Additional Preliminary Individual Comments on the Welfare Risk and Exposure Assessment

March 23, 2014

Contents

03-23-14 Preliminary Draft Comments from Clean Air Scientific Advisory Committee (CASAC) Ozone Review Panel on the Welfare Risk and Exposure Assessment (Feb. 2014). These preliminary pre-meeting comments are from individual members of the Panel and do not represent CASAC consensus comments nor EPA policy. Do not cite or quote.

George Allen

March 23, 2014

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 REA, and are generally 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 welfare REA 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.

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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.

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.

Peter Woodbury

[NOTE: As of 23 March, I have added additional comments beyond the 22 March version. These new comments are preceded by "[23Mar14]" All comments since the March 10 version are shown as "tracked changes]**.**

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 Chaptesr 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?

[23 March] 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 assessements (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 6-40, Table 6-15. Hybrid poplar commonly includes parentage of Populus deltoides and Populus nigra, so the Cottonwood function might be more appropriate for hybrid poplar than the aspen function. Because the Cottonwood function is much more sensitive to ozone, this would affect the results substantially for this crop, although this crop is not widespread, it is also planted as an ornamental.

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.

[March 23] 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?

[Note – all of the comments for Chapter 8 are from 23 March, so they aren't labeled individually below]

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?

[Note – all of the comments for the ES are from 23 March, so they aren't labeled individually below]

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.