really meant by all the references to "particles with an upper 50% cut-point of 2.5 μm. We can say that a suspended particle is less than x AD, but we cannot say for sure how it will be classified by an aerodynamic device. "Upper 50% cut-point" applies only statistically, to an ensemble of particles. The consistent usage of this term seems intended to emphasize the non-ideality of the actual FRM size-cut, which one might call $FRM_{2.5}$. Why not describe $FRM_{2.5}$ (population ii) as an *indicator* of $tPM_{2.5}$ (population i)?

More problematically, is there any operational means to assign a given 1-2 μm particle to the fine (population iii) or coarse mode without referring to its composition and/or morphology? How does one determine, without information beyond the size distribution, even what *fraction* of the coarse mode is sub-2.5 μm or what *fraction* of the fine mode is super-2.5 μm? I think the modes are better first defined as modes of PM generation:

a) Homogeneous formation of nuclei in combustion processes. This yields particles predominantly below 0.1 μm and enriched in EC and certain trace metals.
b) Growth of existing sub-μm particles by reaction with and condensation from the gas phase. This yields 0.1-2.5 μm particles dominated by secondary sulfates, organics, and nitrates.
c) Mechanical breakdown of mineral and organic debris. This yields particles predominantly greater than 2.5 μm that are dominated by minerals and primary organics.

Having defined the modes in these more holistic terms that distinguish particles composition and formation mechanism as well as size, we can then describe $tPM_{2.5}$ (and by extension $FRM_{2.5}$, as an indicator for $tPM_{2.5}$) as an indicator for the nuclei (a) and accumulation (b) modes, and $tPM_{10-2.5}$ (and by extension $FRM_{10-2.5}$) as an indicator for the coarse (c) mode.

Note also that it is a bit confusing to speak of both "the" fine mode, on the one hand, and the nuclei and accumulation modes, on the other. Can a mode have two humps? Should we speak of PM_{10} as the "total mode" aerosol? I suggest just defining "fine" particles as a convenient collective term for the nuclei and accumulation modes.

The above approach would help to avoid recurring problems such as the following sampling.

page/line

1-3/6 "airborne particles with an upper 50% cut-point of 10 μm aerodynamic diameter (PM_{10})"
1-5/2 "$PM_{2.5}$ refers to particles with an upper 50% cutpoint of 2.5 μm aerodynamic diameter."
1-5/4 "the coarse fraction ($PM_{10-2.5}$) refers to the inhalable particles that remain if fine ($PM_{2.5}$) particles are removed from a sample of PM_{10} particles."

$PM_{2.5}$ does not refer to (specific, individual) "particles" but to the aggregate sample of particulate matter that is collected behind a size-selective inlet with a 50% cut-point. The 50% cutpoint is a property of the sampling train, not of the particles themselves.

2-5/30 "The distribution of particles that are mostly smaller than the minimum is termed 'fine.'" "Distribution" is not the right word here – substitute "population of chemically similar particles".

2-6 Although it is common jargon, I note for the record that "mode" is not the right word here. It clearly does not mean the usual statistic, which is the specific size at whatever distribution we are talking about takes its maximum value. Does "mode" refer to a feature in a size distribution or to certain kinds of particles? e.g., what becomes of the coarse mode in a number distribution?

2-9/3 "Aerosol scientists use four different approaches or conventions in the classification of particles by size". Where in this classification does "particles in a specified range of equivalent diameters" fit? How does the "cut-point" approach differ from the "regulatory sizes" approach?

2-10/4 "Definitions of terms used to describe size distributions in modal terms are given below." Unambiguous definitions would need also to specify which equivalent diameter (e.g. aerodynamic or electrical mobility) the distribution and the 1-3 μm window refer to

2-10 "*Nuclei Mode:* That portion of the fine particle mode with diameters below about 0.1 μm." Are the nuclei and accumulation thingies "modes" or "cuts"?.

2-11/1 "$PM_{2.5}$ and fine-mode particles are not equivalent." Can individual particles be classed as fine-mode or coarse-mode? If so, by what standard? An idealized picture such as Figure 2-5 is not good enough. Similarly, we noted above that suspended particles are not themselves PM2.5 or not-PM2.5.

2-14/29 "PM_x, as defined by EPA, refers to a sampler with a penetration curve that collects 50% of x μm particles and excludes 50% of x μm particles." So just divide sample air into two equal streams and you have the universal PM_x sampler, for all x! Figure 2-6 indicates that a $PM_{2.5}$ sampler rejects 94% of 3 μm particles, 50% of 2.5 μm particles, and 16% of 2 μm particles; it is defined by its whole penetration curve, not just the 50% value.

Accounting for field blanks and backup filters

Recent events in the world of business and finance have highlighted how arcane details of accounting need to be understood even by those concerned only with the bottom line. Field blanks and backup filters get about as much attention in PM documentation as executive stock options get in financial statements. This CD mentions the artifact problems associated with sampling carbon aerosols on quartz filters, but keeps all indications of magnitude "off the books" when presenting results (e.g. in Appendix 3B).

If carbon concentrations are to be cited anywhere in the CD, then two pieces of information need prominent coverage:

a) The treatment (or neglect) during data reduction of associated field blanks and backup filters should be specified. In the current draft this is offered only in Appendix 3C, and there only for some of the studies. It merits emphasis, since many at the Agency are unaware of the fact, that the EPA STN network and the EPA IMPROVE network report data differently.

b) The magnitudes (in equivalent concentrations) of field blank and backup filter values for a given network should be summarized. People can reasonably differ on how to interpret these values, but they need to know that they are typically quite different from zero.

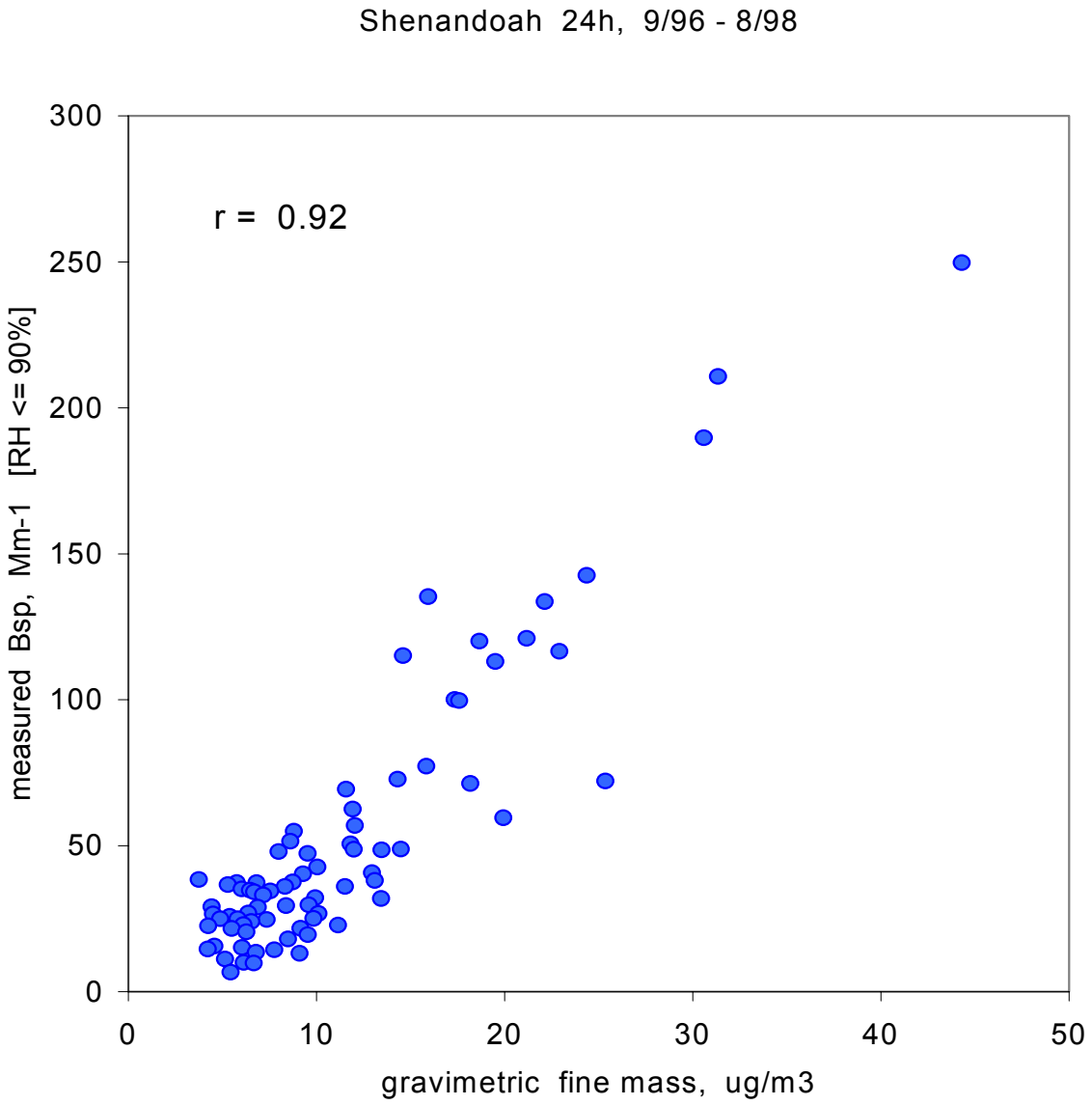
Visibility

The treatment of visibility is completely reworked from the second draft, and the new version is much better in all respects. However, the third draft still avoids any substantive discussion of the close relationship between light extinction and fine particle mass concentrations. This is a critical shortcoming.

One would never suspect from the CD that a secondary NAAQS for $PM_{2.5}$ could conceivably be based on visibility, much less that such a standard (albeit at a weak level) was actually advocated in an *industry*-sponsored review some 20+ years ago (Lodge et al., 1981). Given the recent confusion injected into the health-effects discussion, the Agency should be delighted to highlight this direct, causal, and well-established link between $PM_{2.5}$ and something bad. Instead, it paints visibility as something intangible and subjective, and extinction as an irreducibly complex function of particle size and chemical composition. I have elsewhere criticized this perspective in the context of the Regional Haze Rule and Guidance, as reproduced at the end of these comments.

The dependence of extinction on particle mass is dismissed in a single paragraph at the bottom of 4-153, which describes it sight unseen as "definite but noisy" before moving quickly to "improve" it by incorporating data on humidity and chemical composition. "Noisy" seems a curious term to use for a correlation that typically exceeds 0.9: for what other effect is $PM_{2.5}$ anywhere near as good an indicator?

The figure below shows nephelometer and $PM_{2.5}$ data from the IMPROVE network (vista.cira.colostate.edu/improve) for Shenandoah, a location where the nonlinear humidity effects of concern are strong. Such scatter-plots are seldom published, because they are "old news". Even the original White and Roberts (1977) paper, cited by the CD to support the importance of chemistry and humidity, neglected to mention that the simple correlation between b_{sp} and PM_{15} (total rather than fine mass) was a healthy 0.91 for their data (White and Roberts, 1980).



Specific comments

page/line

4-78/13+ The section on turbidity is weak and could use editing by the author(s) of the visibility section. A couple of examples: (4-78/22) "The residence time of

suspended particles varies with size and environmental conditions (seconds to months or years)"; (4-79/4) "Sulfates, nitrates, and elemental carbon .. exhibit particularly large absorption coefficients". Much of the material and many of the antique references are taken on faith from a single secondary reference (Pueschel, 1993).

4-142/23 "Visibility ..., unlike the particulate matter concentration, is not a property of an element of volume in the atmosphere. Visibility can be quantified only for a sight path ..." This is a false dichotomy. Think instead of "haziness" or "transparency".

4-143/7+ Section 4.3.2 as a whole is weak and largely unneeded.

4-144/14 "when the path radiance is dominant, visibility is poor." Path radiance is always dominant for any target viewed at an appreciable fraction of the visual range.

4-145/5 "Rayleigh scattering ... varies from about 9 Mm^{-1} to 11 Mm^{-1} for most locations of interest ..." This statement is meaningless until a wavelength is specified. After a wavelength is specified, the statement will eliminate either all coastal locations or all Colorado Plateau locations.

4-146/5 "..elemental carbon .. usually results from incomplete combustion of fossil fuels." This claim is somewhat misleading – coal and natural gas are fossil fuels that generate little EC as usually burned, while biomass burning can generate a great deal.

4-146/9 "Most particle absorption data are determined by measuring light transmission.." should be "Absorption is usually determined by measuring light transmission or reflection.."

4-153/6 "An inverse relationship between visual range and the light extinction coefficient .. was developed using a number of restrictive assumptions.." All it requires is that you are in horizontally uniform haze and can ignore the curvature of the earth. What's so "restrictive" about that?

4-153/11+ The coefficient in equation 4-7 is the logarithm of an order-of-magnitude estimate for the contrast threshold ($2x10^{-2}$). Why give it to only 4 significant figures and not, say, 15 ($-\boldsymbol{ln}(2\%) = 3.91202300542815$)? And why is a discussion of units needed, when visual range and $1/b_{ext}$ have the same dimension (length)?

4-159/18 "The Agency expects that the results of these analyses [ASOS] will be available for inclusion in the final PM AQCD." Please try to make this happen.

4-160/6 "..samples are collected at least every third day utilizing filter-based aerosol technology." Unnecessarily pompous: why not "..samples are collected by filters at least every third day."

4-160/19 "Analyses of these data [FRM] are expected to provide a more complete understanding of visibility conditions.." How can they if this CD lays no basis for connecting visibility to $PM_{2.5}$ mass?

4-161/12 "Trends in visibility impairment or haziness often are used as indicators of trends in fine particles." Then why not acknowledge this link in section 4.3.3, Relationships Between Particles and Visibility?

4-162/6 "The increasing sulfate concentrations were later shown to correlate with an increased trend in hazy days at those two locations (Iyer et al., 2000)." This claim is totally bogus – Iyer et al. (2000) considered neither haze, nor its correlation with sulfate. And the reported trend itself was an artifact of changed measurement methods (White, 1997), and contradicts all other trend indications, including those noted in the CD. And in the final analysis, who even cares? Is this 1982-92 anomaly, reported in 1993, one of the most important new developments since the last Criteria Document?

4-162/8+ All of the "visibility" "haziness" and "light extinction" trends discussed on the rest of this page are estimated from chemical concentrations rather than directly observed, and this caveat should be clearly noted.

The following two summary statements are orphans of earlier drafts – the current draft does not support any claim that 40% of southern California extinction is due to nitrate aerosol.

E-34/30 "Haziness in southern California is primarily caused by nitrate and organic PM. Nitrates contribute about 40% to the total light extinction in southern California. Nitrates account for 10 to 20% of the total extinction in other areas of the United States."

4-209/11 "Light scattering by nitrate aerosols is the major cause of visibility impairment in southern California. Nitrates contribute about 40% to the total light extinction in southern California and accounts [sic] for 10 to 20% of the total extinction in other U.S. areas."

References

J.P. Lodge Jr., A.P. Waggoner, D.T. Klodt, and C.N. Crain (1981) Non-health effects of airborne particulate matter. *Atmospheric Environment 15*, 431-482.

W.H. White and P.T. Roberts (1980) On the nature and origins of visibility-reducing aerosols in the Los Angeles air basin. In The Character and Origins of Smog Aerosols, G.M. Hidy et al., editors, John Wiley & Sons, New York.

W.H. White (1997) Deteriorating air or improving measurements? – on interpreting concatenate time series, *Journal of Geophysical Research 102*, 6813-6821.

APPENDIX

Invited Comments on Visibility: Science and Regulation, the June 2002 A&WMA Critical Review by J.G. Watson for publication in September 2002 JA&WMA

The review provides an comprehensive survey of the scientific and technical underpinnings of the EPA's new Regional Haze Rule. It assesses our scientific and measurement capabilities in the context of a given set of regulations and guidance. As a *critical* review, I wish it had also assessed the new regulatory approach in the context of our existing science and capabilities. The goal of protecting visibility comes to our technical community as a political mandate that is not ours to question, but the technical details of its regulatory implementation are a legitimate subject for us to consider.

The EPA has chosen to protect visibility by redefining it in terms of particle species' concentrations averaged over long periods. The review accordingly ignores other seemingly plausible frameworks, including pre-existing regulations at the state level. Ever since 1959, for example, California has considered ambient air quality to be "adverse" if particle concentrations are sufficient to produce an extinction coefficient of 230 Mm^{-1} (prevailing visibility 10 miles) between 10 am and 6 pm when relative humidities are below 70% (Robinson, 1962; Samuels *et al.*, 1973; BAAQMD, 2002). This statewide standard doesn't set natural conditions as its goal, but obviously could be adjusted to do so by lowering the extinction limit in Class I areas.

The California standard differs qualitatively from the federal rule in several ways:

a. It involves much shorter averaging times: the "adverse" determination refers to an 8h period, and progress is tracked in terms of the number of adverse days per year. A similar visibility standard involving 4h averages was established by Colorado in 1989 for the Denver area (Ely *et al.*, 1991). Phoenix, Arizona has an interim visibility target involving 6h averages (ADEQ, 2002).
b. It does not apply overnight: California, Colorado, and Phoenix target only the daylight periods 10 - 18, 08 - 16, and 06 - 18, respectively.
c. It exempts periods of high humidity: The California and Colorado extinction standards apply only when relative humidities are below 70%.
d. It is couched in terms of visibility rather than aerosol chemistry.

Each feature of the 1959 California standard – short averaging times, a focus on daytime conditions and moderate humidities, and optical monitoring – is tailored to the goal of protecting visibility with a minimum of technical challenge. Each of these thrusts was implicitly rejected in EPA's formulation of the 1999 Haze Rule and subsequent guidance. Although the review "is meant to be provocative", it seems rather complaisant in accepting the Agency's framework. I want to explore the logic of the California alternative, not to challenge EPA's choices so much as to illuminate connections and implications that are slighted in the review.

People experience visibility the same way they experience temperature – continuously, in real time. Temperature is typically reported as the current value, or as the day's maximum or overnight minimum, and with time people develop a sense of how "x degrees at y o'clock" will

feel when they step outside. No such sensory interpretation is available for average temperature – the average of a 95 degree day with a 55 degree night does not feel anything like 75 degrees. **Figure 1** shows that debates about visibility metrics and perceptibility thresholds are similarly academic when couched in terms of 24h averages, not to mention 5y averages of these.

The review acknowledges the desirability of better time resolution, and welcomes the promise of continuous aerosol monitors. It overlooks, however, the simple option of diurnally-resolved filter sampling, which has always been available. Early haze monitoring programs that collected 2h to 12h samples include the Tri-City Study (Samuels *et al.*, 1973), RAPS (Jaklevic *et al.*, 1981), SURE (Mueller and Hidy, 1983), ERAQS (Mueller and Watson, 1982), WRAQS (Tombach *et al.*, 1987), and SCENES (Mueller *et al.*, 1986). **Figure 2** shows some daytime-only observations made in Los Angeles over 30 years ago.

Figure 2 shows, among other things, that some researchers had already associated haze with fine particle mass by the time of the first Clean Air Act. Data supporting a simple proportionality between low-humidity extinction and fine-particle mass concentrations accumulated over the ensuing years (Waggoner *et al.*, 1981), leading to consideration of a secondary NAAQS for PM based on visibility. As an alternative to an optical standard like California's, Lodge *et al.* (1981) proposed setting a 3h PM_2 standard at a level corresponding to the desired visibility. It is probably best for us not to dwell on how much better we might understand fine-particle health effects today (USEPA, 2002) had we begun such visibility-motivated monitoring of fine particles 20 years ago.

The 1970's also saw the first regression-based apportionments of optical extinction to distinct chemical fractions of the aerosol. It must be understood that these early efforts did not contradict the simple extinction/fine-mass proportionality reported by others. White (1976), White and Roberts (1977), Cass (1979) and Trijonis (1979) were working with data from total filter samples, collected at generally moderate ambient humidities. They were accordingly using chemistry largely as an available proxy of particle size, employing this information to distinguish the predominantly secondary fine particles from the predominantly crustal coarse particles. It is in fact doubtful that regression can distinguish reliably between the optical effects of secondary sulfates, nitrates, and organics, at least at moderate humidities (Vasconcelos *et al.*, 2001).

With the exception of black carbon, which is usually a minor constituent at the regional scale, the main chemical fractions of PM2.5 are roughly interchangeable with respect to their effects on particle refractive index and density (Sloane, 1983, 1984). The potential interactions of chemical composition with the size distribution of submicrometer particles are complex, poorly characterized, and poorly understood (White, 1986). These considerations leave light scattering by water associated with hygroscopic particles as the main optical effect that requires chemical information in addition to a size-resolved mass concentration. The strength of this effect of course increases with ambient relative humidity.

The focus on chemical resolution in the Haze Rule (and Critical Review) thus stems primarily from a desire to account for the optical impacts of particle water. (Particle chemistry also carries useful information about the sources of haze, of course. But chemical data can be collected outside the strictures of compliance monitoring, as is done with the health-based fine-particle standard.) At high relative humidities, hygroscopic species such as sulfates and nitrates have greater effective scattering cross-sections for a given measured (dry) particle mass than do other fine-particle components.

The importance of accounting for particle water obviously varies with ambient humidity, as the formulation of the California standard recognized. At or near saturation, visibility can be obscured by fog and rain even in a pristine environment, and the review notes that precise measurement even of humidity itself becomes difficult. Accordingly, IMPROVE optical measurements at humidities above 90% are routinely withheld from summary calculations as subject to "weather interference" (Gebhart *et al.*, 2001). The EPA's Haze Rule guidance, on the other hand, requires the inflation of scattering by hygroscopic species at all humidities up to and including 98%. As **Figure 3** shows, averaging-in the most humid observations greatly increases the optical impact imputed to hygroscopic species. Moreover, the bulk of this increase is imputed to the nighttime hours, particularly those after midnight when humidities usually peak (*cf*. **Figure 1**).

EPA's choices of a 24h measurement period and a 0-98% relative humidity range thus conspire to shift our unwitting focus to the hours after midnight. Together they create a need for (i) chemically resolved particle measurements, to distinguish the hygroscopic and non-hygroscopic portions of the aerosol, and (ii) climatological averaging of humidity, to avoid extreme sensitivity to sensor error and meteorological variation. The need to send filter samples off for laboratory analyses means that "regulatory visibility", in terms of deciviews, cannot be reported in real time like temperature can. The use of climatological factors in its calculation means that, once reported, it will not correspond to the actual experience of any particular day. It seems a shame to take the most immediate and accessible of all air quality values and render it so ill-suited to public education and consciousness-raising.

The good news, as the critical review also notes, is that differences in the characterization of visibility have only limited impact on the emissions management decisions that will ultimately be required. For all of the chemical detail and extreme humidity dependence embedded in the calculation of "chemical" extinction, **Figure 4** shows the result nevertheless to track fine-particle mass concentrations reasonably well, particularly within individual geographic regions. If "natural" fine particle concentrations are understood to be 1.21 $\mu g/m^3$ in the west and 2.25 $\mu g/m^3$ in the east, as the guidance currently tells us, then broad and deep emissions reductions will be required to approach natural visibilities no matter what accounting framework we use.

References: (Asterisked entries were included in the Critical Review.)

Arizona Department of Environmental Quality (2002) Final Report, Governor's Brown Cloud Summit. At www.adeq.state.az.us/environ/air/browncloud/download/visibility/final.pdf

Bay Area Air Quality Management District (2002) Ambient air quality standards & Bay Area attainment status. At www.baaqmd.gov/planning/resmod/baas.htm

Cass (1979) *

Ely D.J., Leary T., Ross D.M., and Stewart T.R. (1991) The establishment of the Denver visibility standard. Paper presented at AWMA Annual Meeting in Vancouver.

Gebhart *et al.* (2001) *

Jaklevic J.M., Gattis R.C., Goulding F.S., Loo B.W., and Thompson A.C. (1981) Aerosol Analysis for the Regional Air Pollution Study. USEPA report 600/4-81-006.

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Mueller and Hidy (1983) *

Mueller P.K. and Watson J.G. (1982) Eastern Regional Air Quality Measurements. Electric Power Research Institute report EA-1941.

Robinson E. (1962) Effects of air pollution on visibility. In Air Pollution, edited by A.C. Stern, Academic Press, New York.

Samuels H.J., Twiss S., and Wong E.W. (1973) Visibility, Light Scattering and Mass Concentration of Particulate Matter. Report to California Air Resources Board by California Air and Industrial Hygiene Laboratory.

Sloane (1983, 1984) *

Tombach (1987) *

Trijonis J. (1979) Visibility in the Southwest – an exploration of the historical data base. *Atmospheric Environment 13*, 833-843.

USEPA (2001) * (tracking guidance)

U.S. Environmental Protection Agency (2002) Air Quality Criteria for Particulate Matter, Third External Review Draft. USEPA Report 600/P-99/002aC.

Vasconcelos *et al.* (2001) *

Waggoner A.P., Weiss R.E., Ahlquist N.C., Covert D.S., Will S., and Charlson R.J. (1981) Optical characteristics of atmospheric aerosols. *Atmospheric Environment 15*, 1891-1909.

White (1986) *

White and Roberts (1977) *

White W.H. (1976) Reduction of visibility by sulphates in photochemical smog. *Nature 264*, 735-736.

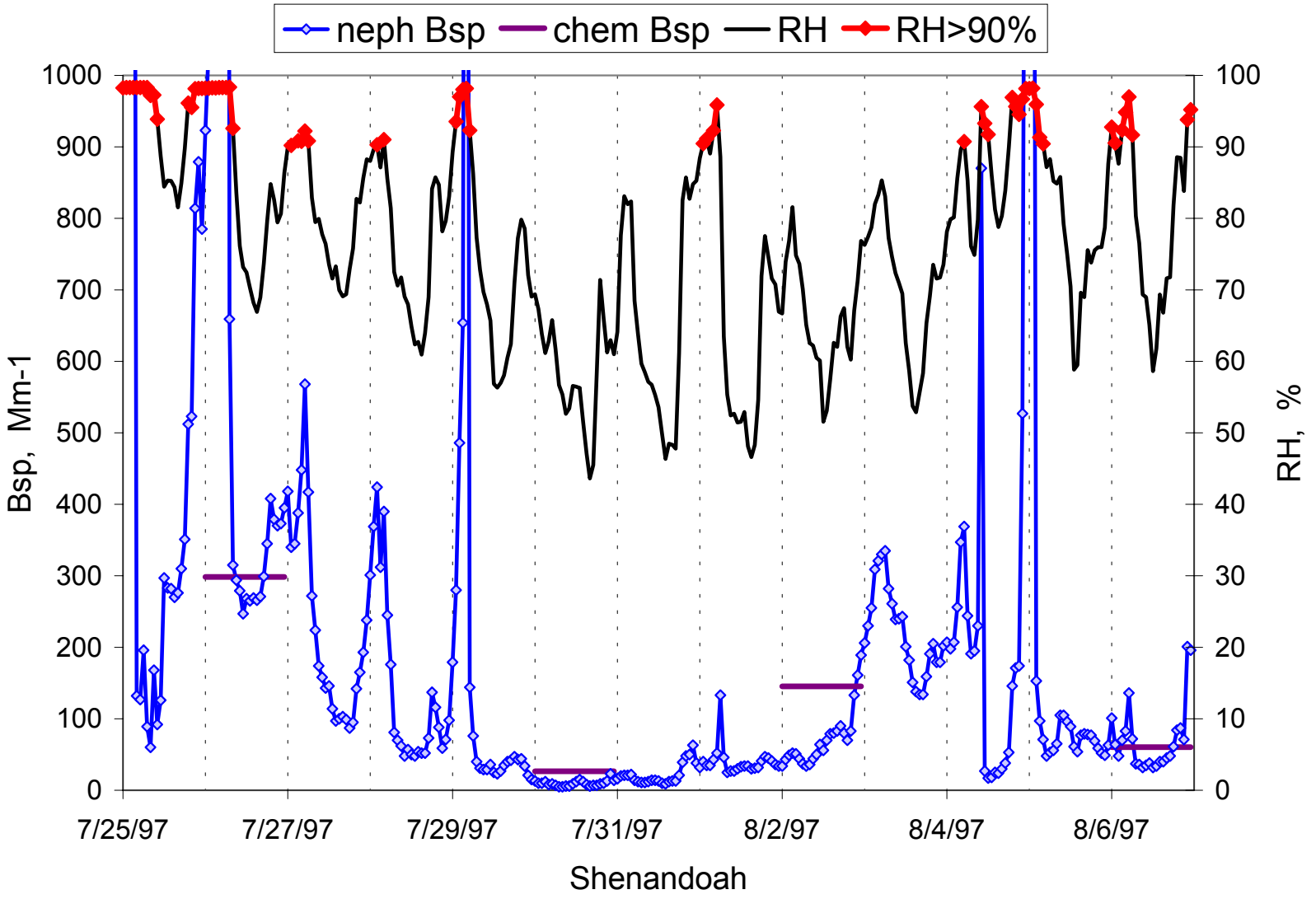


Figure 1. Hourly variations in particle light scattering at Shenandoah National Park, from data on the IMPROVE web site (vista.cira.colostate.edu/improve/). Hourly nephelometer and relative humidity data are shown, along with "chemical" light scattering derived via EPA guidance from 24h filter samples (USEPA, 2001, Equation 6).

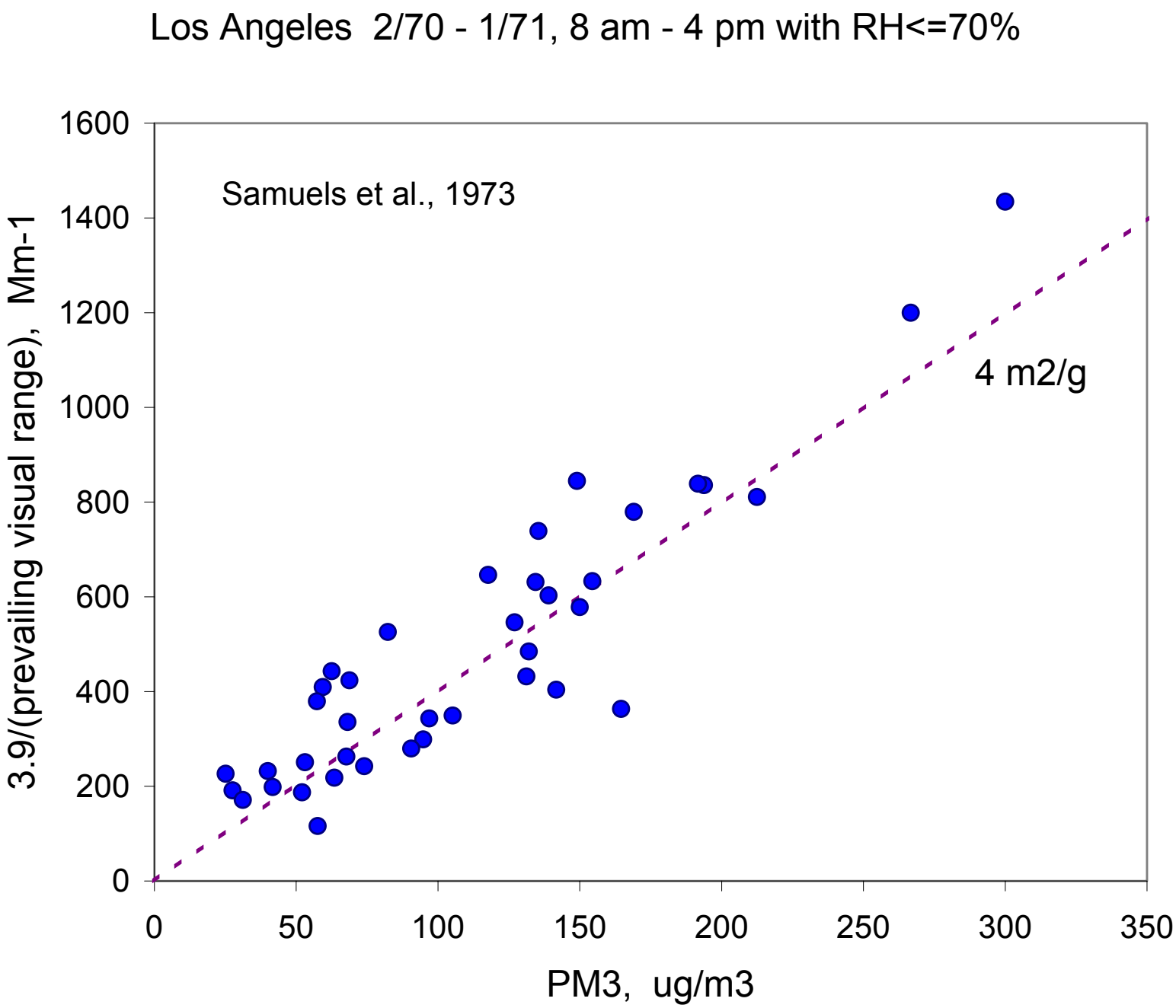


Figure 2. Proportionality of observed daytime haziness to fine particle mass concentration, from data of Samuels *et al.* (1973), page C-2. Visual ranges are 8h averages of hourly human observations, plotted as extinction according to the Koschmieder formula as indicated. Mass concentrations are from 8h samples collected behind a cyclone with 3 μm cutpoint. Relative humidities were measured by sling psychrometry.

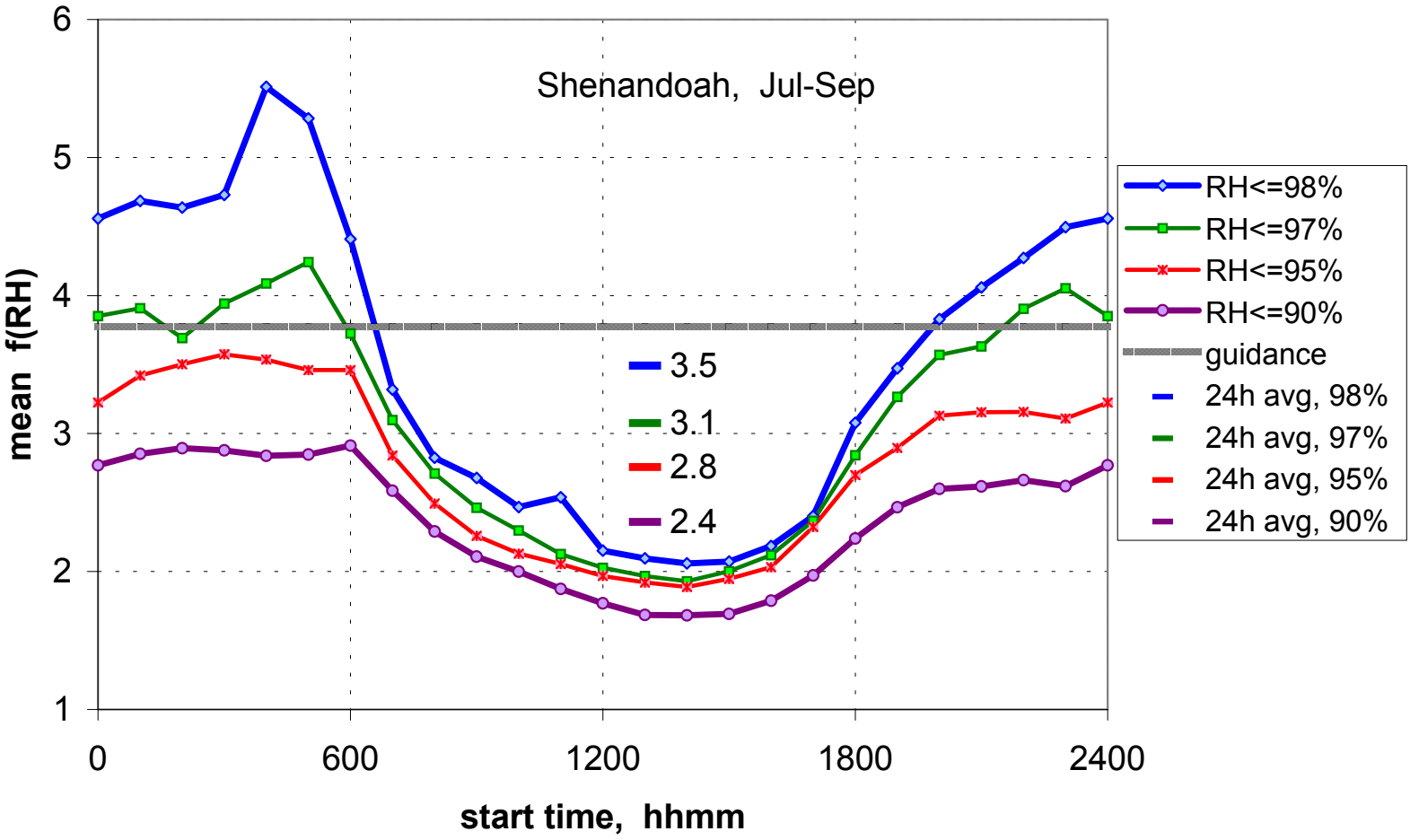


Figure 3. Sensitivity of imputed water to relative humidity limits, from two years' data (9/96, 7-9/97, 7-8/98) on the IMPROVE web site (vista.cira.colostate.edu/improve/). The curves show diurnal cycles calculated following the completeness and averaging procedures described by EPA (USEPA, 2001), but for alternative limits on allowable relative humidities. The indicated 24h value (3.8) from the guidance was calculated by EPA from ten years' data with a 98% limit.

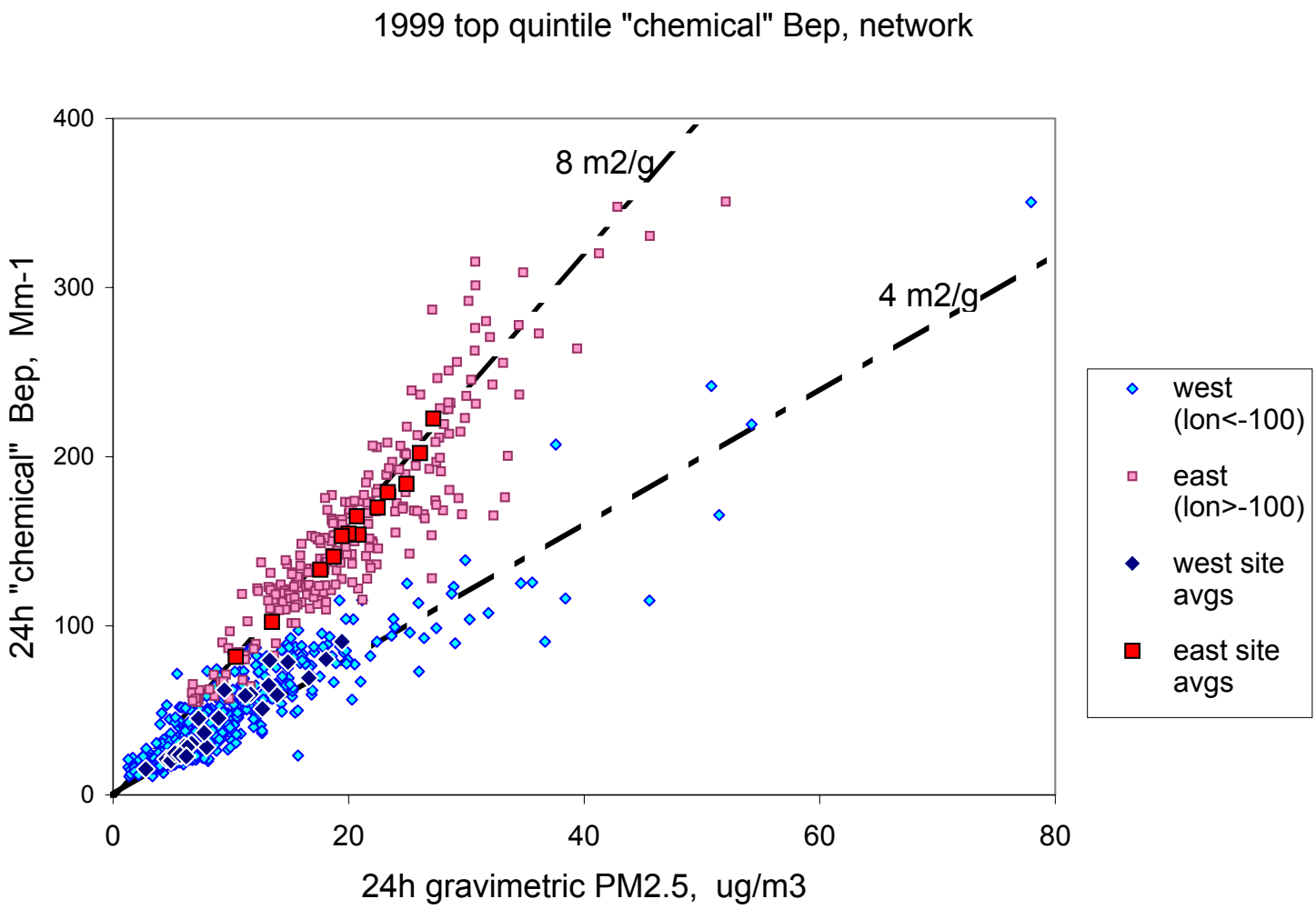


Figure 4. Proportionality of 24h "chemical" extinction to 24h fine particle mass concentration across the IMPROVE network, from data on the web site (vista.cira.colostate.edu/improve/). The haziest 20% of 1999 observations at each site are shown, distinguished as west or east of 100 degrees west longitude.

Dr. Robert Rowe

My comments are focused on Chapter 4, and the related materials in the Executive Summary. As requested, I also reviewed Chapter 9 and found the presentation generally well organized and presented in a manner that highlights the information relevant to the next steps in standard setting. Comments on the Executive Summary are included at the end of this memorandum.

CHAPTER 4 COMMENTS

The sections on visibility, materials, and climate change are well organized, cover appropriate topics and content, present the material factually, and are of suitable length. The revised climate

sections (and eliminated appendices) are a significant improvement over prior versions. My comments on Chapter 4 concern (1) the vegetation and ecosystems sections, (2) economics, and (3) Other text edits.

Vegetation and Ecosystems

Section 4.2, and in particular the ecosystem sections, need improved presentation and shortening.

1. Message and length. From the material presented one learns that ecosystems may be stressed by PM constituents, but that beyond selected vegetation exposure and effects, and a few other effects discussed, our understanding is limited. Because of the importance of ecosystems to human life (and to other components of the ecosystem), investigation into ecosystem effects of PM needs to receive continued attention. These are not contentious nor unimportant points. Unfortunately, these simple factual messages are confounded by the organization and generic content about ecosystem functioning that are not clearly relevant or necessary to present at length given the limited subsequent PM related knowledge, and the messages are compromised by inappropriate normative discussions. Past recommendations to reduce length and identify and focus on information relevant to secondary standard setting were not well attended to (See Taylor comments and Hopke summary letter). Examples of unnecessary repetition include:

1. Summaries are in multiple places: page 4-63 and at the end of the chapter.
2. Repetition of vegetation discussions (deposition, mechanisms, impacts) in Sections 4.2.1 and 4.2.2.2
3. Repetition between section 4.2.2.2 and 4.2.4.
4. Discussion of regional haze and SUVB impacts on vegetation in Section 4.2 and Sections 4.3 and 4.5 (it belongs in one section with only cross references in the other sections).

2. Introduction and organization. Section 4.2 could benefit from an improved introduction and organizing principles.

1. If an introduction presented ecosystem concepts and linkages for impacts, this would motivate the subsequent sections, and chapter would not need bounce back and forth between vegetation and a broader perspective. A chapter and ecosystem introduction could be brief, factual, and straightforward covering generally agreed concepts and frameworks. Information for this is already in Sections 4.2.2.2 and 4.2.4, but it needs improvements and simplification. An introduction could:

a. Identify that ecosystems are a complex mix of communities of plants, animals, insects, and the physical environment (Figure 4-23). Individuals belong to populations, and populations and physical environment interact, such that effects, for example, on a tree leaf affect the tree, the forest, and other components of the ecosystem. Human life depends on life-support systems of ecosystems, which provide a broad array of services to humans, as well as to other parts of the ecosystem (Tables 4-13 and 4-17).

Identify linkages as the set-up for the subsequent sections, such as (1) exposure/deposition (old Section 4.2.1), (2) effects on individuals, (3) effects on populations and ecosystem services, and (3) reductions in services provided to humans human service impacts (not really addressed in any specific way).

Briefly identify what is and is not know specific to ambient PM. In short, most of our understanding for ambient PM is focused on vegetative effects, which are discussed below. Less is known with regard to effects on other terrestrial or aquatic life (other than for specific toxics, metals, acid deposition...).

Ample references, as now exist, reduce the need for long discussions about ecosystem function that are not directly or indirectly tied to subsequent PM deposition and effects.

Much of the vegetation discussion (which also is much of the ecosystem discussion) concerns acidic deposition, metals, and other PM constituents that are (also) regulated under other rules and discussed at length in other documents, as properly identified in the introduction on page 4-2. Thus, additional introduction would help the reader understand the motivation for extended discussion related to these compounds and/or the selection of materials presented, and why the discussion is focused on vegetation impacts from these constituents and not other ecosystem impacts of these constituents (e.g., fish, wildlife, water quality,….).

Section 4.2.1 jumps to particle deposition without motivation (which can be readily fixed as above) or an introduction for the organization and inclusion of content in this section, which can be readily rectified. The same holds for Section 4.2.2.2. The inclusion and organization of materials in both these sections should be reviewed within the overall organization principles, and reviewed to eliminate all repetition with other sections.

3. Section 4.2.4. This section has the most problems and should be removed.

Parts of this section should be merged into an ecosystem introduction (now on pages 4-63 to 4-63) for a cohesive presentation and to remove repetition (e.g., move page 4-137 lines 5-7, Table 4-17 replacing or merged with Table 4-13, Figure 4-23, and page 4-140 line 23 through page 4-141 line 8).

Page 4-137 lines 8 through 31 are normative commentary and admonishment that does not belong in the CD, and most of which has no clear link to PM standard setting.

Page 4-139 lines 6 to page 140 line 22 provide questionable economics, and further inappropriate normative discussion (see below).

Economics

With one unfortunate exception, economics has been deleted from Chapter 4 (and the entire CD). There are few new relevant welfare effect studies since the last CD (e.g., damages of air pollution on cultural materials, reanalyzes of prior visibility studies, and limited other work). If these and past economic studies can be used in subsequent standard setting steps without reference in the CD, omission of economics (or human perceptions and values in general) is OK. However, without this literature, there is little in Chapter 4 to indicate the welfare significance to humans of the actual and potential visibility, materials, and ecosystem impacts identified.
The one exception to the omission of economics is the continued citation to monetary values for all ecosystem services (Pimentel et al., Costanza et al), and the subsequent discussions on page

139 –140. As noted and referenced in my prior comments, the veracity of these estimates are highly suspect. Worse, the information content from these estimates is zero in this standard setting. Regardless of what the total value of all ecosystem services is, there is no particular percent of all ecosystem services that can be even vaguely assigned as being impacted (positive or negatively) by ambient PM (although it appears to be very small, but probably not inconsequential) – it would be much more appropriate to value the services that can be identified as impacted by PM. These economic numbers and discussions are far less valid and useful in PM standard than the omitted economics literature for visibility and materials.
Next, the continuing discussions on 4-140 based on Heal (2000) and Harwell et al (1999) is not constructive and misses the point that when we have biologic measures of PM impacts, economics may then be able to provide additional PM-effect-specific information to also assist in decisions. This section contributes to the unnecessary tenor of the ecosystem discussion as normative preaching rather than frank factual presentation.

Other Chapter 4 Edits

Page 4-1 lines 6 through 14 seem unnecessary. Just start with line 15 "This chapter addresses..." and add "relevant to secondary standards aimed at protecting against welfare effects of PM."
Page 4-1, lines 21-29. These lines repeat earlier information and only obliquely add to the apparent intent of page 4-1 lines 27 through page 4-2 line 2 to identify that the standard is primarily particle size based while vegetative and ecosystem effects have focused on components, as will be discussed in this chapter.

Page 4-65 Lines 7-8: The sentence "Services usually are not considered to be items with market values" is not correct. Ecosystem services include items with readily recognized market values (fish, timber, minerals), and other services without current or readily identified market values (see page 4-203).

Page 4-66 lines 3-13 have an unnecessary normative tenor.

Page 4-66, lines 16 and 17. The topic sentence is immediately contradicted by the next sentence – suggesting ecosystem response starts at the response of individual plants and animals to environmental stresses.

Page 4-82 line 31 discusses an "especially severe" case of regional haze resulting in a 3% to 5% reduction in crop yields. While interesting, the relevance is unknown without some information on the level of the regional haze/ambient PM concentrations in the case study.

Section 4.6 summary. The vegetation/ecosystem section can be improved here as well.

The vegetation/ecosystem is disproportionately long and would benefit from a substantial reduction in length.

Page 4-200. The lead sentence, as written on lines 29-30, is in direct contrast to the earlier presentation and needs to be rewritten. PM deposition to and its impact on vegetation and

ecosystem has been more defined by its chemical components, structure and source, not by size fraction (See next sentences in Section 4.6.1 and page 4-1 lines 29 through page 4-2 line 2).

Page 4-203 lines 3 through 7. Could we broaden these categories. Include water resources in market values. Edit the second to "the use and appreciation of ecosystems for recreational, aesthetic enjoyment and study, and the protection of ecosystems for human and ecosystem well-being." The later category include the services identified in the next paragraph and provides a link to the next paragraph.

Page 4-203 sentence starting on line 12 through line 15. These comments are inappropriate in this document (e.g., the health section of the CD doesn't discuss other health stresses unless related to PM stress).

Page 4-203 starting on page 16 starts over discussing the PM mixture of interest discussed on page 4-200 to 4-201. These discussions should be merged and shortened.

Page 4-205 lines 9 through 29 repeat discussions of haze and SUVB repeat comments on pages 4-211, although the length of repetition in these sections is less a concern than in the main portions of the chapter and the Executive Summary.

See also the Executive Summary comments below.

EXECUTIVE SUMMARY

Adding your overall analysis flow chart (Figure 1-1) would motivate the organization of the executive summary materials, at least for the health analyses.

Some subsections have introductory sentences, while others do not. Those that are there are useful because they are succinct.

Section E.5 should be titled "Welfare Effects" rather than "Environmental Effects." This section is missing an introduction (although a part of one inappropriately shows up under visibility), and it would be useful to identify welfare effects are relevant to the secondary standard. Welfare effects are presented in groups of vegetation and ecosystem effects, visibility effects, materials effects, and climate change effects (through which PM emissions also may indirectly affect human health).

Some vegetation and ecosystem effect bullets are in contrast to the rest of the Executive Summary (and to those in the visibility, materials, and climate change sections).

This section seems disproportionately long compared to other sections.

Much of the first three bullets have little clear motivational linked to PM, and some of it seems inappropriately normative. One could blend parts of this into one bullet (in order) of: the second sentences of bullet 1, followed by bullet 3, followed by the first sentence of bullet 1. Then add

the missing important statements for this section: our knowledge of PM effects on the ecosystem is limited. Much of what we do know is related to vegetation effects. Because of the importance of the ecosystem to human life, there is a need for further understanding of the effects of PM constituents on all components of the ecosystem.

Section E.3.5 on visibility. The reference to Chapter 4 as a whole (first and last sentence on the introduction) belongs in the introduction to E.5.

Section E.5.4 on climate change and E.5.2 on vegetation/ecosystem repeat materials on haze and SUVB.

Section E.6 probably should have a lead sentence identifying why the key conclusions are all related to human health effects and none are related to welfare effects, which appears appropriate.

Dr. Paul Lioy

Review of Chapter 5

General:

There continues to be improvement in the chapter, but there are still some issues that need attention in order to bring the document to closure.

Three main concerns still exist. One is the lack of a coherent discussion about the fact that exposures are affected by the distributions of many variables and parameters, and that a single point in space or time along each distribution cannot easily describe each person, especially among those at risk. A separate section would help.

The second concern is the continued lack of understanding in the first 80 pages that exposures are only significant if they are associated with a biologically relevant duration of contact with a compound or material of concern. There needs to be more attention to this issue. It is just not a concept restricted to chapters 6, 7, and 9.

The third concern is a lack of a section that provides the key points of this chapter for use in Chapter 9 and then the staff paper. As presented, there is no weighting of the bullets in the summary.

Specifics:

The equation for $= \sum \ldots$

is wrong. (#5.2) You do not include a 1/ T unless the left hand side of the equation is an average E (over time). Please make two separate equations to correctly define each concept.

P. 5-10. The authors discuss CHAD and distributional analyses. However, the equations presented in the text for exposure have no terms that reflect probability to distributional function. I suggest adding another equation that describes exposures within the context of distribution functions for various parameters. See Georgopoulos and Lioy, JEAEE, 1994, pg 281 for an example of the necessary equations.

P. 5-12. COH will only be useful in fine particle situations that include absorbing particles – not light scattering particles.

P. 5-12. Please spell out EOHSI – Environmental and Occupational Health Sciences Institute.

P. 5-16. Equations 5-5 and 5-6 can be misinterpreted. Please re-write as:

$$C_a = C_{a0} \left(\frac{P_a}{a+k} \right)$$

$$C = \frac{Q}{V(a+k)}$$

P. 5-18, Line 18. What does Cs stand for?

P. 5-19. I am not sure of the over emphasis in this chapter on SO_4^{-2}. It is not an indicator for all PM. SO_4^{-2}, however, is a good indicator of fine particle infiltration indoors.

P. 5-20. I don't think the word "convenience" is appropriate or representative.

P. 5-22. What is a "pooled" study? Please reword and avoid inventing new terms.

P. 5-24. Not only were the elderly without sources, but also they did not appear to have any activities (stationary elderly subjects).

P. 5-31. What was the significance of the Mage results? He described only one season during the year (need a context).

P. 5-35. Averaging reduces the variance in any system. This value has no significance for the population or individuals at risk. You need to link the population at risk to the exposures of concern e.g., concentration, time, and composition.

P. 5-49, Line 21-24. SO_4^{-2} can be a surrogate for certain components of PM, but not all depends upon the composition and size distribution.

P. 5-51, Line 17. The case is just that, a Case – not the norm.

P. 54-55. Very little discussion about the PAH results; outdoors or indoors.

P. 64, Line 8-10. ETS is an indoor pollutant that can be the most significant contributor to the mass.

P. 64. Indoor chemistry is important because outdoor sources contribute to one or more major "precursors" e.g., O_3, NO_2. Thus, the particles produced could be considered part of the ambient risk.

P. 5-16, Line 18. Long et al. results need to be placed into a context that indicates ultra-fine particle numbers will be higher near sources. Both outdoors and indoors. We need to understand the biological significance of such exposures.

P. 5-68,69. A distribution function for one or more variables is critical for this section. The non-educated reader may not get a true perspective about the range in values for specific variables.

P. 71. One of my concerns with source apportionment is the lack of good estimates for automobiles without sophisticated tracers.

P. 74-85. There is a lack of focus for this section. Please explain the purpose of listing all correlations, and summarize the important points on page 85.

P. 88, Line 13-14. If the composition is different, the relative risk for individual constituents will not be the same.

P. 88. I think there needs consideration of the fact that the type of PM, composition, and duration of contact may lead to a specific or multiple "effect(s)".

P. 89. I do not think there is a "paradox." There is a very specific scientific explanation: the presence or absence of indoor or personal sources. However, the most important point is whether or not the PM or its components are biologically active for the duration of the contact with the PM – ambient, indoor, or personal. The other bullets (points) are all complementary to the above.

P. 91, Line 3. Driven by indoor sources, building design, etc.

P. 91, Line 5. Important point it should be amplified by stating the possibility of being "biologically active."

P. 91. I am not sure if it is constant; however, the mean values had little variability. Thus, one would not expect large variations between cities, but there could be variability within specific sections of a city (e.g. near tunnels etc.).

P. 93. The exception is additions to indoor PM exposures caused by reactive gases (e.g., ozone) highly correlated with outdoor PM and can form secondary aerosol indoors. The variation of the indoor PM products would reflect the strength of correlation between outdoor PM and the outdoor gaseous contaminant, the latter would react with VOC emitted by indoor sources.

P. 93, Line 13. In some cases, the elderly studied were very inactive.

P. 93, Line 24. Finally, the authors mention of the concept of particle toxicity.

P. 94, Line 3-4. This concept has not been factored into the analyses discussed throughout this chapter. It is needed to link chapter 5 to chapters 6-9. How do we discuss relative risk without information on the toxicity of ETS, indoor aldehydes vs. outdoor PM?

P. 96. The authors need to describe the "framework" for error analysis and its purpose. The symbols in this section need to be consistent or listed in Table 5-1. Some are confusing. Please describe the coherence between two different measures:
concentration C_t^* and exposure $\overline{E}_T$

Does the difference between these two variables have physical significance?

P. 98, Line 21-27. This point may be statistically valid, but has to state why this variable has any relevance to one or more health endpoint. What needs to be determined is "whether or not is biologically relevant in "susceptible populations.

P. 99-100. May need to be revisited after the epidemiological analyses.

P. 102, Line 4-5. A very important point not clearly articulated throughout most of this chapter.

P. 102. Could the authors please differentiate the "key" findings from the secondary findings – necessary for closure? Right now all appear to have equal weight.

P. 104, Line 27. Also need to identify toxicity of PM or components.

Dr. Gunter Oberdorster

Chapter 5: Human Exposure

A major shortcoming of this chapter is the lack of discussion on the exposure assessment for ultrafine particles. There are several studies describing measurements of levels of ultrafine particles, ranging from early studies by Brand *et al*. (1992) in Germany to other European studies in the late 90's and 2000-2001; also studies in this country performed and summarized by Cass (2000), from the ARIES study in Atlanta; from the Southern California PM Center; and from Kittelson *et al*. (2001) on Minnesota Highways, finding particle number concentrations as high as $1x10^7$ particles/cm^3. Only the first two of the studies listed above are briefly mentioned in Chapter 3, but only with reference to their low mass without stating actual levels, and none are listed in Chapter 5. The measured number concentrations should be listed here since they reflect human exposure to ultrafine particles.

Chapter 6 – Dosimetry of Particulate Matter:
General comments:

This chapter is logically structured and most areas are discussed in reasonable depth. Some general remarks should precede the individual comments, and they include the following: A good lead-in from Chapter 5 (Exposure) would be to start with a sentence or two describing the Exposure – Dose – Response paradigm. There are several figures showing older data of regional deposition in the respiratory tract of different particle sizes. Whereas these figures are useful to show the inter-individual large variability of deposition efficiencies of inhaled particles, I suggest to limit that to one figure at the beginning of the deposition chapter and then including figures of newer models that have been published, *e.g.*, the ICRP model; this makes it easier to understand differences of deposition between different particle sizes and also differences between different animal species, for example, between rats and humans using the MPPDep model.

In several instances it would be essential to have a more critical evaluation of the information presented, for example, with respect to translocation studies of ultrafine particles (to be addressed later under Specific Comments).

Finally, the section on computational fluid dynamics (CFD) seems to be rather lengthy and I am not sure that this is needed. It would be helpful at the beginning of this section to indicate the potentials for CFD studies with respect to hot spot predictions for inhaled particles, but on the other hand it should also be emphasized that inter-individual variability is so large that a greater accuracy of the overall deposition will not be achieved with CFD computations. Presently available models are not well suited for the purpose.

Specific Comments

Pg. 6-1, line 9: Add "and deposited" after "inhaled"

> Line 21: Delete "particulate" and add after "processes": "the nature of these removal processes is different for solid and soluble components of the inhaled particles."
>
> Line 23: Add after "undergone" the words "clearance or …" It is not clear to me what difference is made here between the terms clearance and translocation. In line 28/29 of this same page translocation is listed next to clearance, implying that these are different removal processes, whereas in this paragraph these two terms seem to be equivalent. As translocation is defined here, it certainly is used mostly to reflect clearance. A clear distinction between the two terms has not been defined in this section and I don't even know what this distinction should be, unless translocation is meant to only describe the physical movement of a particle within the respiratory tract whereas clearance refers to its movement out of the respiratory tract. This, however, would not be consistent with the usual usage of the term translocation out of the respiratory tract.

Pg. 6-3, line 6: What is an "effective" inhaled dose? I suggest to delete "effective"

Line 21: I suggest that here as well as in other places (for example, page 6-4, line 27), phrases like "it is important to note that" or "it should be noted that" be deleted throughout this section.

Pg. 6-5, Figure of respiratory tract regions: It would be helpful in this figure to include also the volumetric depth for each region which would make it easier to understand the method of bolus technique described later in this section.

Pg. 6-6, line 16: The statement that all particles are continuously influenced by gravity is not correct, I suggest to change it to "particles >0.5 μm are increasingly affected by gravity".

Pg. 6-7, line 1: The particle size range given here for being minimally influenced by impaction and sedimentation or diffusion appears to be too wide, I would suggest to decrease it to 0.3 – 0.6 μm.

Line 5: Delete the first sentence and replace it with: "Deposition by interception occurs when an airborne particle traveling close to the airway surface contacts it."

Line 10: A charge of 0 is no charge at all, a minimum charge would be 1.

Pg. 6-9, line 4: Again, reduce the range of particle diameters to 0.3 – 0.6 μm.

Figure 6-2: As indicated before under general comments, I suggest to leave this figure in showing the large inter-individual differences but at the same time showing the general shape of the deposition curve as well as pointing out that the minimum deposition occurs for particles between 0.3 and 0.6 μm. This makes also more sense when examining curves of particle displacement due to sedimentation as well as diffusion.

Pg. 6-10, line 11: Define the term diameter here by changing it to CMD, count median diameter.
Line 20: I suggest to stick with one definition for the number concentration and call the diameter "count median diameter" (CMD) as is generally used.

Pg. 6-11, lines 1-7: The differences in ultrafine particle deposition between men and women could be due to differences in breathing pattern. In some studies, controlled breathing with a given volume is used and in others normal unrestricted breathing, so that I am not sure that a statement really can be made that there is a gender difference in particle deposition under normal breathing conditions. This critical comment ought to be included in this section here.

Pg. 6-13, Figure 6-4: This figure as well as the following figures taken from the earlier publication by Schlesinger should be updated by using one derived from the ICRP prediction model which makes it easier to understand, or also a figure derived from the MPPDep model. I attach a figure for regional as well as total deposition based on data from the ICRP publication of 1994. This figure also includes a wider range of particle sizes going down to the ultrafine particles below 10 nm. Furthermore, the legend of Figure 6-4 as well as for all the other figures showing deposition data state that the particle diameters are MMADs. If the data really were reported as MMAD, then deposition, indeed, is very different depending on the geometric standard deviation for each study. So I don't think that MMADs, but just aerodynamic diameters, are meant here.

Pg. 6-14, line 21: It says here that ET deposition has not been extensively studied, "and this remains the case". However, on the next page, several studies are listed which, indeed, did investigate ET deposition in humans.

Line 30: The value given here for nasal deposition for 1 nm particles of 54% sounds rather low, given that total nasal deposition is essentially 100% at this size. Is the value correct?

Pg. 6-15, line 7: In contrast to the above comment, fractional deposition for 50 – 60 nm particles is given here as 94 - 99% which is much too high and certainly does not agree with other studies by Cheng and Swift, and it also does not agree with the ICRP model as well as with the MPPDep model. Please check the numbers, for this particle size the nasal deposition is minimal for ultrafine particles.

Line 27: The statement that the ET region, especially the nasal passages, act as an efficient filter for particles <100 nm is not correct. This is repeatedly stated in this section and it needs to be corrected. Ultrafine particles <100 nm and coarse particles have very different deposition efficiencies in all three areas of the respiratory tract and for the nasal passages range from a minimum below 10% to a maximum of close to 80% when they approach 1 nm in size. In contrast, deposition in the alveolar region is maximum for ~20 nm particles and in the tracheobronchial region for ~5 nm particles. This needs to be corrected to avoid any misconception by the reader that in general all ultrafine particles have a high deposition efficiency in the nose. This is simply not true as shown also by a number of studies by Cheng and Swift and others.

Pg. 6-16, lines 8 and 9: The studies of Scheuch using bolus technique for ultrafine particles should also be included here.

Line 17: There is a mistake here, change "ET region" to "TB region".

Pg. 6-19, lines 12-14: Can anything be said about the fold increase on cardinal ridges *vs*. the other regions? Is it an order of magnitude difference, less or more? This is particularly important for the design of *in vitro* studies in which generally orders of magnitude higher doses are administered without justification about *in vivo* relevance.

Lines 23-29: Here the same question applies, can anything be said about the increased dose represented by hot spots *vs*. averages?

Pg. 6-20, lines 29 and 30: Are these model calculations done with monodispersed aerosol?

Pg. 6-21, figure legend: What is meant under "normal" breathing conditions? I assume they were simulated rather than being normal.

Pg. 6-22, line 16: Replace "internal" with "airways of lower respiratory tract".

Line 29: How is it possible that the numbers of particles can increase due to hygroscopic growth? Meant is probably that the size increased rather than the numbers.

Pg. 6-23, line 2: After "polydisperse" add "hygroscopic"

Line 6: Replace "will" with "can"

Line 19/20: Are the ventilatory parameters between males and females really different if they are expressed per unit bodyweight?

Line 27: Did males and females breathe the same inspiratory flow rates, *i.e.*, meaning that they are higher per given lung size for females?

Pg. 6-24, lines 5 and 6: Move the last part of this sentence starting in line 6 to the end of the first part, *i.e.*, after the word "males".

Line 17: I am not sure I understand why in females the deposition would be more localized, localized in terms of hot spots?

Line 30: Insert before "particles" the words "3 – 5 μm".

Pg. 6-25, line 1: Was that done under controlled breathing or normal breathing? The issue of controlled breathing is very important in such studies when the issue of gender differences in deposition is evaluated. If, indeed, both genders breathe at the same minute ventilation that may lead to differences in deposition between males and females because of the generally smaller lung size of females.

Line 6: 40 nm is not a very small ultrafine particle as is suggested in this sentence.

Pg. 6-27, line 6: Percentages are given here for children and adults, what is the third percentage for?

Pg. 6-33, lines 24 and 29: 1 μm is given here both as a minimum of particle deposition as well as a peak. It cannot be both at the same time.

Pg. 6-34, figure: These figures are much too complicated; I suggest to use a much simpler figure, for example, one that has been published by Yu *et al*., showing very nicely differences in deposition between rats and humans. Either this figure or the figure on pg. 6-36 (would be even better) should be replaced for clarity reasons.

Pg. 6-37, line 5: Again, the deposition minimum size range 0.1 - 1 μm is too large and should be corrected to 0.3 – 0.6 μm.

Lines 13 and 14: Deposition of 1 nm particle is not highest in the bronchial airways, but in the nose.

Line 26: I am not sure that I understand this, if a model was used, why would they need a CO_2 exposure to increase minute ventilation? Or were those real *in vivo* experiments?

Pg. 6-38, line 4: Again, I am not sure what should be adjusted here – does it mean that rats have to be used while they are exercising?

Line 18: It would be valuable to give an example here about similarities or dissimilarities between doses delivered per unit alveolar surface in rats *vs*. humans. In this context, it would be as important to give some data on differences between regional surface areas between rats and humans, perhaps adding a small table for both species and their different respiratory tract regions with respect to surface areas as well as normal ventilatory parameters.

Lines 28 and 30: I suggest to add the term "mass" to the dosemetric "particle numbers".

Pg. 6-39, lines 2 and 3: What is meant by "deposition density"? It could be added here that this is the case because of greater minute ventilation per unit body mass in the rats.

Pg. 6-41, figure 6-12: I suggest strongly to mark those pathways which are questionable with a question mark; for example, the arrow going from phagocytosis by AM to passage through alveolar epithelium, and also the arrow going from phagocytosis by interstitial macrophages to passage through capillary endothelium.

Pg. 6-42, lines 29 and 30 and
Page 6-43, lines 1and 2: Is there any evidence that macrophages traverse or transmigrate the alveolar capillary endothelium to enter the bloodstream? If so, it should be cited, but I am not aware of any studies showing this.

Pg. 6-48, line 5: Add "smaller" before "particles within".

Lines 28 and following: Macroaggregated albumin has been used for a long time in nuclear medicine, however, a problem is the presence of about 10% of macro-aggregated albumin as well, and the interpretation of the studies have to be done with caution when assuming that they reflect particulate transport. Similarly, the stability of ^{99m}Tc-labelled ultrafine carbon also causes some concern in that the label comes off and may, in fact, attach to other larger-sized molecules so that the interpretation of the results is not easy with respect to ultrafine particle transport across epithelia.

Pg. 6-49, lines 10-22: Contrary to what is stated here, Takenaka *et al*. did not determine that ultrafine silver particles were seen in various extrapulmonary organs, they only determined silver. Since silver is known to be somewhat soluble in the lung, this study does not prove the appearance of ultrafine silver particles in extrapulmonary tissues as is clearly admitted in the authors' discussion in that paper.

Pg. 6-50: In this section on interspecies patterns of clearance, the studies by Kreyling *et al.* published in 1998 (*Radiation Protection Dosimetry* 79, Nos. 1-4: 241-243, 1998) should be included here.

Line 6: Delete "can".

Pg. 6-54, lines 6-9: Tran *et al.* suggested that the surface area is the better dosemetric rather than volume. The sentence in line 8 should be changed to "overload related effects have been noted in …." In line 9, delete "results in" and replace with "includes". In general, this chapter on overload should include a statement saying that under overload conditions mechanisms are very different than those observed undcr low-load conditions; and that one needs to be careful with the interpretation of studies using doses or inhaled concentrations in experimental animals which are in the overload range when evaluating mechanisms and effects of environmental particles.

Pg. 6-55, line 21: The statement that there are differences in deposition between nasal and lower airways using instillation or inhalation is trivial and doesn't need to be made.

Line 28: The tracheobronchial region extends down to the terminal bronchioles, it is not correct to state that the intratracheal instillation results in fairly even distribution of particles in the TB region.

Pg. 6-56, upper para.: The major difference between the two techniques is obviously the dose rate which should be mentioned: A technique should also be mentioned that is recently used more often, *i.e.,* oropharyngeal aspiration, described among others by Foster *et al*., 2000 and Keane-Myers, 1998. It is claimed that this technique results in a more even distribution in the lower respiratory tract than intratracheal instillation.

Line 8: It is important to emphasize the much higher doses given by instillation as is done here; I would even suggest to state that in general these doses are several orders of magnitude higher than realistic inhalation exposures.

Pg. 6-57, line 8: I suggest to replace "is" with "appears to be" and add to the end of the sentence the following: "… although the much higher doses delivered by instillation are probably the reason for this."

Pg. 6-63, lines 13-21: It could be pointed out here that this model is available as a multiple path particle deposition (MPPDep) as a software and can easily be used to construct deposition curves and compare rat and human deposition efficiencies.

Pg. 6-66, lines 22 and 24: There is a typo here, instead of $\mu g/m^3$, it should be mg/m^3.

Pg. 6-67, line 7: The statement is made that better predictions of particle deposition with new models will be made. However, given the large inter-individual differences which were pointed out before, I am not sure what this "better" refers to, and if it even makes a difference whether these predictions ever become more accurate compared to now.

Pg. 6-73, line 28: Along what was said before, the more sophisticated new models are nice, however, do they really improve our predictions?

On this page, lines 22 and 30 "ACQD" should be changed to "AQCD".

Pg. 6-74, lines 4/5: The statement that the nose is a very efficient filter for ultrafine particles is incorrect, the nose is only efficient for ultrafine particles below 5 nm, but then becomes very inefficient, and in fact, the alveolar region and tracheobronchial region have higher deposition efficiencies for 20 and 10 nm ultrafines. Like with larger particles, within the range of ultrafine particles (<100 nm) there are significant differences in the 3 regions of the respiratory tract as to the efficiency with which they deposit. Thus, the statement in line 9 of this page, that ultrafine particle deposition patterns are very much like those of coarse particles is incorrect, and should be deleted.

Line 14: The significant gender differences in deposition are addressed here and I am not sure whether this is due to the controlled breathing pattern used in most of the studies or whether this is also occurring under normal breathing conditions in males *vs*. females.

Line 23: Again, as questioned before, is there a difference between translocation and clearance of particles?

Line 27: Although there are some recent studies that ultrafine particles can be rapidly cleared into the systemic circulation, the ones cited in the document are not proof of such translocation. In the following line (line 28), I would suggest to replace "provides" with "may provide".

Review of Chapter 7, Toxicology of Particulate Matter in Humans and Laboratory Animals.

General comments:

This section lists a large number of new studies performed since the 1996 review. Although most of these are at high doses or concentrations — as is pointed out in the document correctly — this dose aspect is apparently forgotten when it comes to explaining and discussing potential mechanisms of PM. Overall, many of the sections need a more crticial approach when it comes to interpreting the results of specific studies.

Specific comments:

Pg. 7-1, line 17: I am not sure about the difference between tracheal and pulmonary instillation and suggest to use the term "intratracheal" instillation only. Also "nasal" or "nasopharyngeal" instillation are probably the same. A new method — to be mentioned later — is oropharyngeal aspiration which is not yet included here and later in the document.

Pg. 7-3, line 20: It is stated here that the high doses used in animal inhalation and instillation studies are necessary. However, are they really meaningful? The problem is that most often healthy animals are used, whereas epidemiology reports data in compromised humans. Therefore, there is a need to develop animal models of the human compromised condition. Here again, the problem is that the animal models are all of an acute nature whereas the human conditions are mostly chronically-developed diseases. The major underlying question then is as to whether high doses used in healthy animals or in animals acutely damaged induce effects by the same mechanism as are operating in humans with a chronic underlying disease and exposed acutely to very low doses of PM. Mechanisms are most likely to be very different.

Pg. 7-4, Table 7-1, entry 2, 3, 4: The term MMAD is used to describe particle sizes below 0.5 μm, whereas in the previous chapter on dosimetry it was made clear that those particles are not governed by aerodynamic but by thermodynamic properties, so the description should be changed here to either thermodynamic diameter or just leave it blank.

Pg. 7-6, line 22: It is my understanding that particle concentrators can concentrate up to 30 or 40-fold. What is the meaning of "true ambient PM" used in line 23?

Pg. 7-7, line 16: It is somewhat of a misconception that the instilled doses reflect exactly the amount that was instilled. Measurements immediately after instillation show that only between 80-90% of the instilled material are actually in the lower respiratory tract, there is some immediate clearance out of the lung into the respiratory tract (coughing).

Pg. 7-8, Table 7-2: I suggest to separate the table and subdivide into inhalation and instillation studies, and these again into human and animal studies. I also suggest to add another column with the heading "comments", or include comments in the "effect of particles" category. Among the comments could be references to extraordinarily high concentrations or doses being used in a specific study. Also in this table as well as in subsequent tables, the term MMAD is used to describe particle sizes when instillations are used. This is not very meaningful given that instillation study particles are suspended in an aqueous or saline medium and that they will be very different in terms of their diameters compared to the airborne state. What is probably meant here is that this is the size of the collected particles before they were put on a filter, but it should not be assumed that this is the same for the liquid suspended particles.

Pg. 7-14, Table 7-4: For most of the studies listed here, I would not think of the particles as being surrogate for ambient PM. For example, the first study is an overload study which has nothing to do with the environmental particle issue. The next three studies (our own studies) are dealing with PTFE fumes which are highly toxic and again have nothing to do with environmental surrogate particles. The last study is again a high dose study with carbon black and I don't think that this was meant to be an environmental surrogate.

Pg. 7-15, line 7: The term "nuisance" or "inert" dust should not be used anymore, there is no such thing like an inert particle and this term has been abandoned a long time ago. Instead, I suggest to use in line 11 the term "respirable low toxicity particles" instead of "inert particles".

Pg. 7-15 and following, section on ambient particulate matter: When describing the studies here using intratracheal instillations, the dose that was instilled should clearly be stated, for example, line 28 on page 7-15, lines 3 and 5 of page 7-16, etc. Also, when aqueous extracts of filters from PM are used, it should be stated how much of the PM was used to extract the amount that was subsequently instilled (page 7-16, line 25).

Pg. 7-19, line 9: I suggest to add "high concentration to DPM".

Line 12: What is the difference between an irritant mechanism and inflammation? Doesn't irritation of the mucosa also cause inflammation?

Pg. 7-23, line 22: As mentioned before for the tables, the term MMAD is used for particles that are instilled, and it should be explained whether this is for the airborne particles at the time when they were collected.

Pg. 7-24, line 17: I suggest to add to the sentence to read "…..albeit at very high instilled doses"

Pg. 7-25, line 12: Similarly, here I suggest to include before ROFA the words "high doses of "

Pg. 7-26, Table 7-5: The first two studies state that the levels were close to ambient levels: Is that true for 1.5 mg/m^3 and 5.7 mg/m^3 of sulphur and hydrogen? Also the study by Lee *et al*. with concentrations of 94 and 43 mg/m^3 of H_2SO_4 is not very meaningful.

Pg. 7-28, line 15: The issue here is not only the different chemical composition of the particles but also the very different solubility in the lung. Zinc oxide has a retention halftime of only 6 hrs., so that the bioavailability between the two compounds is very different.

Pg. 7-31?, Table 7-6: Again, I suggest to separate these studies by inhalation and by instillation. In the study by Kuschner et al. listed on this page of the table, it says that there were no significant differences found, yet in the text it says that zinc oxide was effective. Please check and correct.

Pg. 7-31, line 5: Dose of the instilled particles was 5 mg which should be indicated here.

Pg. 7-31, line 21: Is the term "active" referring to "bioavailable"?

Line 30: The term metals should be replaced by iron.

Pg. 33, Table 7-7: The first study listed here by Elder *et al*. was not done to determine the effect of bioaerosols, but to mimic early respiratory tract infection in humans by LPS. The deposited dose was 70 EU per rat. In the second study by Michel *et al*. the concentration designation is not complete, is it per m^3?

Pg. 35, Table 7-8: Again, I suggest to separate studies by inhalation and instillation.

Pg. 7-37: All studies listed here by Watkinson et al. list the same particle size of 1.95 μm, regardless of whether it was given by instillation or inhalation. What is it referring to, MMAD (see comments for instillation), and what is the GSD?

Pg. 7-38 and following: There are a number of studies described here using the MCT model. It would be helpful to include a critical discussion of this model: Since this is an acutely induced lung injury model causing eventually high pulmonary hypertension, is it really relevant for any human condition? Or, is it just used to show the proof of principle?

Pg. 7-40, line 27 – 30: It is stated here that inhalation exposure to ROFA was used because of concerns of the relevance for high particles administered by intratracheal instillation. However, do the authors really think that 15 mg/m^3 as an inhaled concentration addresses this concern? This doesn't make good sense, in both cases the animals are highly over-dosed.

Pg. 7-45, lines 26 and following: The topic of compromised host models is discussed here. I think it would be important to discuss the requirements for a compromised animal model to mimic the human condition. For example, one is that it is relevant for a certain human condition; another is that the model should be based on the outcome of the Epi studies with respect to the compromised organ system; a problem is that it is always an acute compromised animal model *vs*. chronic state in humans. I think such general discussion of animal models would be extremely useful, also with respect to interpretation of results from the models that have been used; for example, the MCT model resulting in extreme severely damaged lung is that representative of a specific human condition?

I assume the studies cited here with the MCT model are the same as the ones that have been discussed before in the Toxicology chapter. As have the studies with the use of SHR rats. Missing in this section are the studies by Elder *et al*. where the LPS inhalation model was used to mimic the early stages of human bacterial respiratory infection.

Pg. 7-48, lines 29 – 31: Studies by Kodavanti *et al*. with SH rats are mentioned here, and it is stated that the SH rats were found to be more susceptible to acute pulmonary injury from ROFA exposure than control Wistar rats. However, the results of the Kodavanti *et al*. (2000b) study do not bear that out, in fact, it is just the opposite, at least on the BAL lavage data: They show that the control Wistar rats have greater pulmonary acute effects than the SH rats (*e.g*., protein increases and PMN increases after inhalation of ROFA are much greater in the control rats).

Pg. 7-50, lines 30/31: The doses given to the cultured neurons should be given, are they relevant to what would be expected under *in vivo* exposure conditions?

Pg. 7-62, line 5: When discussing the *in vitro* methodology in general, it should be added here that doses delivered *in vitro*, like with intratracheal administration, are very high on a cellular basis which makes it very difficult to extrapolate to the *in vivo* condition. Thus, dose–response designs for *in vitro* studies should be mandatory, with the lowest doses going down to environmental doses predicted to occur under *in vivo* conditions at the cellular level.

Pg. 7-63 and following, Table 7-10: The column "Exposure Technique" is not needed because these are all *in vitro* studies, as the title says. However, what is missing is a column saying something about the dose per 10^6 cells; this would be very useful for comparing and interpreting the study results. This is important also for the text to include the dose levels and also state whether effects were only present at the highest level or also at the lower levels which are of greater interest.

Pg. 7-75, lines 1-5: In this brief summary the issue of high doses needs also to be addressed.

Following section on potential cellular and molecular mechanisms: This section is rather long and too detailed, again the issue of high doses should be addressed when specific mechanisms are discussed. It is likely that the dose determines the mechanism for a specific underlying effect.

Pg. 7-81, line 19: Macrophages residing in the airways are first cells to encounter inhaled PM, although one could argue that for the smallest of PM (ultrafine particles) macrophages are not very important and that epithelial cells become more of an issue here.

Pg. 7-82, line 1: It seems that emphasis is placed here on doses of ROFA that are non-cytotoxic. However, it needs to be made clear that non-cytotoxic levels don't mean that the levels are environmentally relevant.

Pg. 7-83 and following: How were the cells dosed? Was it done in a dose–response study design so that the slopes of the response could be compared between different PM materials? Or was only one dose of each material given? Comparing these different particulate compounds is very difficult because of different particle sizes, surfaces, composition, *etc*. Therefore, a brief discussion on how this comparison was made, which principles for comparative PM assessment were used, would be extremely helpful here.

Line 30: The conclusion that gene array technology may provide a tool for screening has to be viewed with caution, because of the issue of dose and also the time-course for gene expression.

Pg. 7-85, line 14: Are ROFA particles small enough to be defined as colloidal?

Line 23: What is the size of the SPM particles used?

Pg. 7-87, line 12: It should be added here that the ultrafine TiO_2 particles always showed a significantly greater response than the fine TiO_2 particles.

Pg. 7-88, line 12: In addition to the larger surface area per given mass of ultrafine particles, their fate after deposition is also different in that they interact more rapidly with epithelial target cells rather than being phagocytized by alveolar macrophages.

Line 29: It could be added here in parenthesis that ozone was given at 1 ppm and ultrafine carbon particles at 100 $\mu g/m^3$. Also, old rats were 20 months old and young rats 10 weeks old.

Pg. 7-89, line 4: As stated above, it could be added here that ultrafine particles are not effectively phagocytized by alveolar macrophages and can easily penetrate the airway epithelium, *etc* .

Pg. 7-90, lines 19-22: This is an important point for ultrafine droplets of acids that they do not persist as particles once deposited. However, organic ultrafine particles may persist longer depending on the organic components. This needs more studies.

Pg. 7-91 and following: All the hypotheses listed and described here together with their "supporting" studies need to be analyzed critically with regard to the doses used on which these hypotheses are based, the models used and the PM used.

Pg. 7-107, ultrafine particles, line 21: The half sentence starting with "Teflon particles …." should be changed to "….PTFE fumes was due to particle size alone or in combination with adsorbed fumes." Also, the next sentence stating that subsequent studies with other types of ultrafine particles etc. is not correct; we already had shown that there is a significantly greater inflammatory effect of ultrafine than of fine TiO2 when they were both of the same chemical composition. Moreover, the example given here in line 23 with a study by Kuschner *et al*. is not a very good one because — as mentioned before — there are significant differences between the *in vivo* solubility for ZnO *vs*. MgO, the ZnO being much more bioavailable because of its high *in vivo* solubility.

Line 27 replace "Teflon" with "PTFE"

Line 30: with respect to analysis of chemical constituents of ultrafine particles, our studies with fine and ultrafine TiO_2 (both anatase TiO_2) confirm nicely that given the same chemistry particle size is the most decisive factor for the ultrafine particles to cause greater effects, and this scales very well with surface area (studies reported in Oberdörster *et al*., 2000, HEI report).

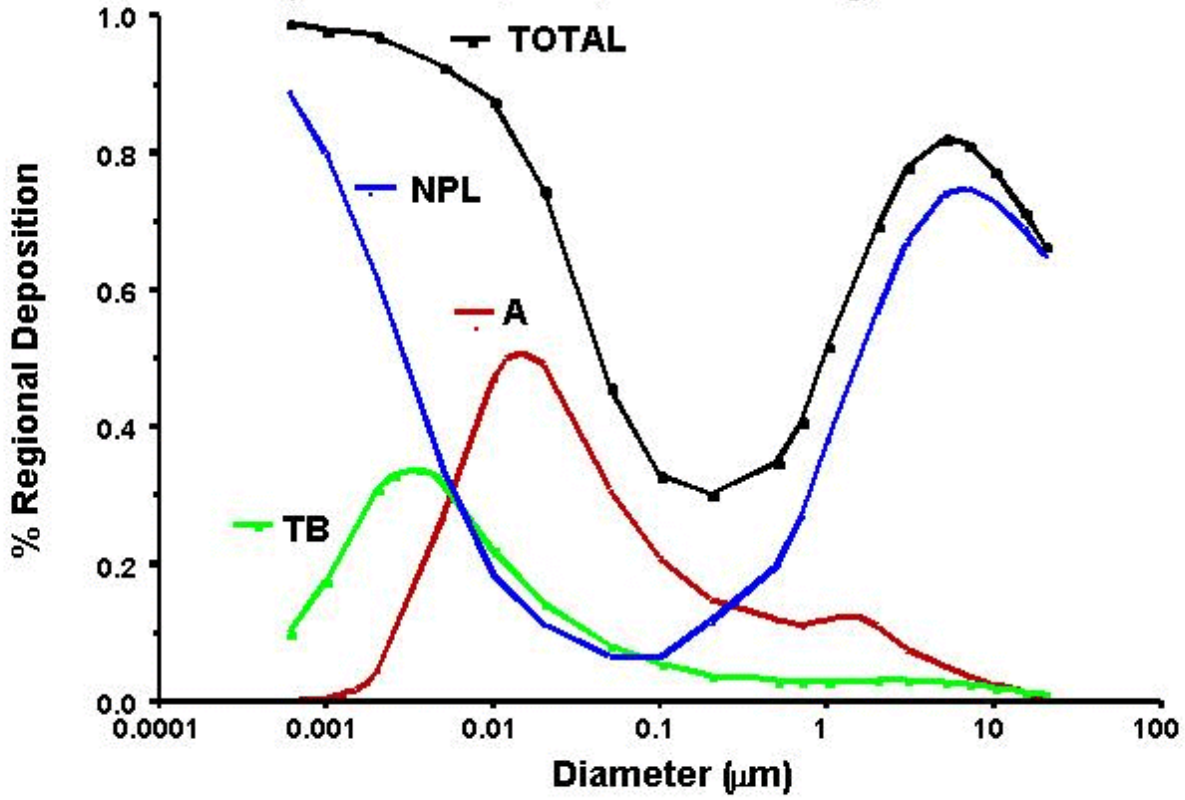
Pg. 7-108, line 4: What is a sulphuric acid ultrafine metal oxide particle?

Pg. 7-110, mechanisms of action: This brief summary is much better than the longer description of hypotheses and mechanisms in the main text, it addresses the issue in a much more critical manner, very good.

Note - the Figure on Page A-119 was supplied by Dr. Oberdörster.

Fractional Deposition of Inhaled Particles in the Human Respiratory Tract

(ICRP Model, 1994; Nose Breathing)



A = Alveolar; TB = Tracheobronchial; NPL = Nasal, Pharyngeal, Laryngeal

Appendix B - Roster and Bio-Sketches of the CASAC Particulate Matter Review Panel

U.S. Environmental Protection Agency
Science Advisory Board
Clean Air Scientific Advisory Committee
CASAC Particulate Matter Review Panel*

CHAIR
Dr. Philip Hopke, Clarkson University, Potsdam, NY
Also Member: Research Strategies Advisory Committee
Executive Committee

CASAC MEMBERS
Dr. Frederick J. Miller, CIIT Centers for Health Research, Research Triangle Park, NC

Mr. Richard L. Poirot, Vermont Agency of Natural Resources, Waterbury, VT

Dr. Frank Speizer, Harvard Medical School, Boston, MA

Dr. George E. Taylor, George Mason University, Fairfax, VA

Dr. Sverre Vedal, National Jewish Medical and Research Center, Denver, CO

Dr. Barbara Zielinska, Desert Research Institute, Reno, NV

CASAC CONSULTANTS
Dr. Jane Q. Koenig, University of Washington, Seattle, WA

Dr. Petros Koutrakis, Harvard University, Boston, MA

Dr. Allan Legge, Biosphere Solutions, Calgary, Alberta, CANADA

Dr. Paul J. Lioy, UMDNJ - Robert Wood Johnson Medical School, Piscataway, NJ

Dr. Morton Lippmann, New York University School of Medicine, Tuxedo, NY

Dr. Joe Mauderly, Lovelace Respiratory Research Institute, Albuquerque, NM

Dr. Roger O. McClellan, Albuquerque, NM

Dr. Gunter Oberdorster, University of Rochester, Rochester, NY

Dr. Robert D. Rowe, Stratus Consulting, Inc., Boulder, CO

Dr. Jonathan M. Samet, Johns Hopkins University, Baltimore, MD

Mr. Ronald White, Silver Spring, MD

Dr. Warren H. White, Washington University, St. Louis, MO

Dr. George T. Wolff, General Motors Corporation, Detroit, MI

SCIENCE ADVISORY BOARD STAFF
Mr. Robert Flaak, Washington, DC

Ms. Zisa Lubarov-Walton, Management Assistant, Washington, DC

*** Members of this SAB Panel consist of**
a. SAB Members: Experts appointed by the Administrator to serve on one of the SAB Standing Committees.
b. SAB Consultants: Experts appointed by the SAB Staff Director to a one-year term to serve on ad hoc Panels formed to address a particular issue.

Clean Air Scientific Advisory Committee (CASAC)
Biosketches

CASAC MEMBERS

Dr. Philip K. Hopke

I am the Bayard D. Clarkson Distinguished Professor, Department of Chemical at Clarkson University where I have been a faculty member since 1989. My area of interest is atmospheric chemistry focusing on particles with an emphasis on the development and application of receptor models, nucleation, sampling and analysis of particle samples. I have been studying these areas for over 25 years and have published extensively on these subjects. I have been involved in exposure assessment and risk assessment through my participation on NRC committees on Advances in Assessing Human Exposure to Airborne Pollutants and on Risk Assessment of Hazardous Air Pollutants. I currently serve of the NRC committees on Research Priorities for Airborne Particulate Matter and on Air Quality Management in the United States. I also serve on the Research Strategies Advisory Committee and the Executive Committee of the EPA Science Advisory Board. I have chaired an SAB Subcommittee on Residual Risk Assessment. I am Chair of the Clean Air Scientific Advisory Committee, and its Subcommittee on Particle Monitoring.

Dr. Richard Poirot

Mr. Richard L. Poirot has worked as an environmental analyst in the Air Quality Planning section of the Vermont Department of Environmental Conservation since 1978. His responsibilities include developing the technical support for State Implementation Plans (SIPs) to ensure attainment and maintenance of Federal and State standards for ozone, particulate matter, and regional haze. Given the rural nature and northeasterly location of Vermont, the influence of regional-scale pollution transport is of particular interest. Lacking sophisticated atmospheric chemistry modeling expertise and resources, Mr. Poirot has also developed interests in drawing inference on the nature of pollution sources from analysis of ambient measurement data, and in working in collaborative regional scientific of science/policy forums. For example, he is or has been a participant on Ambient Monitoring and Assessment Committee for the Northeast States for Coordinated Air Use Management, the Data Analysis workgroup for the Ozone Transport Assessment Group, the Science and Technical Support Workgroup for the FACA Subcommittee on Ozone, Particulate Matter and Regional Haze, the Monitoring and Data Analysis Workgroup for the Mid Atlantic/Northeast Visibility Union (MANE-VU), the EPA PM-2.5 Data Analysis workgroup, the Steering Committee for the Interagency Monitoring of Protected Visual Environments, and the US/Canada (Air Quality Agreement) Subcommittee on Scientific Cooperation.

Mr. Poirot holds a B.A. degree from Dartmouth College, where he majored in geography and environmental studies. In November 2001, he was appointed by the Administrator of the U.S. Environmental Protection Agency (EPA) as a member of the Clean Air Scientific Advisory Committee (CASAC) of EPA's Science Advisory Board.

Dr. Frank Speizer

Dr. Frank E. Speizer is currently Edward H. Kass Professor of Medicine at the Channing Laboratory of the Harvard Medical School, Boston, MA. Since 1988, he has also served as Co-Director of the Channing Laboratory. Dr. Speizer also holds hospital appointments as a senior physician in the Department of Medicine at Brigham and Women's Hospital, Boston; MA and as senior physician in the Department of Medicine at Beth Israel Deaconess Medical Center, Boston. Dr. Speizer received his Bachelor of Arts (A.B.) degree from Stanford University in 1957, and his Doctor of Medicine (M.D.) from the Stanford University Medical School in 1960. He also holds an honorary Master of Arts (A.M.) degree from Harvard University, which was awarded in 1989.

Prior to his current appointment at the Channing Laboratory, Dr. Speizer served as Associate Professor of Epidemiology (Physiology) at the Harvard School of Public Health, Boston (1978-1986), and as Associate Professor of Medicine, Harvard Medical School (1978-1986). Since 1986, he has served as both Professor of Medicine at the Harvard Medical School and as Professor of Environmental Sciences at the Harvard School of Public Health. Dr. Speizer's major committee assignments include serving as: Executive Committee, Dana Farber/Harvard Cancer Center, Boston (1998); Co-Chair, American Thoracic Society Questionnaire Revision Committee (1999); Member, Honors Committee, Harvard Medical School (2002); and Chair, Search Committee for Pulmonary Division Chief, Brigham and Women's Hospital. In October 2001, Dr. Speizer was appointed by the Administrator of the U.S. Environmental Protection Agency (EPA) as a member of the Clean Air Scientific Advisory Committee (CASAC) of EPA's Science Advisory Board. His major professional society involvement includes serving as a Member of the International Society for Infectious Diseases and the American Thoracic Society, National Asthma Research Committee; and as Associate Editor for Environmental Research. Dr. Speizer's awards and honors include: Honorary Fellow, American College of Epidemiology (2000); World Lung Health Award, American Thoracic Society (2000); Member, Institute of Medicine, National Academy of Sciences (2001); Excellence in Women's Health Award, Jacobs Institute of Women's Health (2001); the Charles S. Mott Prize, General Motors Fund for Cancer Research (2001); and the Excellence in Women's Health Award, Brigham and Women's Hospital (2001).

An epidemiologist, Dr. Speizer's major research interests are environmentally- and occupationally-related acute and chronic diseases; the natural history of chronic obstructive lung disease; and epidemiologic studies of risk factors for cancer, heart disease and diabetes. He is extensively published in his disciplinary field of expertise.

Dr. George Taylor

I am a Professor of environmental sciences at George Mason University (GMU) in Fairfax, Virginia and serve as Assistant Dean of the School of Computational Sciences (SCS). The SCS is a graduate education and R&D unit of GMU, specializing in disciplines that are computationally intensive, including space sciences, remote sensing, artificial intelligence, data mining, bioinformatics and genomics, computational modeling (e.g., climate change, air dispersion), and environmental sciences. I provide instruction at the undergraduate (Honor's

Science Program - Astrobiology) and graduate (Ecophysiology of Plants; Biology of Invasive Species; Regional and Global Issues in the Environmental Sciences) levels.

My disciplinary expertise is the physiology and ecology of terrestrial vegetation, with an emphasis on plant stress physiology, atmosphere-canopy interactions, ecotoxicology and ecotoxicogenomics, ecological risk assessment, remote sensing, modeling (whole-plant physiology and growth), and population biology. I have been involved in air pollution research for 25 years, with a focus on ozone, climate change, sulfur oxides, odd-nitrogen species (NOx and HNO3), mercury, and pesticides.

Dr. Sverre Vedal, MD

I am currently Professor of Medicine in the Division of Environmental and Occupational Health Sciences at National Jewish Medical and Research Center in Denver, Colorado. I am a respiratory physician and an epidemiologist. My research focus is primarily on the health effects of air pollution.

I receive active research funding support. I am core leader for an EPA Environmental Lung Center grant to National Jewish that supports clinical studies on air pollution effects and experimental studies investigating the immunological basis for pollutant effects. I am also core leader on an NIH Program Project Grant on berylliosis. I am also the principal investigator of a Canadian-funded research project examining air pollution effects in asthma and COPD. I have published research papers based on previously funded work, as well as review articles on the health effects of particulate matter (PM) and air pollution in general. I have also made presentations, and expressed opinions, on PM health effects at local and international meetings, and for the local news media in Denver.

I serve as a standing member of the Review Committee of the Health Effects Institute (HEI). I was a member of the HEI Expert Panel of the HEI-funded reanalysis of the Six Cities and American Cancer Society pollution cohort studies, and a member of the review committees for the HEI-funded National Mortality, Morbidity and Air Pollution Study (NMMAPS) I and II. I chaired the review committee preparing the commentary on NMMAPS III. I also serve as a member of the National Research Council committee on Air Quality Management in the US. I recently provided an external review for the Southern Appalachian Mountains Initiative (SAMI) on PM impact assessment.

Dr. Barbara Zielinska

I currently hold a position as Research Professor and Director of the Organic Analytical Laboratory at the Division of Atmospheric Sciences of the Desert Research Institute (DRI) in Reno, Nevada. The DRI is an autonomous research division of the University and Community College System of Nevada (UCCSN). DRI was created in 1959 by a special act of the Nevada State Legislature. Under the act and subsequent actions of the University Board of Regents, DRI is charged with conducting basic and applied research in environmental science. The institute employs more than 400 professional, technical, and support staff.

I have been active on air pollution field for over 20 years and I specialize in the analysis of organic compounds in ambient air and in emission sources. My list of publications includes numerous papers concerning analysis of ambient and source samples for polycylic organic hydrocarbons (PAH), nitro-PAH and other toxic air pollutants. I am currently the principal investigator of the following grants and contracts: Section 211(b) tier 2 high-end exposure screening study of baseline and oxygenated gasoline (funded by the American Petroleum Institute); chemical characterization of the exhaust from heavy duty diesel vehicles to evaluate the performance of diesel technology options, including fuel and catalyst (funded by DOE/NREL, SCAQMD and BP/ARCO); chemical characterization of heavy-duty vehicles, tested on chassis dynamometer (Coordinating Research Council); and the characterization of chemical composition and ambient concentrations of particulate and semi-volatile organic compounds for the California Regional PM2.5/PM10 Air Quality Study (CRPAQS). My recently completed research projects include: detailed chemical characterization of diesel and gasoline exhaust for the DOE/NREL comparative toxicity study; apportionment of diesel emissions in underground mines where heavy-duty diesel equipment is used and assessment of miner's exposures to these emissions (funded by the Health Effects Institute); chemical analyses of collected diesel particulate matter samples in the CRC E-43 project (DOE/NREL); and analysis of speciated volatile organic compounds for the 2000 Central California Ozone Study and 1997 Southern California Ozone Study-NARSTO (CARB).

OTHER SAB MEMBERS

Dr. Paul Lioy:

I currently hold a position as Professor of Environmental and Community Medicine, UMDNJ-Robert Wood Johnson Medical School (1985) and Associate Director of the Environmental and Occupational Health Sciences Institute. I specialize in Environmental Exposure and Health. In particular, I direct field studies on single or multiple mediums of exposure, and direct a Division that includes mathematical modeling, controlled human studies, and laboratory and field studies, on environmental toxicants in air, water, and soil.

I have been active on air pollution exposure and health effects for over 25 years. I have published research about different air pollutants that are part of the 812 Analytical Blueprint.

SAB CONSULTANTS

Dr. Jane Q Koenig

Professor in the Department of Environmental Health, University of Washington. To the best of my understanding I do not have any financial conflicts of interest regarding air pollution research. However I do conduct research in the area of the health effects of air pollution and I am currently the director of one of the EPA Research Centers for PM health effects research. I also receive funding for other air pollution related research. I have attached my NIH Other Support page to this report. My community service includes testimony for various citizen groups regarding the health effects of PM and other pollutants. I am on the Advisory Council of the Puget Sound Clean Air Agency and on the science Advisory Board of the National Urban Air Toxics Research Center.

Dr. Petros Koutrakis

Petros Koutrakis is a Professor of Environmental Sciences at the Harvard university School of Public Health. I am an environmental chemist and I have conducted many research projects on human exposure assessment, development of sample and analysis techniques for atmospheric pollutants, development of techniques to expose subjects to concentrated ambient particles, indoor air quality and atmospheric chemistry.

I am the PI of the EPA/Harvard Center on particle health effects, I am the Co-PI on two NIEHS Program projects (Cardiac effects of air pollution). In addition, I am the PI of exposure assessment and air quality studies funded by EPA, HEI, EPRI, API, and DOE.

Dr. Allan H. Legge

I am currently the President of Biosphere Solutions, an environmental consulting company located in Calgary, Alberta, Canada. Prior to forming Biosphere Solutions in 1993 I was a Senior Research Scientist in the Kananaskis Centre for Environmental Research,The University of Calgary from 1972 to 1990 and a Senior Research Officer with the Alberta Research Council from 1990 to 1993. My area of specialization is the evaluation and assessment of the effects of air pollutants such as SO_2,H_2S,NOx,HF, particulate matter (PM) and saline aerosols on forest and agricultural ecosystems. I have been active in this area of research for over 27 years and have published extensively.

I do not receive any research grant funding from any agency of the US Government. I am currently co PI of a research contract from Alberta Environment investigating the environmental consequences to a forest ecosytem of long-term exposure to chronic low concentrations of SO_2 coupled with intermittent exposures to fine and coarse particle elemental sulphur.

Dr. Morton Lippmann

I am a Professor of Environmental Medicine at the New York University School of Medicine, where I have been a faculty member since 1967. My research on particulate matter and its health effects has included air sampling method development, particle deposition and clearance in the respiratory tract, the effects of inhaled aerosols on pulmonary funtions in laboratory animals and human volunteers, and the associations between ambient aerosol exposures and mortality and morbidity in human populations. Sponsors of this research have included NIOSH, NIEHS, EPA, HEI, and EPRI. I have served on all of the prior CASAC PM panels as either a consultant, member, or chair. In various published papers, I have described and discussed the role of CASAC in reviewing criteria document and staff paper drafts and the nature and appropriateness of NAAQS for the prevention of adverse health effects. I do not believe that I have any conflicts of interest in regard to my participation in the current PM document reviews.

Dr. Joe L. Mauderly, DVM

I am Vice President and a Senior Scientist of the Lovelace Respiratory Research Institute (LRRI), an independent nonprofit research organization in Albuquerque, NM. LRRI conducts research for government (including EPA) and industry sponsors on the causes, mechanisms, treatment, and prevention of respiratory disorders, and has had a longstanding emphasis on

inhaled particles and mixed atmospheres containing particles. My personal research has focused for over 30 years on the effects of inhaled particles on respiratory function and structure, the influence of age and other factors on individual susceptibility, the utility of various animal models for such research, and the relative contributions of particles and other air contaminants to the cancer and non-cancer effects of combined exposures. Much of my work has focused on engine emissions.

I am Director of one of LRRI's programs, the National Environmental Respiratory Center (NERC). NERC is funded by 20 government and industry sponsors, including EPA, to conduct research leading to a better understanding of the contributions of individual air contaminants to the adverse health effects of complex mixtures of air pollutants. I am also principal investigator of a DOE-funded program to understand the health hazards of engine emissions, with emphasis on the impacts of changes in diesel fuel, engine, and after-treatment technologies. From time-to-time, I participate in numerous other projects as an advisor or co-investigator, including some funded wholly or in part by EPA.

Dr. Roger O. McClellan, DVM

For the past two years I have served as an independent advisor on toxicology and human health risk analysis issues to public and private organizations. From 1988 to 1999 I served as President and CEO of the Chemical Industry Institute of Toxicology in Research Triangle Park, NC. The CIIT is a non-profit organization supported principally by the chemical industry with a research program directed toward developing a better understanding of the health risks of exposure to chemicals. During my tenure the Institute's research program on the toxicity of airborne materials was substantially expanded. From 1966 to 1988 I provide leadership for the inhalation toxicology operations of the Lovelace Medical organization (now the Lovelace Respiratory Research Institute)in Albuquerque, NM. This non-profit organization was supported largely by government funds and conducted an extensive , multi-disciplinary research program on the toxicity of airborne materials.

My advisory practice involves service on advisory committees and boards and provision of independent advice to federal agencies, government laboratories, universities, public corporations and law firms. The primary issues addressed have been the design, conduct and interpretation of toxicological and epidemiological studies with a broad range of substances including vehicle and power plant emissions, pharmaceutical agents, and consumer products. The end goal is to assess human health risks.

Dr. Gunter Oberdörster -

I hold the position of Professor of Toxicology at the Department of Environmental Medicine, University of Rochester and of Director of the EPA-funded PM Center at Rochester. My area of expertise is inhalation toxicology of non-fibrous and fibrous particles. Specifically, I perform inhalation studies in rodents using laboratory-generated particulate compounds.

My professional activities at the University of Rochester are supported by grants/contracts from EPA, NIEHS, International Carbon Black Association, and DuPont. This research is

related to effects and mechanisms of inhaled particles in the lung and extrapulmonary organs. A DOD grant is pending on the pulmonary toxicology of ultrafine particles.

Dr. Robert Rowe

I am an economist with, and President of, Stratus Consulting Inc. For 25 years I have conducted research on the human health and welfare impacts of air pollution, including PM, and the economic valuation of these impacts (with particular focus on human health and visibility). This work has included studies measuring the benefits and costs of alternative regulatory strategies on the national and state level. My work has been primarily funded by U.S. and Canadian federal and state/provincial agencies, but also has been funded by energy suppliers and institutes (including work on the air pollution externalities of electricity production and of transportation). The primary focus of my non-air work is assessing damages for Natural Resource Damage Assessment, generally for public trustees. I have 15 years service on EPA Science Advisory Board committees dealing with air pollution standards.

Dr. Jonathan M. Samet, M.D., M.S.

I am currently Professor and Chairman in the Department of Epidemiology of the Johns Hopkins University Bloomberg School of Public Health, a position that I have held since 1994. I am also Co-Director of the Risk Sciences and Public Policy Institute and Director of the Institute for Global Tobacco Control. My research is in the general area of environmental epidemiology, much of it emphasizing the health effects of air pollution, a research topic of interest for me for two decades.

With regard to particulate matter, since 1994, with money from the Health Effects Institute, my colleagues and I have using time-series methods to address the effect of particulate matter and other air pollutants on mortality. Our work, particularly reports from the National Morbidity, Mortality, and Air Pollution Study (NMMAPS), is cited by the EPA in its Criteria Document and draft Staff Paper.

I have written widely on the health effects of air pollution. In various articles over the years, I have summarized the evidence and provided judgments as to the findings. I also chair the National Research Council's Committee on Research Priorities for Particulate Matter. This Committee has now issues three reports on the nation's research agenda for particulate matter.

Mr. Ronald H. White, M.S.T.

I am currently employed as the Assistant Executive Director for Education, Research and Community Affairs at the National Osteoporosis Foundation (NOF). I also serve as a non-paid volunteer for the American Lung Association, where I was previously employed as Assistant Vice President for National Policy. I currently serve as a member of the National Research Council Committee on Research Priorities for Airborne Particulate Matter, as well as on the External Scientific Advisory Committee of the National Environmental Respiratory Center.

Neither myself, the NOF nor the ALA receives research funding from the USEPA. The ALA currently has a cooperative agreement with the USEPA that supports education and outreach programs on the issue of indoor air quality and public health.

The ALA has been an active advocate for the establishment of national air quality standards generally, and particulate matter standards specifically, that are fully protective of public health. Consistent with this effort, the ALA has submitted comments to CASAC on earlier drafts of the Particulate Matter Criteria Document, and has publicly expressed support for adoption of particulate matter health standards that are fully protective of public health.

Dr. Fred J. Miller

I currently serve as the Interim Vice President for Research at the CIIT Centers for Heath Research (CIIT) in Research Triangle Park, North Carolina. I also hold the position of Head of the Division of Quantitative Biology and Physical Sciences at CIIT. In addition, I am a member of the regular faculty in the Department of Medicine at Duke University Medical Center where I hold the title of Research Professor in Medicine. At CIIT, I manage interdisciplinary teams of scientists working on research and risk assessment issues related to respiratory tract dosimetry and toxicology of inhaled gases and particles. The major portion of funding for research at CIIT comes from industrial member companies and from the Long-range Research Initiative of the American Chemistry Council.

I am interested in developing and implementing research strategies and projects that will permit increased utilization of animal toxicological results to evaluate the likelihood of human risk from exposure to inhaled chemicals. My research interests include pulmonary toxicology, respiratory tract dosimetry of gases and particles, lung physiology and anatomy, extrapolation modeling, and risk assessment. CIIT scientists have and are continuing to conduct research that is relevant to the setting of ambient standards and workplace exposure limits for particles. Some examples of the types of particulate research that CIIT does are: (1) subchronic exposures of rats, mice, and hamsters to pigmentary and ultrafine titanium dioxide to address the basis of the unique response of rats to concentrations that overload macrophage-mediated clearance of particles, (2) studies to develop mouse models of pulmonary inflammation involving either allergy or viral infection for assessing responses to inhaled particles, (3) studies on the pharmacokinetics and neurotoxicity of various forms of inhaled manganese, (4) developing respiratory tract dosimetry models for the deposition and clearance of particles in animals and humans, and (5) conducting experimental dosimetry studies in laboratory animals.

Dr. George T. Wolff -

I am presently the Principal Scientist for General Motors Public Policy Center. It is my responsibility to provide accurate and relevant scientific assessments on air quality and climate change issues, which serve as input to GM's public policy positions and decisions. GM is the sole funding source for this work.

I have been an atmospheric scientist for almost 30 years and have published over 100 papers and reports in the scientific literature on a wide variety of air quality issues including PM.

In my capacity as a member of the CASAC PM Panel, I do not represent General Motors. I represent myself as an independent and objective air quality scientist. However, GM does have a vested interest in the PM review, as they would like to see environmental regulations based on sound science.

In my capacity as former Chair of CASAC, I have had numerous occasions to speak and write about the previous PM review to scientific groups, trade groups, environmental organizations, legislative bodies, and other governmental entities. In all cases, my message was similar. I summarized the review process and the conclusions reached by the previous Panel which are contained in the Closure letter sent to the Administrator on June 13, 1996.

NOTICE

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