Preliminary Individual Comments

on the Welfare Risk and Exposure Assessment (Second Draft)

CASAC Ozone Review Panel

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Contents

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Welfare Risk and Exposure Assessment for Ozone—Second External Review Draft

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 C-R relationships based on above canopy monitoring. Staff notes that there is understory foliar injury data in the national assessment but that only tree data were included in the analysis. This may be a missed opportunity.

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.

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.

Howard S. Neufeld

Comments on Welfare Risk Assessment – 2nd Revision

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.

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

Review of Ozone REA-Welfare 2nd Draft.

Like the Health REA, this REA is a marked improvement over the prior Draft, and over the REAs 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. Is it me, or is missing an Executive Summary?

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.
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 fund 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, an done 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. This could be conveniently done in a table.

<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 tin the figure caption (e.g., "Relative Biomass Loss (RBL)…"),

Figures 6-6 though 6-10: I would keep the same scales as 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".

Peter Woodbury
10 March 2014

Review of the Second Draft Welfare Risk and Exposure Assessment for Ozone

[NOTE: As of 10 March 2014, I have not completed my review of this document. I include comprehensive comments for Chapter 7, and a few comments for Chapter 6, and will provide more as soon as I can]

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?

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?

Chapter 3: Scope

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

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?

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?

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?

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?

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?

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?

[NOTE: As of 10 March 2014, I have not completed my review of this chapter, so I include a few comments below and will provide more as soon as I can]

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!

Regarding crops, it is progress to use regions rather than only national results (for example Figure 6-18). 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.

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?

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?

I have read this section a couple of times, and still don't understand the calculation. 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.

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?

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?

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?