UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
WASHINGTON D.C. 20460

OFFICE OF THE ADMINISTRATOR
SCIENCE ADVISORY BOARD

June 18, 2014

EPA-CASAC-14-003

The Honorable Gina McCarthy
Administrator
U.S. Environmental Protection Agency
1200 Pennsylvania Avenue, N.W.
Washington, D.C. 20460

Subject: CASAC Review of the EPA's *Welfare Risk and Exposure Assessment for Ozone (Second External Review Draft)*

Dear Administrator McCarthy:

The Clean Air Scientific Advisory Committee (CASAC) Ozone Review Panel met on March 25 - 27, 2014, to review the EPA's *Welfare Risk and Exposure Assessment for Ozone (Second External Review Draft)*, hereinafter referred to as the Second Draft WREA. The Chartered CASAC met by teleconference on May 28, 2014 to deliberate on the Panel's findings and recommendations. Enclosed are the Chartered CASAC's consensus responses to the agency's charge questions and individual comments from the CASAC Ozone Review Panel. The main points are highlighted below.

The CASAC finds that the Second Draft WREA largely achieves its ambitious goal of characterizing ambient ozone exposure and its relationship to ecological effects and estimating the resulting impacts to ecosystem services. The Second Draft WREA offers scientifically defensible estimates of the impact of ozone exposure on tree biomass loss, crop yields and visible foliar injury. We support the connections laid out in the Second Draft WREA between these effects and adverse impacts on public welfare. The estimates of exposures that correspond to predicted levels of tree biomass loss, crop loss and visible foliar injury using the W126 index (a sigmoidally weighted seasonal sum of hourly ozone concentrations) found in the Second Draft WREA are derived with scientifically defensible data, models and assumptions. The CASAC finds that the exposure-response relationships presented in the Second Draft WREA are appropriate to be used for considering potential alternative secondary standards.

The Second Draft WREA also estimates the impact of ozone exposure on a broad array of related ecosystem services important to society. The CASAC supports the EPA's ecosystem services approach and finds that the analyses at the national scale and case study scale provide scientifically defensible estimates of effects on these services. Included in these estimates are the effects of biomass loss on timber production, food production, carbon sequestration, and the effects of visible foliar injury on aesthetic values and outdoor recreation. The CASAC finds that these estimates represent the current state

of the science and are therefore appropriate for decision making. When quantitative estimates are not possible, the Second Draft WREA offers scientifically defensible qualitative assessments of the magnitude and significance of ozone-related effects on ecosystem services such as net primary productivity, community composition, hydrological cycles, pollination and recreational services.

In all of its analyses, the Second Draft WREA applies the W126 index of cumulative ozone exposure. The CASAC previously supported the use of this metric and reaffirms here its biological relevance and superiority to the current metric for assessing the welfare impacts of ozone.

Consistent with CASAC recommendations, the Second Draft WREA uses a more comprehensive approach to its air quality modeling including the higher order direct decoupled method, HDDM. The HDDM enables development of internally consistent, policy-relevant air quality scenarios upon which the exposure and risk assessment can be based. This represents a significant improvement over the approach used in the First Draft WREA and offers consistency with the air quality modeling in the Second Draft Health Risk and Exposure Assessment (HREA).

The Second Draft WREA correctly recognizes the complexity of ecosystem responses to ozone and the availability of exposure-response functions for a relatively limited but important set of tree species. The Second Draft WREA presents a qualitative assessment of uncertainty that appropriately identifies key sources of uncertainty and their potential effects on risk estimates. The discussion of uncertainty, while very thorough, is so extensive as to inappropriately imply low confidence in what is known and what can be inferred with an acceptable degree of confidence. Recommendations for consolidating the discussion of uncertainty and emphasizing the strength of the evidence are detailed in the attached responses to charge questions. Also, the Second Draft WREA in some analyses inappropriately assumes that ozone has no effect on species for which an exposures-response function is not known. Therefore the resulting risk estimates likely underestimate ozone-related biomass losses in trees and crops.

Some revisions are needed to clearly distinguish between potential magnitudes of loss from ozone exposures and evidence of ozone-induced loss that has already occurred. Overlap in the spatial distribution of ozone exposures with the spatial distributions of bark beetle infestations and of fire risks is not sufficient to justify a causal inference although, as acknowledged in the Second Draft WREA, expert opinion relates both bark beetle infestations and fire risks to ozone.

The currently reported finding of only small differences in risk between just meeting the current standard and a W126-based level of 15 ppm-hrs must not be interpreted to mean that just meeting the current standard will be as protective as meeting a W126-based standard at 15 ppm-hrs. There are two key factors that must be considered when making this comparison. First, air quality was simulated in the Second Draft WREA based on the magnitude of across-the-board reductions in NO_x emissions required to bring the highest monitor down to the target level. Meeting a target level at the highest monitor requires substantial reductions below the targeted level through the rest of the region. This artificial simulation does not represent an actual control strategy and may conflate differences in control strategies required to meet different standards and different targets. As a result, there may be a number of monitors that meet the current standard but would not meet an alternative W126 standard. Second, and equally important, the current form of the standard is much less biologically relevant for protecting vegetation than is a seasonal, peak weighted index such as the W126, which was designed to measure the *cumulative* effects of ozone exposure.

In the consensus responses to charge questions, the CASAC offers some suggestions for improvements to the Second Draft WREA but does not find that any new analyses are needed for this review. Overall, the CASAC finds the Second Draft WREA to be scientifically sound and appropriate for informing the analyses of important adverse impacts on public welfare and consideration of potential alternative secondary standards in the Policy Assessment.

Sincerely,

/signed/

Dr. H. Christopher Frey, Chair
Clean Air Scientific Advisory Committee

Enclosures

NOTICE

This report has been written as part of the activities of the EPA's Clean Air Scientific Advisory Committee (CASAC), a federal advisory committee independently chartered to provide extramural scientific information and advice to the Administrator and other officials of the EPA. The CASAC provides balanced, expert assessment of scientific matters related to issues and problems facing the agency. This report has not been reviewed for approval by the agency and, hence, the contents of this report do not necessarily represent the views and policies of the EPA, nor of other agencies within the Executive Branch of the federal government. In addition, any mention of trade names or commercial products does not constitute a recommendation for use. The CASAC reports are posted on the EPA website at: http://www.epa.gov/casac.

U.S. Environmental Protection Agency
Clean Air Scientific Advisory Committee (CASAC)

CHAIR

Dr. H. Christopher Frey, Distinguished University Professor, Department of Civil, Construction and Environmental Engineering, College of Engineering, North Carolina State University, Raleigh, NC and Visiting Professor, Department of Civil and Environmental Engineering, Adjunct Professor, Division of Environment, Hong Kong University of Science and Technology

MEMBERS

Mr. George A. Allen, Senior Scientist, Northeast States for Coordinated Air Use Management (NESCAUM), Boston, MA

Dr. Ana Diez-Roux, Dean, School of Public Health, Drexel University, Philadelphia, PA

Dr. Jack Harkema, Professor, Department of Pathobiology, College of Veterinary Medicine, Michigan State University, East Lansing, MI

Dr. Helen Suh, Interim Chair, Director of Population Health Doctoral Program, Department of Health Sciences, Northeastern University, Boston, MA

Dr. Kathleen Weathers, Senior Scientist, Cary Institute of Ecosystem Studies, Millbrook, NY

Dr. Ronald Wyzga, Technical Executive, Air Quality Health and Risk, Electric Power Research Institute, Palo Alto, CA

SCIENCE ADVISORY BOARD STAFF

Mr. Aaron Yeow, Designated Federal Officer, U.S. Environmental Protection Agency, Washington, DC

U.S. Environmental Protection Agency
Clean Air Scientific Advisory Committee
Ozone Review Panel

CHAIR

Dr. H. Christopher Frey, Distinguished University Professor, Department of Civil, Construction and Environmental Engineering, College of Engineering, North Carolina State University, Raleigh, NC and Visiting Professor, Department of Civil and Environmental Engineering, Adjunct Professor, Division of Environment, Hong Kong University of Science and Technology

MEMBERS

Mr. George A. Allen, Senior Scientist, Northeast States for Coordinated Air Use Management (NESCAUM), Boston, MA

Dr. Ana Diez-Roux, Dean, School of Public Health, Drexel University, Philadelphia, PA

Dr. Jack Harkema, Professor, Department of Pathobiology, College of Veterinary Medicine, Michigan State University, East Lansing, MI

Dr. Helen Suh, Interim Chair, Director of Population Health Doctoral Program, Department of Health Sciences, Northeastern University, Boston, MA

Dr. Kathleen Weathers, Senior Scientist, Cary Institute of Ecosystem Studies, Millbrook, NY

Dr. Ronald Wyzga, Technical Executive, Air Quality Health and Risk, Electric Power Research Institute, Palo Alto, CA

CONSULTANTS

Mr. Ed Avol, Professor, Preventive Medicine, Keck School of Medicine, University of Southern California, Los Angeles, CA

Dr. Michelle Bell, Professor, School of Forestry and Environmental Studies, Yale University, New Haven, CT

Dr. Joseph D. Brain, Cecil K. and Philip Drinker Professor of Environmental Physiology, Department of Environmental Health, Harvard School of Public Health, Harvard University, Boston, MA

Dr. David Chock, Independent Consultant, Bloomfield Hills, MI

Dr. David A. Grantz, Department of Botany and Plant Sciences, University of California at Riverside, Kearney Agricultural Center, Parlier, CA

Dr. Daniel Jacob, Professor, Atmospheric Sciences, School of Engineering and Applied Sciences, Harvard University, Cambridge, MA

Dr. Steven Kleeberger, Professor and Lab Chief, Laboratory of Respiratory Biology, National Institute of Environmental Health Sciences, National Institutes of Health, Research Triangle Park, NC

Dr. Frederick J. Miller, Independent Consultant, Cary, NC

Dr. Howard Neufeld, Professor, Department of Biology, Appalachian State University, Boone, NC

Dr. Armistead (Ted) Russell, Professor, Department of Civil and Environmental Engineering, Georgia Institute of Technology, Atlanta, GA

Dr. James Ultman, Professor, Chemical Engineering, Bioengineering Program, Pennsylvania State University, University Park, PA

Dr. Sverre Vedal, Professor, Department of Environmental and Occupational Health Sciences, School of Public Health, University of Washington, Seattle, WA

Dr. Peter Woodbury, Senior Research Associate, Department of Crop and Soil Sciences, College of Agriculture and Life Sciences, Cornell University, Ithaca, NY, U.S.A.

SCIENCE ADVISORY BOARD STAFF

Dr. Holly Stallworth, Designated Federal Officer, U.S. Environmental Protection Agency, Washington, DC

CASAC Consensus Responses to Charge Questions on
EPA's Welfare Risk and Exposure Assessment for Ozone (Second External Review Draft)

Chapter 1: Introduction

Question 1. To what extent does the Panel find the introductory and background material, including that pertaining to previous reviews of the O_3 standards and the current review, to be clearly communicated and appropriately characterized?

Overall, the introductory and background material are appropriate and clearly communicated. The goals are explicitly laid out in easy-to-understand terms and the organization of the document is succinctly explained.

Because the W126 index is so important, a figure showing its weighting scheme should be included either in the introduction, or located in Chapter 4 and referenced in the introduction. This figure would show the ozone concentration on the independent axis, and the weight on the dependent axis.

It is important to clarify that "natural" volatile organic compound (VOC) emissions include anthropogenic emissions due to human management of agricultural and forest ecosystems, including the type of vegetation, fire regimes, and so on. This issue is important because the portion of "natural" VOC emissions caused by human activities is potentially controllable with changes in land management practices.

Chapter 2: Conceptual Model

Question 2. *To what extent does the Panel find that the discussions accurately and clearly reflect the air quality, ecosystem effects evidence, ecosystem services, and exposure and risk considerations relevant for quantitative assessment, building from information contained in the final ISA?*

Chapter 2 provides a good summary of the key points that will be used to develop the risk models. The chemistry behind ozone formation and its persistence in the environment is explained briefly, but thoroughly. The ecological effects of ozone are well summarized and the rationales for which metrics are evaluated are placed into context by citing relevant literature.

Chapter 3: Scope

Question 3. To what extent does the Panel find the scope of the welfare risk and exposure assessment is clearly communicated?

Chapter 3 is useful and provides an appropriate level of detail. A clearer explanation of the roll-back methodologies would be helpful, including clarifying the methods, perhaps referring to a more detailed appendix. The clear definitions of uncertainty and variability help greatly in comprehending analyses presented throughout this document.

Chapter 4: Air Quality Considerations

Question 4. What are the views of the Panel on the appropriateness of the methods used to characterize O_3 air quality for the exposure and risk assessment? What are the views of the Panel on the HDDM-based adjustment methodology used to adjust O_3 concentrations to just meet the existing O_3 standard and levels for average W126 scenarios, coupled with the interpolation method used to create a national surface of W126 concentrations for all scenarios?

Overall, the methods used to characterize ozone air quality are appropriate and a significant advancement over previous REAs. The use of the higher order direct decoupled method (HDDM) in combination with air quality modeling to develop internally consistent scenarios for air quality based on specified alternatives to the current standard is a major step forward. The methods used are appropriate, though it is apparent that the approach may evolve as the EPA and others gain experience with the methods used.

The use of Voronoi Neighbor Averaging (VNA) as an interpolation approach is appropriate, although results reported in the appendix suggest that the Downscaler (DS) method performs similarly. The VNA method is used in the WREA and the DS method is used in the HREA. EPA should consider using the same interpolation approach in both the WREA and HREA, or should provide an explanation for the different choices made.

Question 5. To what extent does the Panel find that the discussion of uncertainty related to the air quality inputs to the exposure and risk assessment appropriately includes important sources of uncertainty?

The discussion of uncertainty is beneficial, though qualitative. Quantitative uncertainty analysis of the air quality results should be conducted in future assessments. Each of the uncertainty estimates (magnitude and direction) should be consistent with the uncertainty estimates in the HREA.

Chapter 5: Ozone Risk to Ecosystem Services

Question 6. To what extent does the Panel find the assessment, interpretation, and presentation of the methods and results of the updated ecosystem services assessment to be technically sound, appropriately balanced, and clearly communicated?

The central effort in this chapter is incorporation of qualitative and quantitative variables into an assessment of risks to welfare due to ozone. There is sufficient detail with regard to the methodologies employed. The EPA assesses an acceptable set of ecosystem services. There is an appropriate balance of quantitative and qualitative analyses. As such, this chapter is useful for informing the analyses and proposed standard setting in the Policy Assessment (PA).

Figure 5-2 nicely sets up relationships among supporting, regulating, cultural and provisional ecosystem services. The EPA appropriately recognizes at the outset the complexity of ecosystem responses to ozone and the relative paucity of available data. In light of these two factors, EPA is quite thorough in its description of assessment methodologies. For ecosystem services, there is an *a priori* assumption

that if the total value of an ecosystem service is small, then the effect of ozone exposure will also likely be small. In contrast if the total value of an ecosystem service is large, the impacts could be quite significant, even if the ozone effects are relatively small. This is a justifiable and appropriate framework when extrapolating to a national scale assessment.

The effort to monetize welfare effects is appropriate, even though the relevant techniques are still in development. It is important to incorporate willingness to pay into the risk assessments. The chapter could be improved by further exploring this topic.

The main limitations in this chapter are the confounding of *potential* magnitudes of loss with the *evidence* of ozone-induced loss. These should be clearly distinguished. Distinguishing potential from evidential loss would clarify and strengthen the risk analyses. As written, the loss due to ozone is stated to be contained in the current value of the services, which is true but uninformative, as no evidence for such loss is presented. Therefore, this particular discussion does not contribute to the risk assessment. This issue is considered further in response to Charge Question 8 below.

Question 7. To what extent does the Panel support the revised structure of the ecosystem services discussions, including integrating ecological effects analyses directly with the ecosystem services assessments?

The CASAC commends the EPA for revising the structure of the ecosystem services discussions based on the CASAC's prior advice. In the specific case of the hydrologic cycle, effects are not yet well understood. Net effects of ozone-induced stomatal closure and ozone-induced sluggish stomatal closure, both of which have been demonstrated, may be site- or species-specific. Section 5.3.1 may overstate the case for ozone impacts, especially when extrapolated to the national scale. A shorter and more focused section on potential impacts and supporting data could elaborate upon what is already stated at Line 22, page 5-5, i.e., that ozone impacts on the hydrologic cycle are likely though specific directions and magnitudes may be more difficult to generalize. The follow-up discussion (lines 23-29, page 5-5) showing that the public places a very high value on protecting water quality supports the importance of the risk assessment concerning potential ozone impacts to watersheds. Such a high level of public concern provides strong evidence of the adversity of effects on hydrology and water quality to human welfare.

Question 8. To what extent is the combination of O3 exposure data with other data sources (e.g., fire data, bark beetle maps, trail maps) to link areas of concern/interest with areas of higher vegetation risk due to O3 technically sound?

The Second Draft WREA should be careful to note that in each of these examples posed in the charge question, the overlap in spatial distributions is only correlation. Inferences of causality associated with these correlations are based on very limited evidence. For example, currently only very limited data and expert opinion link ozone with fire and bark beetles. Furthermore, an inference of causality could be confounded by factors such as drought, high temperatures, and human population encroachment; thus, the simple overlaying of Geographic Information System (GIS) layers should not be presented in such a way as to imply causation. However, correlation is meaningful if there is reasonable evidence of a causal linkage.

Greater emphasis should be given to distinguishing between (1) quantification of the areas of spatial overlap and the value of potential loss, and (2) independently assessing potential adverse effects that

may occur from exposure to ozone, using all available quantitative and qualitative metrics.

Question 9. To what extent does the Panel find that the discussion of uncertainty and variability has included all important sources of uncertainty and variability and appropriately characterized their relationship to the ecosystem services estimates?

Some of the language in this chapter pertaining to uncertainty and variability should be modified for clarity. For example, on Page 5-20, Section 5.7 and throughout the discussion, we suggest changing "not possible" and "not quantifiable" to "not currently feasible" to provide explicit acknowledgement of the current limits of available information and resources. This approach better reflects that more could be done in future analyses were additional time and resources to be made available.

The discussion of uncertainty is very thorough and most of the discussion is appropriately consolidated into section 5.6 near the end of the chapter. The EPA is particularly explicit in evaluating its ability and inability to estimate risks for various ecosystem services (see page 5-20). This provides a measure of objectivity to the analyses.

The CASAC concludes that there is a sufficient degree of scientific confidence in the data and models presented in the WREA to enable assessment of adverse impacts and comparisons of potential alternative standards. The Second Draft WREA overstates the uncertainty by excessive repetition, to the extent that it inappropriately erodes the value of the risk assessments themselves. Therefore, the discussion of uncertainty and variation must be revised in the final WREA to more accurately reflect the balance between known impacts and their current uncertainty.

Chapter 6: Biomass Loss

Question 10. To what extent does the Panel find the assessment, interpretation, and presentation of the methods and results of the biomass loss risk assessment to be technically sound, appropriately balanced, and clearly communicated?

The assessment, interpretation and presentation of the biomass loss risk assessment is scientifically sound and clearly communicated. The continued emphasis on Class I areas is appropriate, particularly with the thorough description of the rationale. The continued emphasis on exposure-response (E-R) data,[1] the carefully documented reconciliation of open-top chamber (OTC) studies and other types of data, and the quantitative treatment of response curves including the 2 parameter Weibull function, are appropriate and scientifically justified. The National Crop Loss Assessment Network (NCLAN) crop yield E-R studies and National Health and Environmental Effects Laboratory (NHEERL) tree seedling biomass studies remain the most comprehensive set of such data available anywhere in the world, and are central to the assessment of ecosystem risk.

It is appropriate to use regions rather than a single national result for crop yield. EPA should also summarize results by county, based on results shown in Appendix A (Figures A-31, A-32, and A-37). For example, the number of counties in which yield loss is predicted to exceed a threshold should be

[1] With transition to the W126 metric, the phrase "Exposure-Response (E-R)" should be used in place of "Concentration-Response (C-R)" found in the Second Draft WREA.

presented for current and alternative standards, as is done at the national level in the current draft.

The CASAC is concerned about potential bias in the air quality simulations of across-the-board regional reductions in NOx to attain various W126 levels. Because of the sigmoidal weighting of this metric, an unrealistic across-the-board regional reduction will drive some areas with low concentrations to very low levels, disproportionately affecting the impact of alternative W126-based standards. Thus the finding of only small differences in risk between just meeting the current standards and applying any of the alternate W126-based standards should not be interpreted to mean that just meeting the current standard will be as protective as would a W126-based standard, for two reasons. First, the "across-the-board" NOx reduction is not intended to represent an actual or optimal control strategy, and thus should not be interpreted as such. There may be a number of monitors that meet the current standard but do not meet an alternate annual W126 standard. The number of such monitors should be calculated for recent years. Second, the current form of the standard is much less biologically relevant for protecting vegetation than would be a seasonal, peak weighted standard such as the proposed W126-based approach. This third point is consistent with previous recommendations by the CASAC.

The comparison of seedling and adult tree Relative Biomass Loss (RBL) (Table 6-3) is useful. At moderate values of W126 the seedling systems appear to approximate adult tree responses, but the substantial divergence at higher W126 suggests that either the systems are very different for some species, or that the models are not well parameterized. The discussion should not overstate the similarities. Effects on seedlings are considered strong evidence of impact, particularly since the effects will compound over the life of the tree.

A reference is required for the statement for aspen that "OTC studies found very consistent biomass loss between seedling and adult trees." There may be confusion about whether this is OTC or FACE data, which should be clarified in the text, as should the age of the adult trees.

This chapter states that loblolly pine seedlings are insensitive to ozone, though in Chapter 5 (page 5-16, line 1-2) foliar injury is described. Thus a reference is required for the loblolly pine E-R data on which the statement is based. In addition, EPA should consider an additional study (Shafer and Heagle, 1989) which found family differences in ozone sensitivity, with losses up to 13% in loblolly pine after three seasons of exposure.

There is a discrepancy in the magnitude of firewood utilization. Values cited in individual comments for the state of New York are greater than those cited in Table 6-13 for the entire nation.

The relative changes evaluated in Table 6-4 might be easier to comprehend if they are evaluated with respect to circumference squared or cubed, which would more closely approximate changes in biomass. A potential caveat is that changes in water content of trees would also manifest as changes in circumference.

The cottonwood response data, from a natural urban gradient with minimal OTC support, are given too much emphasis (e.g., Figure 6-2, page 6-5). These data are from a single study using a gradient methodology without experimental control of ozone exposure. They require further confirmation before they should be invoked heavily in the ozone risk assessment.

In Section 6.6.2, the i-tree model is appropriately applied to trees with known E-R functions. However, other species for which E-R functions are not available appear to be assigned zero sensitivity to ozone.

This results in a low bias that should be acknowledged and discussed. Future analyses should avoid the default assumption of no effect in species for which E-R functions are lacking.

The discussion in Section 6.10 is similar to the discussion section in Chapter 5, but has been stripped down to a bulleted list. A more thorough discussion would be useful, attempting to integrate the highly varied topics into a few conclusions.

Question 11. To what extent does the Panel find the carbon sequestration estimates from the Forest and Agricultural Sector Optimization Model Greenhouse Gas version (FASOMGHG) (Section 6.6.1) to be technically sound and appropriately characterized?

The carbon sequestration estimates from FASOMGHG are technically sound and appropriately characterized. The comparisons made with automobiles removed from circulation (e.g., at line 16-18, page 6-47) are particularly effective. The use of median parameter values for the E-R functions is an appropriate approach and its associated uncertainty may be overstated. The CASAC is concerned that the use of a median value for the E-R function applied to a population of mixed sensitive and tolerant species may represent a bias toward underestimation of impact. This will depend on the distribution of sensitivity among individual species. For example, late successional species may be less sensitive to ozone due to slower growth and lower rates of gas exchange (Harkov and Brennan, 1982) than pioneer species. This issue would benefit from greater exploration in the text.

Annual crop systems are appropriately discounted because of the periodic tilling of the soil and brief life cycles.

Question 12. To what extent does the Panel find the weighted biomass loss analysis in Section 6.8 to be a technically sound approach to assess potential ecosystem-level effects nationwide and in Class I areas?

The CASAC agrees that weighted biomass loss is an appropriate means of examining regional impacts, though it may fail to protect the most sensitive species. The decision to focus on a 2% loss level for trees and to use this to analyze compounding effects throughout the lifespan of a tree is appropriate. Simple calculations of compounding effects indicate substantial losses in productivity over very few years, given only a 2% biomass loss per year. Thus, 2% relative biomass loss per year is an appropriate criterion for adverse effect.

The reported calculations related to biomass loss require clarification. Greater clarity is needed in describing the following issues: (1) potential biases related to the small number of species for which E-R functions are known, and the role in the risk analysis of species with unknown E-R relationships; (2) effects of competition between sensitive and tolerant trees in a community; and (3) the potential impact of the relative number of late- and early-successional species included in the analyses, since these species may differ in their sensitivity to ozone.

Additional treatment on an individual species basis, as in Figures 6-11 and 6-12, would be useful although some reviewers found these figures difficult to interpret.

Question 13. To what extent does the Panel find that the discussion of uncertainty and variability has included all important sources of uncertainty and variability and appropriately characterized their relationship to biomass loss estimates?

There is sufficient evaluation of uncertainty, though for clarity and brevity this can be consolidated into a single section near the end of the chapter. There should be more concern about systematic biases than imprecision. These result from attempts to generalize or to simplify calculations. Impacts on the analyses can be discussed where appropriate.

Chapter 7: Foliar Injury

Question 14. To what extent does the Panel find the assessment, interpretation, and presentation of the methods and results of the foliar injury risk assessment to be technically sound, appropriately balanced, and clearly communicated?

The foliar injury risk assessment is scientifically sound and appropriately balanced. For better communication, it would be helpful to explain more clearly the concept and meaning of the biosite index, as is done in the Executive Summary. It is appropriate to use willingness-to-pay (WTP) to help quantify aesthetic impacts. However, the enumeration of these monetary values does not directly quantify the risk due to ozone; it is more of a potential risk. The paragraph at page 7-9, lines 25-28 represents a clear example of such potential risk and the inability to identify risk due to ozone is clearly stated. However, the role of potential risk in the WREA is not clear, since the loss due to ozone may be zero in such cases. The concept that losses due to ozone are embedded in the current value of the commodity is also unclear. EPA should provide more clarity of these concepts and better establish their relevance to the WREA. For example, there may be qualitative factors to consider with respect to possible "anticipated" effects.

Question 15. What are the views of the Panel on the analysis of the Forest Health Monitoring data in Section 7.2, including the finding of the lack of a statistical relationship between the severity of foliar injury and W126 index values or soil moisture levels?

This charge question is poorly phrased. A preponderance of no effect data does not invalidate the relationship over ranges where injury is observed, as shown by the significant results found using censored regression (Table 7-5). Because most readers are not familiar with censored regression, more explanation should be provided in the text, either in this chapter or in an appendix. Also, more details of the analysis should be provided including the model tested and the meaning of marginal effect. The remainder of this chapter and all other summary sections should also be corrected to state that there is a significant relationship between ozone exposure and foliar injury (e.g., page 7-72, line 5-10).

Figures 7-9 and 7-11, showing the cumulative number of biosites with any injury as a function of W126, are very clear and effective in communicating the risk due to ozone. This analysis also reveals a change in the E-R slope near 10 ppm-hrs. However, this slope change is not a threshold for no injury. Based on this E-R slope change, 10 ppm-hrs is a reasonable candidate level for consideration in the WREA, along with other levels.

Question 16. What are the views of the panel on the appropriateness of the characterization of vegetation strata (i.e., herb, shrub, tree) for the analyses of sensitive species cover in the three national park case studies (Section 7.4)?

These strata seem appropriate but in some parks, such as Great Smoky Mountains National Park, many herbs are found beneath a dense, closed canopy of trees. Published studies show that their exposure to O_3 will be much less than plants out in the open or above the canopy. Thus, their sensitivity to O_3 will appear less than what one would assume based on the readings from stationary monitors, all of which are located in open areas or above the canopy. Even so, there can still be large and significant effects of ozone on understory herbs (e.g., Souza, et. al., 2006). Staff notes that there are understory foliar injury data in the national assessment but that only tree data were included in the analysis. Analysis of these understory data would strengthen the risk assessment.

Question 17. What are the views of the Panel on the usefulness of the screening-level assessment of visible foliar injury in national parks in Section 7.3? Specifically, what are the views of the Panel regarding conclusions appropriate to draw from applying the W126 benchmark scenarios derived from the national-scale Forest Health Monitoring data analysis in the screening-level assessment?

As suggested by the CASAC previously, it is appropriate to focus on the W126 and appropriate not to include other metrics used by Kohut (2007). It is also appropriate to use updated drought and ozone data. The application of VNA introduces some uncertainty, as does any spatial interpolation method, but is an efficient way to extend the analysis to parks without monitors, as was done previously by Kohut using other statistical techniques. The benchmark explanation section (7.3.1.4) is particularly difficult to read and comprehend, and should be clarified. The concept of consistent percentage of biosites (page 7-26, line 5) is not very clear, and requires further explanation. Several figures in this section are not informative and should be replaced; one or two highly relevant figures would be preferable, with the rest relegated to the appendices or simply described in the text.

Question 18. To what extent does the Panel find that the discussion of uncertainty and variability have covered important sources of uncertainty and variability and appropriately characterized their relationship to foliar injury risks?

Uncertainty is well described, and should be consolidated for brevity. The tabular summary (Table 7-23) is a good format to summarize the discussion. However, the summary (page 7-72) promulgates the confusion mentioned for Charge Question 15, i.e., that there is no relationship between foliar injury and either O_3 or soil moisture when in fact there is.

Chapter 8: Synthesis

Question 19. To what extent does the Panel find the synthesis to be a useful integration and summarization of key results and insights regarding the overall welfare exposure and risk analyses?

Chapter 8 is currently more of a summary than a synthesis. A shorter, more focused chapter is needed.

The repetition of previously presented graphs and methodology is not as useful as the synthesis inherent in these tables. Sections 8.3 and 8.4 are closer to the necessary synthesis. Table 8-1 is excellent and could be combined with Table 8-2, indicating that all the data can be interpreted together, as the basis

for discussion in this chapter. The needed synthesis should suggest an answer to the question, “If an 8-hr primary standard of x ppb were adopted, at what level would a W126 standard have to be set to provide additional benefit, and how much benefit would be derived?”

The chapter ends with the important observation that the difference between “just meets 75 ppb” and “just meets W126 of 15 ppm hrs” is important. Greater analysis of how other 8-hr standards match with various levels of W126 would be instructive. The revised synthesis should include evaluation of potential ranges of W126 for requisite protection against the array of adverse effects of ozone exposure considered in the WREA.

Too much is made of uncertainties in the chapter. Formal and informal confidence boundaries should be estimated, but employed to bound the conclusions rather than to discredit them. Responses to methodological concerns from previous chapters should be carried forward into this synthesis.

Executive Summary

Question 20. To what extent does the Panel find the Executive Summary to be a useful summary of the data and methods used to estimate exposures and risks to ecosystems and the key results of the assessment?

The Executive Summary provides some of the discussion and summary that would have been helpful in Chapter 8. Comments on procedures within the WREA should be carried forward into the Executive Summary. The conflation of overlapping spatial area with causation, noted in numerous places in the WREA, is inappropriately perpetuated in the Executive Summary. Questions related to use of median parameters for E-R functions will impact conclusions here. The use of national estimates of consumer and producer impacts of crop loss will underestimate losses to growers in heavily ozone impacted areas.

Overall, this draft Executive Summary makes an important contribution to the PA. It is well written and adequately summarizes the results from the WREA. The physical layout is easy to read and the language is straightforward to follow.

References

Harkov, R.S. and Brennan, E. 1982. An ecophysiological analysis of the response of woody and herbaceous plants to oxidant injury. *Journal of Environmental Management* 15:251-261.

Kohut, R. 2007. Assessing the risk of foliar injury from ozone on vegetation in parks in the US National Park Service's Vital Signs network. *Environmental Pollution* 149:348-357.

Shafer, S.R. and Heagle, A.S. 1989. Growth responses of field-grown loblolly pine to chronic doses of ozone during multiple growing seasons. *Canadian Journal of Forest Research* 19:821-831.

Souza, L., H.S. Neufeld, A.H. Chappelka, K.O. Burkey and A.W. Davison. 2006. Seasonal development of ozone-induced foliar injury on tall milkweed in Great Smoky Mountains National Park. *Environmental Pollution* 141:175-183.

Individual Comments by CASAC Ozone Review Panel Members on EPA's Welfare Risk and Exposure Assessment (Second Draft)

Individual Comments: George Allen

These comments focus on Chapter 4 of the Welfare REA.

General Comments.

Overall chapter 4 (Air Quality Considerations) is substantially improved over the first draft WREA and generally is responsive to CASAC comments on that draft. The implementation of the HDDM rollback method is well done with appropriate modifications to adapt it from the 12 urban focus regions to the nine NOAA climate regions. The use of NOx only emission reductions (no VOC reduction scenarios) is appropriate for rural areas which are almost always NOx limited unless there is a large NOx source nearby. Table 4-1 (page 4-14) is especially useful in understanding the relationship between the secondary W126 metric and the primary design values under different W126 rollback targets.

Chapter 4 Charge Questions (Air Quality Considerations).

4. What are the views of the Panel on the appropriateness of the methods used to characterize O3 air quality for the (welfare) exposure and risk assessment? What are the views of the Panel on the HDDM-based adjustment methodology used to adjust O3 concentrations to just meet the existing O3 standard and levels for average W126 scenarios, coupled with the interpolation method used to create a national surface of W126 concentrations for all scenarios?

The methods used to characterize O3 air quality in the nine NOAA climate regions are appropriate and clearly explained. The use of the HDDM rollback method appears to work well for just meeting the existing (same as primary) standard and the three scenarios for a W126 secondary standard (15, 11, and 7 ppm-h). The national surface maps showing adjustments to different NAAQS scenarios are very helpful in understanding the spatial dynamics of the adjustments. The VNA interpolation method for national surface of W126 values for different scenarios appears to be a reasonable choice. Figures 4-7 through 4-14 are very helpful in evaluating these scenarios.

#5. To what extent does the Panel find that the discussion of uncertainty related to the air quality inputs to the exposure and risk assessment appropriately includes important sources of uncertainty?

As with the health REA, the WREA clearly summarizes the qualitative uncertainty for key elements in this risk assessment. Table 4-2 (pages 4-27 to 4-31) clearly describes each of these uncertainties along with the direction and magnitude of them.

Individual Comments: David Grantz

Chapter 5: O3 Risk to Ecosystem Services

6. To what extent does the Panel find the assessment, interpretation, and presentation of the methods and results of the updated ecosystem services assessment to be technically sound, appropriately balanced, and clearly communicated?

The central effort in this chapter is incorporation of qualitative variables into assessment of risk due to ozone. Due to the complexity of ecosystems and the relative paucity of data compared with human health, it is critical that these factors receive the level of consideration provided. Figure 5-2 sets up these relationships nicely.

The limitation in the chapter is confounding of potential magnitudes of loss with evidence of ozone induced loss. The two concepts can and should be clearly distinguished. Examples abound (page 5-6, line 5; elsewhere in the WREA and in the PA). The loss due to ozone is stated to be contained in the current value of the services (true but uninformative), and as there may be no loss due to ozone, and no evidence for such loss is presented, the discussion does not seem to contribute to risk assessment. This is considered further in response to Question 8 below.

The effort to monetize welfare effects is appropriate, though techniques are still in development. It is important to incorporate willingness to pay into this risk assessment.

The reference (page 5-3, line 27) to a 13% per year decline in NPP over 45 years may require restatement for clarity or accuracy. As written, this would imply a substantial compounded decline to exceedingly low levels of current productivity.

The statement at page 5-20, line 20, that decreased biomass leads to decreased NPP seems backward to me. If biomass loss were mostly leaf material, this could be the case, but biomass is mostly stem and trunk material.

7. To what extent does the Panel support the revised structure of the ecosystem services discussions, including integrating ecological effects analyses directly with the ecosystem services assessments?

This changed structure was implemented in response to previous CASAC review, and I continue to support it.

In the case of the hydrologic cycle, effects are not yet well understood. Section 5.3.1 may overstate the case, and reads too much like a search for deleterious consequences rather than a true analysis. As presented, if runoff increases it is bad for various reasons, but if runoff decreases, then it is bad for other reasons. Logically, something should be made better if it does not increase, and other things if it does not decrease. The argument here is that any change is bad, which is not defensible. A shorter and more focused section on potential impacts and supporting data could elaborate upon what is already stated (Line 22, page 5-5).

8. To what extent is the combination of O3 exposure data with other data sources (e.g. fire data, bark beetle maps, trail maps) to link areas of concern/interest with areas of higher vegetation risk due to O3 technically sound?

In each of these examples, the similar spatial distribution says nothing about causation by ozone. There is confounding by drought, high temperatures, human population encroachment, etc., that make the simple overlaying of GIS layers not persuasive. A potentially better approach might be to use the overlap to identify an area which can be quantified, use the size or value of the resource within that area to state the potential harm that may be threatened by ozone, then use other measures of risk (including quantitative elements from Chapter 6) to evaluate the risk to this resource due to ozone. Only very limited data and expert opinion link ozone with fire and bark beetles. These should be marshalled to support statements such as that at page 5-8, line 14. Suggestion that the documented spatial overlap implies causation is not defensible. Similarly, equating current value of a resource with a reflection of undemonstrated loss due to ozone (e.g. page 5-6, line 5; also in the PA) should be revisited.

9. To what extent does the Panel find that the discussion of uncertainty and variability has included all important sources of uncertainty and variability and appropriately characterized their relationship to the ecosystem services estimates?

The discussion of uncertainty is certainly complete enough, and repeated often enough. However, I suggest that the repetition throughout the text be consolidated in the appropriate section 5.6, near the end of the Chapter. In this, as in other chapters of the WREA and PA, uncertainty is considered so often that the impact of the information is eroded.

Chapter 6: Biomass Loss

10. To what extent does the Panel find the assessment, interpretation, and presentation of the methods and results of the biomass loss risk assessment to be technically sound, appropriately balanced, and clearly communicated?

The continued emphasis on Class I areas is appropriate, particularly with the excellent description of the rationale (Page 6-2, lines 4-9). The continued emphasis on exposure response data, the carefully documented reconciliation of OTC and other types of data, and the quantitative treatment of response curves including the 2 parameter Weibull function, are appropriate. The NCLAN and NHEERL studies remain the most comprehensive set of such data available anywhere in the world, and are central to the evaluation of ecosystem risk.

The cottonwood response data, from a natural urban gradient with minimal OTC support, is given perhaps too much attention (e.g. Figure 6-2, page 6-5). These data are clear outliers and require further confirmation before they should be invoked heavily in the ozone risk assessment. This does not suggest they are not accurate, but that they are distinct enough to require special care to confirm them.

The comparison of seedling and adult tree RBL (Table 6-3) is very useful. While this indicates that at moderate values of W126 the seedling systems approximate adult tree response, the substantial

divergence at higher W126 suggests that either the systems are very different or that the models are not yet sufficiently well parameterized. Either way, the text in all of Section 6.2.1.1 should be evaluated for accuracy and for clarity. As written it is difficult to follow and the main conclusion is lost. The comparison in Table 6-4 might be easier to comprehend if relative changes were evaluated in terms of circumference squared or even cubed, which would more closely approximate changes in biomass, which is proportional to volume. A potential caveat is that changes in water content of trees would also manifest as changes in circumference.

It should be stated that RBL is on an annual basis (if it is). In Table 6-6 and elsewhere, it should be stated that RBL is in percent rather than fraction, for clarity. I found Figures 6-11 and 6-12 very hard to interpret. The column headings in Table 6-21 are not self explanatory. Is NOA simply current ozone? What is ES/15?

The Discussion (Section 6.10) is superficially similar to the more appropriate Discussion in Chapter 5, but has been stripped down to a bulleted list. A more complex discussion would be useful, attempting to integrate the highly varied topics into a few conclusions.

11. *To what extent does the Panel find the carbon sequestration estimates from the Forest and Agricultural Sector Optimization Model Greenhouse Gas version (FASOMGHG) (Section 6.6.1) to be technically sound and appropriately characterized?*

The comparisons made (e.g. at line 16-18, page 6-47) are very appropriate. Agricultural systems are appropriately discounted because of the working of the soil and brief life cycles. The use of median parameter values for the C-R functions is a strong approach and its role in uncertainty may be overstated.

12. *To what extent does the Panel find the weighted biomass loss analysis in Section 6.8 to be a technically sound approach to assess potential ecosystem-level effects nationwide and in Class I areas?*

The weighted biomass loss is an appropriate means of examining regional impacts. It must be recognized that this metric may lead to failure to protect the most sensitive species in a region.

13. *To what extent does the Panel find that the discussion of uncertainty and variability has included all important sources of uncertainty and variability and appropriately characterized their relationship to biomass loss estimates?*

As noted for Chapter 5, above, there is sufficient evaluation of uncertainty. It can be consolidated into a single section near the end of the chapter, for clarity and brevity.

Chapter 7: Foliar Injury

14. *To what extent does the Panel find the assessment, interpretation, and presentation of the methods and results of the foliar injury risk assessment to be technically sound, appropriately balanced, and clearly communicated?*

It is important and appropriate that staff have related foliar injury to aesthetic value. While CASAC noted in oral discussion of the first External Review Draft that many visitors to or observers of eastern deciduous forests may not notice or object to the enhanced coloration of ozone damaged foliage, this may be less true in areas of coniferous forest (e.g. in the Sierra Nevada and San Bernardino Mountains) where a suite of factors have left large areas of brown foliage and dead trees. In these cases the least discerning observer will likely find the view degraded as noted in the text (page 7-6, line 11-17).

Willingness to Pay is a crude index but is becoming more nuanced. It is appropriate to use WTP to attempt to value aesthetic impacts. However, as noted above, the enumeration of these monetary values does not in itself address the risk due to ozone. It is at most a potential risk. Much of the discussion of valuation could be consolidated in an introductory section, with the likelihood of risk due to ozone evaluated in an uncluttered manner later in the chapter. The paragraph at page 7-9, lines 28-28 represents a clear example of this, with the inability to identify risk due to ozone clearly stated. It is unclear what the text contributes to the evaluation of ozone risk, since the loss due to ozone may be zero and this is not excluded in this paragraph. The concept that losses due to ozone are embedded in the current value of the commodity is also unclear, as noted above.

The Discussion (Section 7.6) is too much of a bulleted list and not enough synthesis. As in the previous chapters, it might be better to rename this a summary, and leave it as a bulleted list, then have a real discussion in Chapter 8. Otherwise, more actual discussion of how it all fits together should replace the current Section 7.6.

15. *What are the views of the Panel on the analysis of the Forest Health Monitoring data in Section 7.2, including the finding of the lack of a statistical relationship between the severity of foliar injury and W126 index values or soil moisture levels?*

This question (15) is poorly phrased. Once the censored regression was run (Table 7-5) the expected relationships between foliar injury and both ozone and drought became significant. Because of the lack of familiarity of most readers with censored regression, more explanation should be provided in the text. Staff notes that the statistics are more complex than for simple regression, but more information including the model tested, and the meaning of marginal effect should be provided. This should also be amended in Section 7.6 (page 7-72, line 5-10) where the lack of relationship is again stated, incorrectly. A preponderance of no effect data do not invalidate the relationship over ranges where injury is observed.

Figures 7-9 and 7-11, showing cumulative number of biosites with any injury as a function of W126, are very clear and effective in communicating the risk due to ozone. This treatment also reveals a clean break point near 10 ppm hr (though not a threshold for no injury). This is one of the few objective indicators of potential levels of the new standard and should be carried through the risk analysis. In contrast, Figures 7-10 and 7-12 are based on an arbitrary level of injury, reveal a less clear distribution, and are less informative.

Figure 7-6 is confusing, it appears that the small numbers in the figure would be Palmer Z indices, but they rather appear to be subunits of each state with no particular relevance to interpretation of the data. If the numbers can be removed while leaving the sub-regional boundaries this would improve the clarity of the figure.

16. *What are the views of the panel on the appropriateness of the characterization of vegetation strata (i.e., herb, shrub, tree) for the analyses of sensitive species cover in the three national park case studies (Section 7.4)?*

It is appropriate to consider stratified vegetation. Understory vegetation will be protected from ambient ozone by reduced circulation and by deposition to overlying canopy, thus disrupting E-R relationships based on above-canopy monitoring. Staff notes that there are understory foliar injury data in the national assessment but that only tree data were included in the analysis. Analysis of these understory data would strengthen the risk assessment.

17. *What are the views of the Panel on the usefulness of the screening-level assessment of visible foliar injury in national parks in Section 7.3? Specifically, what are the views of the Panel regarding conclusions appropriate to draw from applying the W126 benchmark scenarios derived from the national-scale Forest Health Monitoring data analysis in the screening-level assessment?*

As suggested by CASAC previously, it is appropriate to focus on the W126 to the exclusion of the other metrics explored by Kohut. It is also appropriate to use updated drought and ozone data. The application of VNA certainly introduces some uncertainty, but seems like a very efficient way to extend the analysis to parks without monitors, as was done previously by Kohut using other statistical techniques.

The concept of consistent percentage of biosites (page 7-26, line 5) is not very clear, and requires further explanation. Were no other levels consistent at some value of W126? Text should describe how this level was identified? It seems that Figure 7-16 for all years (lower right) should serve to make this obvious, but does not. Table 7-6 is only slightly further explanatory. From Figure 7-16 it seems unclear how any level of injury or of W126 could be independent of Palmer Z, as all panels except 2006 appear to have a meaningful negative slope.

This section has too many poorly informative figures. Much of it shows geographic differences but these are not discussed nor analyzed in the text. One or two highly relevant figures would be preferable, with the rest relegated to the Appendices or simply described in the text.

18. *To what extent does the Panel find that the discussion of uncertainty and variability have covered important sources of uncertainty and variability and appropriately characterized their relationship to foliar injury risks?*

Uncertainty is again very well described, and again might be consolidated for brevity.

Chapter 8: Synthesis

19. *To what extent does the Panel find the synthesis to be a useful integration and summarization of key results and insights regarding the overall welfare exposure and risk analyses?*

Table 8-1 is excellent and should be the sole basis for discussion in this chapter. I would prefer if Table 8-2 were combined with 8-1, indicating that all the data can be interpreted together to evaluate risk due to ozone. The review and repetition of previously presented graphs and methodology is not as useful as the synthesis inherent in these tables. What is really needed in this chapter is a true synthesis of "Risk

due to Ozone", taking the whole document into account.

Throughout the document too much is made of uncertainties. They are real, but are just methodological imperfections. All studies have them. It is unfortunate to read (page 8-30, line 25-26) that "limitations and uncertainties…may have a large impact on…confidence.." I do not see how the analyses could have been done very much better. Therefore, this is a state of the art treatment, and while the formal and informal confidence boundaries should be estimated, they should not be used to discredit the conclusions.

Executive Summary

20. To what extent does the Panel find the Executive Summary to be a useful summary of the data and methods

The ES is very appropriately and accurately written. This provides the discussion and summary that should be in Chapter 8. For the first time here we encounter the conclusion that a level of the cumulative standard near 10 ppm hr is indicated by the data. We find a conveniently located definition of 'biosite'. However, the conflation of overlapping spatial areas with causation is perpetuated in the ES, which should be reconsidered for appropriateness.

Individual Comments: Daniel J. Jacob
Comments on second draft of Welfare REA for ozone

Chapter 4: Air Quality Considerations

4. What are the views of the Panel on the appropriateness of the methods used to characterize O3 air quality for the exposure and risk assessment? What are the views of the Panel on the HDDM-based adjustment methodology used to adjust O3 concentrations to just meet the existing O3 standard and levels for average W126 scenarios, coupled with the interpolation method used to create a national surface of W126 concentrations for all scenarios?

Overall I think that the method is appropriate. The reliance on monitoring data for interpolation is a big improvement over the previous draft that fused CMAQ results. The document references chapter 4 of the Health REA for details of the HDDM implementation and I have made some comments there that I won't repeat here.

4.1 Page 4-11: I have some concern over the partitioning of the US into just 9 regions for reducing emissions. The justification based on climatic coherence of these regions doesn't make much sense to me. The partitioning should ideally be done on the scale over which the secondary standard is to be managed, and the corresponding regions would likely be much smaller (county level?). Using coarse regions biases the results by requiring larger reductions than would be needed if smaller regions were used.

4.2 Page 4-12: the sole focus on NOx emission reductions presumes that meeting the secondary standard will not be limited by urban areas. Is that assumption valid? That's not clear to me.

4.3 In Figures 4-10, 4-12, 4-14, white presumably means zero or negative differences (say so in caption). But I'm confused. Why are there blue areas in regions for which the 75 ppb standard allows to meet the W-126 standard, like the Northwest in 4-10?

4.4 Page 4-6, line 3: "most species are not photochemically active during nighttime hours". None would be.

5. To what extent does the Panel find that the discussion of uncertainty related to the air quality inputs to the exposure and risk assessment appropriately includes important sources of uncertainty?

5.1 I don't see the point of this qualitative uncertainty analysis. Without quantitative uncertainty estimates one cannot propagate errors to the REA, which should be the whole point.

5.2 One missing factor of uncertainty that needs some discussion is the ability to quantify the sensitivity of ozone to emission reductions through CMAQ. If I recall, the first draft showed large CMAQ errors in simulating ozone in the Intermountain West. In that region at least, I strongly doubt that CMAQ

sensitivities are correct.

Individual Comments: Howard S. Neufeld

Chapter 1: Introduction

1. The introduction does a good job of summarizing the intent and history involved with developing the Welfare Risk Assessment. The goals are explicitly laid out in easy to understand terms and the organization the subsequent document is succinctly explained.

Chapter 2: Conceptual Model

2. The authors do a good job of summarizing the key points that will eventually be used to develop the risk models. The chemistry behind ozone formation and its persistence in the environment is briefly but thoroughly explained to the reader. The summaries of the ecological effects of ozone are well explained and the rationales for which metrics are evaluated are placed into context by citing relevant organizations and literature.

Chapter 3: Scope

3. This chapter is very thorough, but I think the authors could better explain the roll-back methodologies used. For the reader not familiar with such techniques, these are difficult concepts to grasp. The discussion of both the 8-hr and W126 standards and how altering one affects the other is well done. Section 3.2.1.3, though, is more difficult to comprehend. When the authors state that they are simulating just meeting various alternative standards, it is not entirely clear how that process works. Perhaps they could refer to an appendix that takes the reader through a demonstration of this process for illustrative purposes.

The relative biomass loss procedures for both trees and crops is very reasonable and takes into consideration the comments and suggestions from the CASAC. I am quite satisfied with the analyses done in this second assessment. In section 3.2.3.4, staff states that one of the metrics evaluated was the percent of trails affected by foliar injury. Is this the percent of trail length with foliar injury, or just the percent of trails where injury was reported? Perhaps they could clarify this (later on, I thought this was explained better in Chapter 7).

I appreciate staff explicitly defining what is meant by uncertainty and variability. That greatly helps in comprehending the analyses done throughout this REA.

Chapter 4: Air Quality Considerations

4. The air quality methods seem appropriate for characterizing welfare risk. Although I don't have experience with the HDDM methodology, it does appear to be the best way to model air quality distributions. Going over the 2012 publication on using HDDM (which was commissioned by the EPA for this purpose) helped me understand this protocol (http://www.epa.gov/ttn/naaqs/standards/ozone/data/20120814Model_Based_Rollback.pdf). It might help with comprehension if a short primer or explanation of HDDM methodology is included in the appendices. Likewise, the interpolation method (Voronoi Neighbor Averaging) also appears most

reasonable. However, this generates a question with respect to Figure 4-5, and that concerns the abrupt line of change in the surface estimate along the TN – NC border, wherein the concentration is estimated to be much lower in NC than TN. Is that real, or is it an artifact of the interpolation methodology?

5. The description of the uncertainties for air quality data, and how they are treated, is covered very thoroughly and in depth. As well, staff has identified most if not all of the important sources of uncertainty. Table 4.2 clearly outlines the status of uncertainty associated with various methodologies.

Chapter 5: O_3 Risk to Ecosystem Services

6. The analyses of ecosystem services are well done, thorough, and clearly stated. I did find Figure 5.7 somewhat confusing. The two panels, one showing the existing standard and the other various W126 scenarios, appear to me identical. Is that how it should be? I also found Tables 5.3 and 5.4 difficult to interpret. I think they either should be revised or have a clearer legend associated with it.

The embedding of potential losses due to ozone within a measure of ecosystem service without having concrete measures of those losses (and which may or may not exist) makes any risk justifications problematic. This section would be stronger if known magnitudes of loss were elaborated separately from potential losses.

Also, on page 5-13, section 5.4, there is the statement that where there is high O_3, there is more bark beetle attack, but no mention is made of possible spurious correlations with temperature. In addition, there are few if any causative studies linking these two, so more caution may be required here.

7. While integrating ecological effects with service assessments might be viewed positively, service assessments are highly dependent on public opinion and can change with time and for reasons not always easily predicted. On the other hand, ecological effects are more deterministic and predictable, and so for developing risk assessments, it might be more prudent to keep these separate. However, I do see the value of an overall integrated assessment, so I can go with the current organization for now.

8. Although the data on the influence of O_3 on fire susceptibility and bark beetle attack are correlational in nature, there is strong evidence from numerous field studies to suggest causality and that O_3 does indeed pre-dispose these trees to increased severity of fire and beetle attack. However, as noted earlier, there are essentially no peer-review studies that link these factors causally, so caution should be emphasized here. Furthermore, there other possible confounding factors at play here, so perhaps this entire section should be revisited.

9. The discussion of the sources and consequences of the uncertainty and variability associated with ecosystem services is comprehensive and appropriately characterized. Improvements in clarity of message could possibly be obtained by elimination of redundancies when discussing uncertainties.

Below are some technical comments for improving Chapter 5:

Figure 5.2 is particularly well done and establishes the framework for the rest of the chapter.

Figures 5-3 and 5-7 are difficult to interpret, since all panels in each figure appear exactly similar. If there are differences, then perhaps these figures need to be reconfigured to make the differences more explicit. One solution would be to plot the initial panel, and then make the other panels "difference" graphs from the first one. Then, small changes in geography will show up more clearly." Also, for

many figure legends, EPA needs to sub-script the "3" in O_3.

Page 5-4, Table 5-1 - add a column for "*not important*" instead of the footnote and then the totals will presumably sum to 100%.

Page 5-4, line 19 to page 5-5 line 3. What is the linkage between fish and ozone impacts on ecosystems and habitats? Is there information on willingness to pay for plant species, for which there is substantial evidence of effects? There needs to be a better linkage in this section with a clear conceptual model of how ozone affects an endpoint, and how much people value that endpoint, or changes in that endpoint (such as population size of a valued species). For fish, some such information is presented in the subsequent section, so perhaps material could be rearranged to support conceptual models.

Page 5-5, line 12 – change "has" to "have" since it refers to the plural "studies"

Page 5-13, line 17 – Perhaps clarify that the bark-beetle-induced foliar damage is not included in Chapter 7.

Page 5-15, Table 5-5 – as before for Table 5-1, add a column for "*not important*" instead of the footnote and then the totals will presumably sum to 100%.

Chapter 6: Biomass Loss

10. The explanation of the use of the Weibull function to characterize biomass loss from C-R studies is well done and more than adequately justifies its use for this purpose. The graphs (Figures 6.2 and 6.3) require units on the X-axis. For Table 6.4 – were the diameter changes from McLaughlin et al. (2007) true losses in growth or simply shrinkage from water stress? In Table 6.5, it states that loblolly pine seedlings are relatively insensitive, but see: *Shafer, S.R. and Heagle, A.S. 1989. Growth responses of field-grown loblolly pine to chronic doses of ozone during multiple growing seasons. CJFR 19:821-831.* These researchers found family differences in ozone sensitivity and that ambient ozone caused losses up to 13% after three seasons of exposure. So calling this species insensitive seems inappropriate. Where were loblolly pine C-R data obtained from? With respect to Figures 6.4 and 6.5, might I suggest using the same Y-axis scale for comparative purposes? Also, should W126 in these graphs be on the X-axis, as it is the independent variable? It's a little difficult figuring out how to interpret Figures 6.13 and 6.14. And there are no units for either axis. Table 6.12: it should be *Quercus rubra*, not "*rubrum*", and there is no genus or species name for hickory.

Perhaps less emphasis should be placed on the cottonwood study. The response of this one species, from this one particular study, needs confirmation by additional studies before you can give it much emphasis in the risk analysis. Certainly, these data are unusual, but they stand out considerably from the other studies.

11. I am not as familiar with this model, but the justification given seems adequate to use for estimating greenhouse gas sequestration by trees and crops. The use of the median parameters though, for the C-R functions is a wise choice.

12. This weighting technique seems the most appropriate avenue to go down with respect to estimating impacts of ozone at the ecosystem-level, and I supported this effort in the last version of the REA, and support it again here. I also support the decision to focus on the 2% loss rate for trees and to use this to

analyze for compounding effects throughout the lifespan of a tree.

13. As in previous sections of the REA, the treatment of uncertainty and variability is well done and appropriate.

Chapter 7: Foliar Injury

14. I have no substantive comments here other than to confirm that the analyses and presentation were well done. However, it would help everyone reading this to further explain the concept of biosite index and what it means (as done in the Executive Summary, for example). It takes several readings to fully understand this and how it is used to analyze foliar injury responses to O_3 and soil moisture.

15. It is confusing to state that with regard to the USFS data that no relationship exists between foliar injury and either O_3 or soil moisture, and then on the next page (7-15) state that there are significant relationships once a censored regression analysis is performed. The initial lack of a significant relationship, as staff notes, results from the overwhelming number of sites with no reported injury. Staff then performs a censored regression (perhaps this statistical technique should be more clearly explained in an appendix) and do find relationships between foliar injury and both O_3 and soil moisture. This section should be re-worded to reflect the nuances of these statistical techniques and the inability of traditional analyses to find statistical relationships when data contain an inordinate number of zero values. This type of data distribution (lots of zeroes, only a few instances of measurable responses) is quite common for foliar injury analyses, and so some sort of standardized analysis technique should be adopted for such data and for future analyses.

The conclusions reached on page 7-17 are the most important in this section and show that foliar injury reports are sensitive to low amounts of ozone, but the response becomes saturated at higher W126 indice values. Thus, the way this charge question is worded is misleading. There *are* relationships between foliar injury and the W126 index.

16. The use of vegetation strata (herbs, shrubs, trees) is appropriate, but one should note that in some parks, especially Great Smoky Mountains National Park, many herbs are found adjacent to, or beneath, a dense, closed canopy of trees (LAI > 5 often). Published studies show that their exposure to O_3 will be much less than plants out in the open or above the canopy. Thus, their sensitivity to O_3 will be less than what one would assume based on the readings from stationary monitors, all of which are located in open areas, or above the canopy.

17. I believe the benchmark explanation section (7.3.1.4) is particularly difficult to read and comprehend; in particular, the concept of percent of biosites with any foliar injury (5%, 15%, etc.). What exactly is being evaluated here is confusing – foliar injury or just the presence of foliar injury without regard to its magnitude or the percent of sites showing injury? Also, the English in this section needs revision (incomplete and confusing sentences). For example, the base scenario is confusing. The explanation in Table 7-6 states: "*17.7% of all biosites…showed any injury (the W126…above which a consistent percentage of all biosites…showed any injury.*" Just what exactly does this mean? What is meant by "consistent"?

The goal of refining Kohut's analyses of Park sensitivities, or vulnerabilities to O_3 is laudable, especially with regard to using the W126 and avoiding the misleading impression of "thresholds", hence the use of the term "benchmarks". However, how this is explained and presented to the reader could be greatly

improved so as to enhance clarity and purpose.

18. The uncertainty and variability analyses are well characterized for foliar risks. The summary though (page 7-72), promulgates the confusion mentioned above in comment 15 that there is no relationship between foliar injury and either O_3 or soil moisture, when in fact, there is.

Chapter 8: Synthesis

19. I think this chapter should be more in the form of an executive summary. Currently, it rehashes much of the methodologies employed in earlier chapters, which distracts from its goal of summarizing and synthesizing the results of the risk analyses. Here is where editing out extraneous material could assist staff in getting the main points out succinctly to the public. I suggest greatly shortening this chapter.

Executive Summary

20. The executive summary is very well written. It is what I had in mind after reading Chapter 8. This clearly gets all the main points across to the reader, and more than adequately summarizes and synthesizes the results from the rest of the REA.

Armistead (Ted) Russell

Like the Health REA, this WREA is a marked improvement over the prior Draft, and over the WREAs from years past, and it shows a very positive evolution in the approach and the presentation. It also does a good job of balancing readability and detail.

In terms of the air quality characterization, many of the things I said about the Health REA go here as well. I like the use of an advanced air quality model to capture ozone responses to emissions controls. This should provide a more realistic set of exposure surfaces to characterize what happens when you meet various air quality metrics. They have also done a more advanced and comprehensive analysis of welfare endpoints. The resulting document is nice and concise, achieving a good balance between depth and readability.

Chapter 1. Introduction: Good. No real comments.

Chapter 2. In general, I found Chapter 2 readable and sufficient.

Minor comments:

2-2 ;16 and 18: I would not use "local valleys" to describe local decreases in ozone as the use of "valleys" has a geographic connotation that may be confusing.

2-4 l24: Do you mean "intrusions" not "inversions"?

Figure 2-1 is not that effective as shown.

Chapter 3: Scope.

Question 2. I Thought the Scope read well and provided a good view of what was done in the last assessment and what was being done here.

Section 3.2.1.3 is weak on describing how they simulated just meeting the various standard levels.

Minor Comments:

3-13 l4-5: In what way did Acadia National Park "not fit" the selection criterion? Be more explicit.

3-14 l7: Use "practical," not "possible."

Chapter 4: Air quality characterization.

Starting first with the charge questions:

1. Question 4. The use of HDDM-based adjustment is a major step forward. Reading this chapter and the supporting Appendix, and the appendices from the H-REA, demonstrate a considerable

amount of work, thought and analysis. I would recommend that they figure out which interpolation method is best for both the H-REA and W-REA analyses in the future such that they can more readily compare resulting fields. I think that any of the three methods (VNS, eVNA and DS) are probably fine for this type of analysis since the key is primarily in the differences between fields. True, one may show to be more accurate for one type of analysis, but it would be good to use on method throughout both the HREA and WREA. Much less, the differences are quite small, and the rational for using VNA is not strong. The choice is based on VNA having a lower bias than DS, but DS had a higher R2 and lower error, and eVNA had a lower bias than VNA. Again, I doubt that the choice makes much of a difference given how the results are used.

2. Their discussion of uncertainty is fine, though very qualitative. I keep hoping for a quantitative analysis, even if with lots of caveats. The statement found in part E of the table that "benefits of reducing high ozone …. would be generally underestimated." Still needs to be better supported. I would make sure that each of the uncertainty estimates (magnitude and direction) is consistent with the H-REA.

I really liked the characterization/comparison of the fields found in 4.3.4. The presentation was to the point, and the figures presented the findings in a very compact fashion. Figures 4-15 and 4-16 are a very nice addition to just the maps. The most striking result of this chapter, and one that should be emphasized in any summary discussion, is the similarity in the national surfaces and frequency distributions for the existing standard and W126 of 15 ppm-hr. (Also, I would call Figures 4-15a/16a frequency distributions not probability densities.) It would be great to shown how the other metrics compare (e.g., health standards of 60, 65 and 75 compare with W126s of 15, 11 and 7). This should also be in the PA.

I think their use of nine regions is fine.

One concern is that Chapter 4 needs a summary that provides an overall view of the results.

I would have liked to see how much emission reduction is required in each region to reach each level within the chapter and not have to go to the appendix. Bring Table 4A-2 from the appendix up to the text. There is much to be learned from this table. For one, while the two surfaces and frequency distributions look similar for the 75 ppb and W126 of 15, the control levels required are rather different.

<u>Minor comments:</u>

4-7, l22. Do you mean US monitors outside of the contiguous US, or all monitors outside the contiguous US?

Figures 4-12/14. Label what the white area stands for.

Chapter 6: The figure captions are not adequate. For example, Figs. 6-4 and 6-5 have multiple lines that are not explained. Figs. 6-6 through 6-10 "RBL" in the figure is not given in the figure caption (e.g., it should have "Relative Biomass Loss (RBL)…"),

Figures 6-6 though 6-10: I would keep the same scales to the extent that is practical, and make the numbers have fewer significant figures.

Chapter 7: No major comments.

Minor Comments:

Figures 7-9 though 13: How do you get non-monotonic behavior? Please explain.

7-2 l 7: "for have" should be "have".

Chapter 8: Synthesis. I think the synthesis chapter is potentially important, though the current chapter is not as synthetic as it might be. Much of it is more of a summary, and maybe it should be called "Summary and Synthesis". Sections 8.3 and 8.4 are more synthetic, and Section 8.5 is a reasonable recap of the uncertainties, but not a synthesis. What should the Administrator/ reader take away from the uncertainty analysis?

I think the Chapter ends with one of the most important observations, that being that the difference in the just meet 75 ppb and just meet W126 of 15 is key. This also suggests that a further analysis of how other 8-hr standards match with W126 standards is important. How would one answer "If an 8-hr standard of 70 (or 65 or 60) ppb were adopted for the primary standard, at what level would a W126 standard have to be placed to provide any benefits, and how much benefit would be derived?"

Minor Comments:

8-22, l 13: "Figure 7-8" should be "Figure 7-9".

Executive Summary:

I liked the executive summary.

The Summary and Conclusions in the ES should include the finding that the 75 ppb standard and W126-15 lead to similar ozone levels and significant reductions from current levels in terms of long term exposure (using W126 as a measure). That is well stated in the Conclusions. However, it should also note that the control levels are not necessarily the same. Further, it should include results from the analyses of meeting various W126 potential standards in terms of welfare outcomes evaluated (e.g., consumer surplus/producer surplus and carbon sequestration).

One thing it lacks is a feel for what current ozone levels are throughout the US. I am not sure if it might make Fig. ES-2 too messy, but if the current estimated W126 could be shown, that might help transmit this information. If this is not done, one could merge ES-6 and ES-2 as they are both large figures.

Not sure having a picture with bags of pine cones is a good use of space. What information is being transmitted with the inset of the GSM NP? A bit more comment is in order.

When presenting carbon sequestration, it might be good to provide some feel for the current rate of carbon sequestration by the forestry and agriculture sectors, as well as taking cars off the road. Likewise, when discussing pollutant removal, some context would be good.

You should refer to the National Park as "Kings Canyon", at least the first time.

Individual Comments: Peter Woodbury

Chapter 1: Introduction

1. To what extent does the Panel find the introductory and background material, including that pertaining to previous reviews of the O3 standards and the current review, to be clearly communicated and appropriately characterized?

Overall, this chapter is useful and provides an appropriate level of detail, see specific comments below.

Page 2-3, lines 10-14. Because the W126 is so important to the WREA, I think a figure showing it's weighting scheme should be included either in the introduction, or located in Chapter 4 and referenced in the introduction. This figure would show the ozone concentration on the independent axis, and the weight on the dependent axis.

Page 2-3, starting on line 19. See comments elsewhere about clarifying that "natural" VOC emissions actually include anthropogenic emissions due to human management of agricultural and forest ecosystems, including the type of vegetation, fire regimes, etc.

Chapter 2: Conceptual Model

2. To what extent does the Panel find that the discussions accurately and clearly reflect the air quality, ecosystem effects evidence, ecosystem services, and exposure and risk considerations relevant for quantitative assessment, building from information contained in the final ISA?

Overall, this chapter is useful and provides an appropriate level of detail, see one comment on Figure 2-2 below.

Page 2-9, Figure 2-2. The quality of this figure should be improved, specifically some arrow colors are hard to see and the resolution of the text can be improved.

Chapter 3: Scope

3. To what extent does the Panel find the scope of the welfare risk and exposure assessment is clearly communicated?

As for Chapters 1 and 2, this chapter is useful and provides an appropriate level of detail, see detailed comments below.

Page 3-9, beginning line 22. Be consistent in using "FASOMGHG" vs "FASOM".

Page 3-9, beginning line 27. I don't think FASOM was designed for use specifically by EPA to assess ozone impacts.

Page 3-11, lines 11-12, the end of this sentence doesn't make sense.

Chapter 4: Air Quality Considerations

4. What are the views of the Panel on the appropriateness of the methods used to characterize O3 air quality for the exposure and risk assessment? What are the views of the Panel on the HDDM-based adjustment methodology used to adjust O3 concentrations to just meet the existing O3 standard and levels for average W126 scenarios, coupled with the interpolation method used to create a national

surface of W126 concentrations for all scenarios?

5. To what extent does the Panel find that the discussion of uncertainty related to the air quality inputs to the exposure and risk assessment appropriately includes important sources of uncertainty?
Page 4-31, Table 4-2, and related discussion. It seems to me that there is likely to be a strong bias effect of using across-the-board NOx reductions. For the large climate regions used, there is a wide variation in ozone exposure values throughout the region. If "across-the-board" cuts are used to reduce ozone exposure in the highest locations, then ozone exposure in locations where it is lower to start with will tend to become very low. But an actual targeted control strategy would likely only reduce NOx in the locations necessary to bring the high ozone-exposure locations into compliance, thus having much less effect on other locations than would across-the-board cuts. If this bias is large, then it strongly affects all of the risk analyses based on the alternate standards, because impacts of ozone under the alternate standards would be underestimated because ozone exposure values are underestimated for much of each region. This issue seems to be unique to the WREA, because we are interested in ozone exposures throughout the entire region, instead of just the urban regions. This issue is acknowledged in this table, but the influence is listed as "both". Instead, I think it should be listed as underestimating ozone exposure and risk, possibly to a large degree. Furthermore, I think some effort should be expended to try to quantify this source of bias. There is some discussion in the table of uncertainties in ozone reduction scenarios, but these are all related to the urban analyses, not the rural/regional analyses.

Chapter 5: O3 Risk to Ecosystem Services
6. To what extent does the Panel find the assessment, interpretation, and presentation of the methods and results of the updated ecosystem services assessment to be technically sound, appropriately balanced, and clearly communicated?
In general, this chapter is useful and at an appropriate level of detail. However, I think some further information about willingness to pay related to vegetation, and also a re-arrangement of the water cycling and habitat sections would be helpful, as would a clarification of conceptual models for these risk assessments (see detailed comments below).
7. To what extent does the Panel support the revised structure of the ecosystem services discussions, including integrating ecological effects analyses directly with the ecosystem services assessments?
This integration is useful, see comment above for suggested improvements.
8. To what extent is the combination of O3 exposure data with other data sources (e.g. fire data, bark beetle maps, trail maps) to link areas of concern/interest with areas of higher vegetation risk due to O3 technically sound?
Such analysis is worthwhile, but since the contribution of ozone to fire risk and bark beetle damage is not quantified, the utility is limited.
9. To what extent does the Panel find that the discussion of uncertainty and variability has included all important sources of uncertainty and variability and appropriately characterized their relationship to the ecosystem services estimates?

In general, this discussion and the summary table are useful. However, on Page 5-20, Section 5.7. Throughout the discussion, I suggest changing the usage of "not possible" and "not quantifiable" and the like to something that acknowledges the limits of current information and current resources. Something like "not currently feasible" would better reflect that more could be done with more time and resources.

Specific comments for Chapter 5.

Page 5-4, Table 5-1. I suggest adding a column for "not important" instead of the footnote, then the totals will presumably sum to 100%.

Page 5-4, line 19 to page 5-5 line 3. What is the linkage between fish and ozone impacts on ecosystems and habitats? Is there information on willingness to pay for plant species, for which there is substantial evidence of effects? I think there needs to be a better linkage in this section with a clear conceptual model of how ozone affects an endpoint, and how much people value that endpoint, or changes in that endpoint (such as population size of a valued species). For fish, some such information is presented in the subsequent section, so perhaps a material could be rearranged to support conceptual models.

Page 5-7, Figure 5-3. I can't distinguish the color codes on this map, or see any difference between the 3 panels.

Page 5-12, Figure 5-7. . I can't see any difference between the 2 panels.

Page 5-13, line 17. Clarify the bark-beetle induce foliar damage is not included in Chapter 7.

Page 5-15, Table 5-5. As for Table 5-1, I suggest adding a column for "not important" instead of the footnote, then the totals will presumably sum to 100%.

Chapter 6: Biomass Loss

10. To what extent does the Panel find the assessment, interpretation, and presentation of the methods and results of the biomass loss risk assessment to be technically sound, appropriately balanced, and clearly communicated?

Page 6-1, Figure 1. I think the word "National" should be removed from the provisioning services of timber harvest and agricultural harvest. See my comments from previous versions of the WREA and PA and elsewhere in comments on the 2nd draft documents that yields are important at local and county scales, not just at the national scale.

Figures 6-2 and 6-3 should be improved by moving the legend to the right of the main figure panel and arranging the legend species in the same order (top to bottom) as in the main figure panel.

Page 6-9. Tulip Poplar "summary" box. Replace "ZELIG and lower" with "ZELIG at lower"

Page 6-9. What is the reference for the statement for aspen that "OTC studies found very consistent biomass loss between seedling and adult trees"? Does this statement refer to saplings in the Aspen FACE study or to something else? It's hard to put an "adult" tree in a chamber!

Page 6-17. Table 6-6 would be improved by separating individual values into columns, rather than presenting 3 values connected by slashes. This would make it much easier to read the values in the table.

Page 6-35, Table 6-13. I am not familiar with the cited source. But for firewood, our estimate for New York State alone is 1.2 million cords per year This New York value is a personal communication from Sloane Crawford of the NY DEC, but there are older survey data estimating 3 million cords used during

the 1989-1990 winter, and 890,000 cords used in the 1994-1995 winter (Canham & Martin 1996, see below). All of these values for a single state are much higher than the value for the US in the table, so I think the estimate in the table is much too low, or is in different units.

Canham, H. O. and T. D. Martin. 1996. Residential fuelwood consumption in New York State 1994-1995. #3 of the New York Center for Forestry Research and Development, SUNY-ESF, Syracuse, NY.

Regarding crops, it is progress to use regions rather than only national results (for example Figure 6-18, and discussion on page 6-41). However, I think it would be even stronger to summarize some results in tabular form by county, based for example on the results shown in the Appendix figures A-31, A-32, and A-37. Focusing on sensitive species might make sense. The number of counties in which yield loss is predicted to exceed certain yield loss percentages could be presented for the current and for alternative standards. I acknowledge that this information is summarized at the national level in tables and figures such as 6-18 and 6-19. However, I think it would strengthen the results to show additionally in a table the number of counties exceeding certain predicted yield loss values for the current standard compared to current exposure levels and for alternate standards compared to the current standard.

Page 6-38, line 18. Should “6B” be “6B, and 6C”? Note that 6A also provides county level estimates of relative yield loss.

Page 6-50 and beyond (Section 6.6.2). For the i-Tree model, it is reasonable to model just the species that have concentration-response functions. However, only those species should be modeled – it should not be assumed that other species do not respond to ozone. The values in the bottom rows of Table 6-19 (page 6-52) show the percentages, and this is helpful information. But I suggest that results be presented as the percentage change (RYL) for modeled species only to avoid bias. Also, on Page 6-50, line 20-22 discusses the use of “standard growth rates” What are these rates? If they are based on measured growth rates, these rates occurred over some years with some ozone exposure, so the baseline then includes that ozone exposure, which needs to be quantified. But elsewhere it changes in yield seem to be compared to a zero ozone baseline, for example Page 6-51 line 1 and in Table 6-20 (page 6-54). How was a zero ozone growth rate calculated? At this point, I don’t know how to interpret any of the i-Tree results, including those for pollution removal.

Page 6-59. The citation of Carlton et al. (210) should be clarified. To my knowledge, that publication is concerned with secondary organic aerosol formation, not other air pollutants such as ozone. I’m not sure how relevant effects on SOA are for effects on ozone.

11. To what extent does the Panel find the carbon sequestration estimates from the Forest and Agricultural Sector Optimization Model Greenhouse Gas version (FASOMGHG) (Section 6.6.1) to be technically sound and appropriately characterized?

Page 6-49. Another important source of uncertainty in modeling both forest yield and carbon sequestration using FASOMGHG is discussed below (question 12). In brief, for mixed-species forest stands, total stand growth will be affected less than the average response of individual trees because of competition among species varying in sensitivity to ozone. Conversely, effects of ozone on sensitive

species are underestimated by using median response values within and among species. I raised this issue in review of the first draft WREA and PA, but it has not been addressed, perhaps because it is challenging to do so. But again, this is not just a source of uncertainty, but a source of bias in all of these analyses.

12. To what extent does the Panel find the weighted biomass loss analysis in Section 6.8 to be a technically sound approach to assess potential ecosystem-level effects nationwide and in Class I areas?

As in my comments on the first draft WREA and PA, I still have a question about the RBL values weighted by basal area. Does the denominator basal area in the calculation include only the 12 species with C-R functions or does it include all species? If the latter, it is biased. If the former, the interpretation will vary depending on what fraction of the basal area is for species without C-R functions, as seems to be acknowledged in the text. Furthermore, if the goal is to assess ozone effects on total biomass growth of a mixed-species forest, then this value is not very informative because it will overestimate impacts in mixed species forests because of not including competition between sensitive and insensitive species (see previous comments on competition). If the purpose is to assess ozone impacts on sensitive species, this value is also not informative because it underestimates impacts on sensitive species for the same reason. A comparatively small growth decline in a sensitive species (e.g. 2%) based on a seedling study may translate into a larger effect at the stand scale.

Because of issues with the weighted biomass loss approach, greater emphasis should be given to results produced for individual trees, as shown in Figures 6-11 and 6-12. Most of this information is currently relegated to Appendix 6C. However, these results could be summarized in a table for individual species to quantify the area (perhaps as fraction of total area of the species or counties) in which biomass loss is predicted to decrease by 1%, 2%, and 5%. If RBL for ozone-sensitive species is an important welfare endpoint, this information would be useful. But it should be noted that these single-species predictions could greatly underestimate RBL for sensitive species occurring in mixed-species stands, as many do.

As discussed above, for predicted biomass loss for mixed-species stands, the weighted RBL would likely overestimate RBL because of competition between species that vary widely in sensitivity to ozone.

Page 6-74, line 3. Change “studies” to “studies with mature trees”.

13. To what extent does the Panel find that the discussion of uncertainty and variability has included all important sources of uncertainty and variability and appropriately characterized their relationship to biomass loss estimates?

The tabular summary (Table 6-27) is a good format to summarize the discussion. See comments above for additional issues related to uncertainty. Note that uncertainties and bias can be reduced substantially based on recommendations above, such as presenting a percentage change in urban carbon sequestration instead of a ton basis. Changing the metric means that analysis can be conducted just for species with C-R functions, greatly reducing the bias of implicitly assuming all other species are not at all sensitive to ozone. There is still an uncertainty in extrapolating from the known 12 species to all species, but a major source of bias has been removed.

A potentially important source of bias is the use of "across-the-board" reductions in NOx to estimate ozone exposure for alternate standards (see response to Charge Question 5, Chapter 4 above). If this bias is substantial it could mean that the ozone exposure is underestimated and thus the benefit of the alternate standards might all be underestimated. This could mean that the benefits of any of the alternate standards compared to "just meeting" the current standard and are underestimated as well.

Chapter 7: Foliar Injury

14. To what extent does the Panel find the assessment, interpretation, and presentation of the methods and results of the foliar injury risk assessment to be technically sound, appropriately balanced, and clearly communicated?

Overall, this chapter is informative, well written, and with an appropriate amount of detail, with further details provided in appendices. The analysis is technically sound, balanced, and clearly communicated. The tables and figures in particular are informative and appropriately summarize a lot of important information in a way that is useful for this document and for the PA. I do provide a few specific suggestions for improvements to the figures and tables and a few other comments below.

Page 7-6, Figure 7-3. It would be helpful to identify the panels in the legend (species, with or without ozone damage). Also, I think there is a 4th species in the figure that is not listed in the text, perhaps it is black cherry?

Page 7-8, Table 7-2. Align on decimal, also be consistent in number of places to the right of the decimal point within each column.

Page 7-27, Table 7-6. In the 3 right-most columns, remove "W126" from within table cells and place it below the column header, then align numerical values on the decimal point. This will make it easier to read the values in the table. More generally, in all tables provide the units in each column just below the column header.

Page 7-32, Figure 7-20. I suggest a white background for the maps to improve the visibility of the symbols. Also, the font is so small for the park codes that I'm not sure it's worth including the codes. Using a filled circle symbol would make it easier to see the patterns.

Pages 7-34 to 7-35, Table 7-8. If I understand correctly, only the bold rows have the possibility of an "average monitor" or a "highest monitor" being different (because they have more than one monitor. Adding a column for "single monitor" for such parks would better represent the data.

Page 7-38, Table 7-9. Removing the "%"symbol from the body of the table and placing it under each appropriate column heading and aligning on the decimal point would make it easier to read the values in the table.

Page 7-40, Table 7-10. If you have a preferred time period (7, 5, or 3 months) you could show foliar injury values just for that period and show the change in values for the other 2 periods. This would make it easier to see the differences due to the averaging period.

Page 7-48, Table 7-11. Are the units for WTP per day or per visit? This question applies to

similar tables for each of the other park case studies.

Page 7-48, Table 7-12. For this and other tables, I suggest putting the units in each column just below the column heading.

Page 7-60 and others. Figure 7-29 and others. Spell out abbreviations for parks in figure captions, and provide more descriptive titles so that the figure can be better understood without reading the text. For example, something like “Percentage of plant species sensitive to foliar symptoms from ozone exposure present along trails in the Rocky Mountain National Park”.

15. What are the views of the Panel on the analysis of the Forest Health Monitoring data in Section 7.2, including the finding of the lack of a statistical relationship between the severity of foliar injury and W126 index values or soil moisture levels?

This analysis is generally appropriate, some specific suggestions are presented below.

Page 7-15, line 5. Insert “spatial” before “resolutions”.

Page 7-15. I am not sure that censored regression is appropriate. However, the analysis shown in Figures 7-9 to 7-13 does seem very useful and appropriate.

In Figures 7-9, 7-10, 7-11, 7-12, and 7-13, provide some summary information about the sites in the legends (at least the number of sites, or the range in the number of sites, and some mention of the type of sites.

Page 7-20, In Figure 7-13 (and all other similar figures), order legend values to correspond to order of regions in the panel.

16. What are the views of the panel on the appropriateness of the characterization of vegetation strata (i.e., herb, shrub, tree) for the analyses of sensitive species cover in the three national park case studies (Section 7.4)?

These strata seem appropriate.

17. What are the views of the Panel on the usefulness of the screening-level assessment of visible foliar injury in national parks in Section 7.3? Specifically, what are the views of the Panel regarding conclusions appropriate to draw from applying the W126 benchmark scenarios derived from the national-scale Forest Health Monitoring data analysis in the screening-level assessment?

This analysis seems appropriate, as does the use of the benchmark scenarios derived from the FHM data.

18. To what extent does the Panel find that the discussion of uncertainty and variability have covered important sources of uncertainty and variability and appropriately characterized their relationship to foliar injury risks?

This discussion is useful and appropriate, and the tabular summary (Table 7-23) is a good format to summarize the discussion.

Chapter 8: Synthesis

19. To what extent does the Panel find the synthesis to be a useful integration and summarization of key results and insights regarding the overall welfare exposure and risk analyses?

The scope and content of this Executive summary is generally appropriate. However, it will be important to assure that comments for the entire document are also applied to summary sections such as this chapter, and the Executive Summary. See also some detailed comments below, some of which suggest changes that affect the selection of and interpretation of these summary results.

Page 8-4, lines 3-5. Clarify whether CASAC suggested these values in the context of 1 year or for a 3-year average.

Page 8-7, beginning on Line 7. As discussed in comments elsewhere (see response to Charge Question 12), there are methodological issues with the weighted RBL approach. Because of issues with the weighted biomass loss approach, greater emphasis should be given to results produced for individual trees (see response to Charge Question 12).

Page 8-8, lines 3-7. I believe that the values of 4 and 10 ppm-hrs are based on the Monte Carlo type iterations shown in Figure 6-4. However, I do not think that these values are appropriate as a summary. As discussed elsewhere, it is not appropriate to combine all of these 12 species together and calculate a median response. Instead, it would be better to report the responses for individual tree species, since all of the C-R functions for tree species are for common, widespread, important species. This is important, because as shown in Figure 6-4 and Figure 6-2, there is a very wide range in ozone sensitivity among the 12 species. The values of 4 and 10 ppm-hrs fail to capture this variability -- specifically they fail to capture expected impacts on black cherry and cottonwood.

Page 8-8, beginning line 8. As discussed in comments elsewhere in document, the regional results for relative yield loss for both trees and crops do not capture the higher losses that occur for some counties, so presenting the number of counties that would exceed benchmark values would better characterize impacts. Similarly, national and regional impacts on producer and consumer surpluses do not capture that there will be winners and losers in different locations, and the impacts do not “average out” if you are one of those who is losing due to ozone impacts.

Page 8-9 and 8-10. The methods used for the FASOMGHG are problematic, as discussed in previous comments on the first drafts and these second drafts. In brief, for mixed-species forest stands, total stand growth will be affected less than the average response of individual trees because of competition among species varying in sensitivity to ozone. Conversely, effects of ozone on sensitive species are underestimated by using median or average response values among species. These same issues apply to estimates of carbon sequestration.

Page 8-12, Figure 8-2. The quality (resolution) of the figure should be improved.

Page 8-14, beginning line 29. As discussed elsewhere, there are methodological issues and questions

related to the i-Tree model analysis that are so serious that I don't know how to interpret the results (see comment above for "Page 6-50 and beyond (Section 6.6.2)").

Page 8-16, section 8.2.2.3. As discussed in comments elsewhere in document, clarify text to avoid implying that known ozone-sensitive species include all ozone-sensitive species.

Page 8-17, lines 6-9. See comments elsewhere about the need to develop different summary values than those presented here for crops and trees (see comment below for Page ES-1, paragraph 2).

Page 8-16 onward. A potentially important source of bias is the use of regional "across-the-board" reductions in NOx to estimate ozone exposure for alternate standards (see response to Charge Question 5, Chapter 4 above). If this bias is substantial it could mean that the ozone exposure is underestimated and thus the benefit of the alternate standards might all be underestimated. This could mean that the benefits of any of the alternate standards compared to "just meeting" the current standard and are underestimated as well. This bias affects all quantitative risk analysis, including crop and tree biomass loss and foliar injury in parks.

Page 8-21, Section 8.3.2. As discussed in comments elsewhere in document, the average national results for producer and consumer surpluses miss the fact that there will be producers in some regions that will be winners and producers in other regions who will be losers, this is not captured in aggregate national estimates.

Pages 8-24 to 8-25, Table 8-1. This format of summary table is very helpful. However, I have concerns about many of the values presented and would like to see many changes based on comments elsewhere about methods of analysis, sources of bias, averaging of results across space and time, etc.

Page 8-26, Table 8-2. This format of summary table is very helpful. However, I have concerns about many of the values presented due to issues with the methods used with the i-Tree model, and potential bias in estimating ozone exposure with regional "across-the-board" reductions in NOx.

Page 8-27, lines 8-9. I don't think the 12 tree species are "normally distributed" in terms of response to ozone exposure in the range of the alternate standards being investigated. Instead there is one extremely sensitive species (cottonwood), one sensitive species (black cherry) etc.

Page 8-31, lines 4-18. This section acknowledges uncertainty in modeled ozone exposure adjustments from use of regional "across-the-board" reductions in NOx to estimate ozone exposure for alternate standards. But I think this is not just uncertainty, but a potentially important source of bias that could affect all of the quantitative risk analyses (see response to Charge Question 5, Chapter 4 above).

Page 8-32, lines 3-11. See comments elsewhere about problems with the FASOMGHG analyses for mixed-species forest growth.

Page 8-33 to 8-36 (Conclusions). See comments above and throughout the WREA.

Executive Summary

20. To what extent does the Panel find the Executive Summary to be a useful summary of the data and

methods used to estimate exposures and risks to ecosystems and the key results of the assessment?

The scope and content of this Executive summary is generally appropriate. However, it will be important to assure that comments for the entire document are also applied to summary sections such as this chapter, and the Executive Summary. See also some detailed comments above for Chapter 8 and below, some of which suggest changes that affect the selection of and interpretation of these summary results.

Page ES-1, paragraph 2. I believe that the values of 4 and 10 ppm-hrs are based on the Monte Carlo type iterations shown in Figure 6-4. However, I do not think that these values are appropriate as a summary. As discussed elsewhere, it is not appropriate to combine all of these 12 species together and calculate a median response. Instead, it would be better to report the responses for individual tree species, since all of the C-R functions for tree species are for common, widespread, important species. This is important, because as shown in Figure 6-4 and Figure 6-2, there is a very wide range in ozone sensitivity among the 12 species. The values of 4 and 10 ppm-hrs fail to capture this variability -- specifically they fail to capture expected impacts on black cherry and cottonwood.

Page ES-1, paragraph 4. Clarify “adjusting” to “modeling improved”

Page ES-2, Figure ES-1. Reformat or increase image size to increase font size, also increase resolution.

Page ES-4, Figure ES-2. The resolution should be improved in this figure. Also using a different symbol shape in addition to different symbol colors would help distinguish among the different types of monitoring stations.

Page ES-5, Figure ES-3 The font size should be increased in this figure.

Page ES-5, Figure ES-4. Reformat or increase image size to increase font size, also increase resolution.

Page ES-6, Figure ES-5. Reformat or increase image size to increase font size, also increase resolution.

Page ES-6, Paragraph 2. As discussed in comments elsewhere in document, for timber production and crop yield loss, provide the fraction of counties with biomass loss above benchmarks, in addition to the average national results.

Page ES-6 and ES-7. As discussed in comments elsewhere in document, the average national results for producer and consumer surpluses miss the fact that there will be producers in some regions that will be winners and producers in other regions who will be losers, this is not captured in aggregate national estimates

ES-7. See comments elsewhere in document about problems with the national forest yield loss methodology and with the i-Tree modeling assumptions. In brief, the national methodology underestimates effects on individual sensitive tree species and likely overestimates effects on mixed-species forest growth rates. The i-Tree modeling assumes that species without C-R functions are not sensitive to ozone, which is not a reasonable assumption.

Page ES-9, Figure ES-7. Reformat or increase image size to increase font size, also increase resolution.

Page ES-10, paragraph 1. As discussed in comments elsewhere in document, clarify text to avoid implying that known ozone-sensitive species include all ozone-sensitive species.

Page ES-12, paragraph 2. Fix spelling error in first sentence of Conclusions.

Page ES-12, paragraph 3. Explain more thoroughly what the effects mean – are they for individual species, and how many species have C-R functions for crops and trees.