

Welfare REA Preliminary Individual Comments. Do not cite or quote. These are preliminary individual comments from members of the CASAC Ozone Review Panel for discussion at the September 11 – 13, 2012 meeting. They do not represent EPA policy or consensus CASAC advice. Updated 9-4-12.

**Preliminary Individual Comments on the Welfare Risk and Exposure Assessment for Ozone  
(First External Review Draft updated August 2012)**

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### ***Comments from Dr. Daniel J. Jacob***

Responses to charge questions 4-6 of Welfare REA Draft 1

#### ***Chapter 4: Air Quality Considerations***

*4. To what extent does the Panel consider the years of air quality data to be appropriate for use in the exposure and risk assessment?*

I think that the use of 2006-2010 is appropriate.

1. 4-4, line 1: I don't understand the logic for excluding exceptional events. They would have a welfare impact, even if they can't be controlled. The 2008 California wildfires (which accounted for all the exclusions according to the text) seem like an odd exclusion since Singh et al. (Atmos. Environ. 2010) showed that these fires didn't make significant ozone unless mixed with urban plumes.
2. 4-4, line 1: Figure 4-3 shows a W126 hotspot from the wildfires in central Idaho. Why weren't they removed as exceptional events, like the California wildfires? That seems inconsistent but I think I know why – that's because there were no observations to exclude! The W126 hotspot in central Idaho is in my opinion a model artifact and I elaborate on that below.

*5. What are the views of the Panel on the approach used to develop a national scale surface of W126?*

1. I would like some more information on the data fusion approach, because some aspects of the results seem weird (see my comments to charge question 5 on the REA Health Draft 1). I think that it would be important to show comparisons of W126 in the fused data and in the actual observations for specific sites, and I would recommend using the CASTNet sites for that since they are so relevant to the secondary standard.
2. In particular, I'm surprised that Figure 4-3 shows such high W126 values over the intermountain west. This must largely reflect CMAQ information since there aren't many monitors there. However, I have some doubts as to the quality of the CMAQ simulation in that region. For example, the W126 hotspot in central Idaho is due to wildfires, but that just reflects the simulation of large ozone production from wildfires in CMAQ. There is evidence from the literature that models overestimate ozone production from wildfires (Singh et al., Atmos. Environ. 2010; Alvarado et al., JGR 2010). My own work analyzing the CASTNet observations shows no ozone enhancements from wildfires. For example, I see no ozone enhancements at Glacier National Park from the fires in central Idaho. I'm ready to acknowledge the controversy over ozone production from wildfires, but that would translate into large uncertainty in the national scale surface of W126.
3. Following on the above point, I think that there is enough uncertainty in CMAQ simulations of background ozone that using CMAQ to extrapolate ozone data to the national scale may be inappropriate for a W126-based standard. This can be assessed by

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comparing W126 in the fused data to observations at the CASTNet sites, as per above comment.

*6. Regarding the methods for simulating just meeting the ozone standard:*

*a) To what extent does the Panel find that the quadratic rollback approach used in the first draft REA for simulating just meeting the current standard is a reasonable approach?*

1. If the quadratic rollback approach is used then it should be implemented in the same way as for the Health REA. Right now it doesn't seem to be, notably in the correction for background. Background correction is probably more important for the welfare assessment than for the health assessment.
2. The quadratic rollback approach seems to do weird things. For example, it decreases the W126 in central Idaho even though the high W126 there is from wildfires that are not evidently controllable.

*b) Does the Panel have suggestions for alternative approaches for simulating just meeting the current secondary standard or alternative standards based on the W126 metric?*

1. The HDDM was proposed as a better alternative to the rollback method for the Health REA, and I endorsed this in my response to the charge questions for that REA. I think it would be a better alternative for the Welfare REA as well, and would address for example the above-mentioned weird result of the rollback method in central Idaho. See my responses to the charge questions on chapter 4 of the Health REA for further comments on application of the HDDM.

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## **Comments from Dr. Howard Neufeld**

### **REA Welfare Charge Questions August 28, 2012**

#### **Chapter 1: Introduction**

1. Does the Panel find the introductory and background material, including that pertaining to previous reviews of the O<sub>3</sub> 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 materials accurately reflect and clearly communicate the currently available welfare effects evidence, and the relevance of that evidence for quantitative exposure and risk assessment, as characterized in the 3<sup>rd</sup> Draft ISA?

#### **Chapter 3: Scope**

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

#### **Chapter 4: Air Quality Considerations**

4. To what extent does the Panel consider the years of air quality data to be appropriate for use in the exposure and risk assessment?

4-4, line 15 – not sure that “photochemically” is the appropriate word to use here.

Line 25 – you need to define “N”

4-5, lines 9-10: I think there is a mistake in the equation here, unless you defining N100 differently from how I have in the past. Should N100 be for hours  $\geq$  or just  $>$ ? With respect to the SUM06, isn't this definition an AOT06? Traditional SUM06 accumulate all hours  $\geq$  60 ppb, but don't subtract 60.

Figures throughout chapter, and all subsequent chapters: all of the figures showing kriged ozone surfaces, and also those showing biomass losses (any figure with a scale) are lacking units. All such figures should have units explicitly shown either on the figure itself or in the legend.

5. What are the views of the Panel on the approach used to develop a national scale surface of W126?

I agree with the methodologies used in this chapter.

6. Regarding the methods for simulating just meeting the ozone standard:
- a) To what extent does the Panel find that the quadratic rollback approach used in the first draft REA for simulating just meeting the current standard is a reasonable approach?

Based on the explanations in the chapter, I think this is a most reasonable approach to use.

- b) Does the Panel have suggestions for alternative approaches for simulating just meeting the current secondary standard or alternative standards based on the W126 metric?

No.

## **Chapter 5: Ecological Effects**

7. Regarding the assessment of relative biomass loss (RBL) for individual species:
- a) What are the views of the Panel on the use of the linear model forced through the origin to assess the proportional relationship between the relative biomass loss (RBL) values for each species comparing the RBL at recent ambient conditions to the RBL under the scenario modeling O<sub>3</sub> just meeting the current standard?

There are times when forcing a regression through the origin makes sense, especially when you have a situation where a value of zero for the independent variable mandates a zero value for the dependent variable such as crop yield vs acreage – you can't have any yield if there is no land. However, such regressions do not yield interpretable  $r^2$  values, although one author (Eisenhauer 2003) suggests several alternative substitutions to get around this. The more important issue to address is whether or not RBL is linear all the way to the origin, or if it increases non-linearly near the origin, and then becomes linear the further away it is. If so, then perhaps a non-linear approach is called for. Citation: **Eisenhauer, J.G. 2003. Regression through the Origin. Teaching Statistics 25: 76-80.**

It is my understanding that the RBL is based on biomass losses relative to an ozone exposure of zero. Since there are no areas where background ozone is this low for extended periods of time, is this the relevant baseline on which to base the RBL? What about selecting a robust estimate of background from

which to deduce RBL?

- b) To what extent does the Panel find that this an appropriate analysis to compare the proportional changes in RBL? Does the Panel have suggestions for alternative approaches that provides a comparable result and maintains the cell-by-cell approach to help control for environmental variability?

I found the approach used for determining RBL to be logical and pragmatic, given the databases they had to work with. However, there is no citation of this paper: **Matyssek, R., H. Sandermann, G. Wieser, F. Booker, S. Cieslik, R. Musselman and D. Ernst. 2008. The challenge of making ozone risk assessment for forest trees more mechanistic. Environmental Pollution 156:567-582.** The authors make an important distinction when doing modeling which is that one must distinguish between uptake (flux density) and sensitivity per unit uptake. Thus, two trees could take up equivalent amounts of ozone, but differ in sensitivity, due to a variety of internal mechanisms, so it is the combination of these two parameters that ultimately determine the risk to trees from ozone.

Furthermore, the modeling does not seem to take into account the range of genetic variation within a species. Perhaps some probabilistic approach could be taken to account for this through the use of some weighting factor, so that when the RBL is scaled up to the landscape, it is modified somewhat by this accounting.

Also, was there any discussion of the quality of the data used to construct the C-R curves? For example, I thought sugar maple was highly insensitive to ozone. The studies by Topa seemed to indicate that biomass losses occurred primarily at 3X ambient exposures. In her 2001 article in Plant, Cell and Environment, she states that there was no statistical difference in dry weight of seedlings in the 1.0X and 1.7X ambient treatments. This does not suggest a very sensitive species to ozone, yet it seems highly sensitive in the REA analysis. Why is this? How confident are we that the data used really do give us realistic RBLs? I was not sure of the citations or databases used to develop the 11 tree RBL curves – can that be provided?

- c) To what extent does the panel agree with the approach used to combine the 11 tree species into one analysis?

I think combining the analyses of all 11 trees into one analysis obscures individual tree responses, and puts too much weight on extremely insensitive species. If the goal is to protect the most sensitive, this would tend to bias

against that. Nonetheless, it is informative to see the range of responses among these species and the box diagram does a good job expressing that variation to the reader.

8. Regarding the assessment of RBL for combined species:

- a) To what extent does the Panel support the use of the Importance Values from the U.S. Forest Service to weight the RBL values in extrapolating from individual trees to larger ecosystem level effects?

I think this is critical to the analysis. It's important to first get the individual tree responses, and then to expand up the organizational scale to account for the abundances of each of these species in the forest (but see above discussion 7.b about genetic variation). This helps adjust the impact analysis to account for the situation where you might have a very sensitive tree species to ozone, but which is a minor component of the forest. From a RBL standpoint, this is the way to proceed. From a purely biodiversity point of view, this would de-emphasize such species, so one would have to make some provision for rare species that might be in danger of being extirpated, either directly from exposure to ozone, or indirectly through competitive interactions under ozone stress.

- b) What are the views of the Panel on the use of the summed-RBL as a metric to use for assessing effects at the larger ecosystem scale?

As noted above, I think this is an appropriate metric to use. However, it does not seem to take into account competitive interactions (e.g., ZELIG model), so the question does arise as to what might be the magnitude of difference in the metrics when competition is not accounted for.

- c) Does the panel have any recommendations for methods to include a wider range of tree species (beyond the 8 species included)?

I currently have unpublished data on several more tree species from the NPS-EPA funded OTC study done in Great Smoky Mountains National Park. The only way I know to include those response functions would be to have them peer-reviewed in some way, even if not in a formal journal publication. I am currently trying to complete these analyses with a statistician from the EPA. Other than this, I don't know of any other data that might be used. What about earlier data using OTCs? Are there any that might be included?

9. What are the views of the Panel on the use of federally designated Class I and Critical Habitat areas as endpoints for this analysis? Does the Panel have any suggestions for additional parks beyond Rocky Mountain National Park and Sequoia/Kings Canyon National Park that should be analyzed? Does the panel have recommendations for

additional or alternative geographic analysis areas that could be used?

Given that Class I areas are designated not to have significant declines in air quality, using them as endpoints seems quite reasonable, even if they constitute a small fraction of the landscape. It's a little greater stretch to justify using all critical habitats since some of these are not terrestrial habitats, and the linkages between the aquatic and terrestrial habitats are either unknown or difficult to quantify.

It might make sense to include Great Smoky, Shenandoah or Arcadia National Parks so that Parks in the east are included. And the western portion of Pennsylvania and eastern portions of Ohio seem hotspots that could use additional scrutiny. Western Pennsylvania contains the largest and most productive black cherry sites, and this species is one of the most sensitive species to ozone that we know of. It would seem reasonable to concentrate on forests where this species makes up a substantial proportion of the individuals, and where it is also under the greatest stress from ozone.

10. To what extent does the Panel find that the vegetation mapping data from USGS and NPS used to generate a scaled-RBL surface in Great Smokey Mountain National Park is appropriate?

The vegetation mapping is sufficient, but the ozone data seem overly coarse. In addition, these analyses don't seem to take into account the fact that ozone exposures increase substantially with elevation, and that the form of the exposure also changes (flat profiles at high elevations, exposing plants to ozone early in the morning when their stomata have a greater chance of being open).

11. Regarding the screening level foliar injury risk assessment:

- a) To what extent does the Panel find the updated assessment of foliar injury risk in national parks originally performed in Kohut (2007) to be an appropriate screening level risk assessment?

I think the Kohut assessments are an efficient way to screen national parks for potential impacts on plants due to ozone. I think the updated analysis used by EPA, which did not differ that much from Kohut's earlier analysis, but which did account for the recent declining trends in ozone, is highly appropriate. It is very interesting to note that no Park showed an increase in sensitivity to ozone with this analysis, while several moved to a lower sensitivity due to declining ozone exposures in recent years.

- b) What are the views of the Panel regarding the potential methods for estimating O<sub>3</sub>

exposure at additional parks?

I am not clear what these other potential methods are. If Parks don't have their own monitoring data, then some sort of kriging methodology using data from nearby monitors would be suitable I think. Using POMs data seems a reasonable thing to do also.

- c) What are the Panel's views regarding the appropriateness of requiring that two criteria must be satisfied (i.e., based on both W126 and N100) in order to receive higher risk ratings?

I think this somewhat conflicts with previous analyses that emphasize the cumulative nature of plant responses to ozone. The REA and PA both go to great lengths to justify using cumulative exposure indices, such as the W126, whereas here, a Park is moved down an entire sensitivity category based solely on whether or not the N100 index is exceeded (since both indices need to be exceeded, as I understand this). It is entirely possible to have a Park at high risk due to chronic, moderate ozone (say between 60 and 99 ppb), which could cause significant biological impacts, yet never exceed the N100. I would suggest that if the W126 is exceeded in all three years, even when the N100 is not, that the Park be classified as at high risk for adverse effects. I'm not as sure what to do if the converse is true, but I'm guessing that when the N100 is exceeded, so is the W126.

Given the inverse relationship that sometimes exists between soil moisture and ozone exposure, it is entirely possible that a plant could be at higher risk with slightly lower ozone (or years without peaks > 100 ppb) than in years with high ozone and where there are peaks > 100 ppb. Perhaps a three dimensional space needs to be developed, with soil moisture, W126 and N100 as parameters and specific volumes within that space designated as High, Moderate and Low risk.

- d) Is the Panel aware of any assessments of foliar injury in national parks conducted between 2006 and 2010 that could potentially be used to validate the updated risk ratings?

I would ask that the EPA incorporate the findings of Gretchen Smith's recent paper which summarizes the past 16 years of biomonitoring in the Forest Health Monitoring Program. While these analyses were not always done in national parks, the conclusions might be useful for interpreting foliar injury assessments, especially her insights regarding the relationship between wet and dry years and incidence of visible foliar injury. Here is that citation:

**Smith, G. 2012. Ambient ozone injury to forest plants in Northeast and North Central USA: 16 years of biomonitoring. Environmental Monitoring and Assessment 184:4049-4065.**

12. Regarding the assessment of cover of O<sub>3</sub> sensitive species:

- a) To what extent does the Panel find the preliminary analysis of sensitive species cover to be an appropriate and useful approach to highlight areas of potentially higher risk due to the presence of sensitive species?

These analyses are not yet done, so I cannot comment on them at this point in time.

- b) To what extent does the Panel find the vegetation mapping data appropriate to assess the cover of O<sub>3</sub> sensitive species in GSMNP?

I'd like to hear more from the people who developed this map to hear exactly what it means in terms of assessing sensitivity to ozone. One thing that concerns me is that herbaceous species, which are often in the forest understory, are exposed to significantly reduced amounts of ozone compared to foliage out in the open or at the top of the canopy. Thus, one should consider modifying exposure of these plants, perhaps in several ways, such as including one factor to account for lower light (and hence lower stomatal conductances perhaps) and lower ozone (see Neufeld et al. 1992 which shows that ozone at 1 m in dense, cove forests in the Smokies can be as much as 50% less than that above the canopy). Perhaps herbs and shrubs can have a dummy variable that indicates whether they are beneath a forest canopy or not, and which would then act as a surrogate for reduced exposure.

- c) What are the views of the Panel on the decision to not distinguish between vegetation strata (i.e. herb, shrub, tree)? To what extent does the Panel agree with this methodology relative to analyzing the strata individually?

See comment in section b above.

- d) What are the views of the panel on using benchmarks, similar to those used in the Kohut analysis of foliar injury risk, to allow estimates of change between

exposure scenarios?

I am assuming that “benchmarks” means “thresholds”. Given the coarseness of the analysis, I think benchmarks provide a good estimate of the risk to an area from ozone. Anything more detailed might be over-analyzed with the result that there is no more confidence in those conclusions than those from a benchmark analysis.

## **Chapter 6: Ecosystem Services**

13. To what extent does the Panel find that EPA has adequately characterized the range of ecosystem services that are potentially adversely affected by O3?

It is my feeling that the EPA has appropriately characterized the range of ecosystem services. Instead of re-inventing the wheel they are using the Millennium Ecosystem Assessment of 2009, which more than adequately summarizes the services that ecosystems provide.

14. To what extent does the Panel agree with EPA’s ecosystem services framework, connecting O3 exposure, through ecological effects to ecosystem services?

Yes, I agree with the framework and have no substantive recommendations for change.

15. Does the panel agree with EPA’s use of combined O3 exposure data with other data sources (e.g. fire data, bark beetle maps, trail maps) to link areas of concern or interest with areas of higher vegetative risk due to O3? Does the Panel have any recommendations for additional datasets and ecosystem services that could add to or improve these analyses?

Yes, these are important linkages. Some are tenuous at best, but in the interests of covering all possible linkages, should be included here. The only addition I would consider is some new data that show beetle infested pine forests produce more VOCs than healthy forests, and may actually contribute to additional ozone formation as a result – sort of a vicious feedforward loop. A similar situation has been postulated for areas infested with Kudzu.

16. Regarding the analysis of forest yield impacts:

a) To what extent does the Panel agree that the Forest and Agricultural Sector

Optimization Model (FASOM) model is appropriate to assess timber and crop yield changes and the effects of those changes on additional ecosystem services?

I have to admit not having much expertise with FASOM, so I don't feel qualified to comment on the appropriateness of its use. The write up in this section seemed to make sense to me though.

b) What are the views of the Panel on the extrapolation of concentration-response functions across similar species?

I think it is highly speculative to consider that similar species will necessarily react in the same manner to ozone. Consider black cherry and pin cherry, two closely related species: black cherry is considered one of the most sensitive to ozone, while pin cherry is perhaps more tolerant. However, given that researchers cannot develop ozone response functions for every single species, extrapolation is probably the best we can do at this point in time. Grouping species by certain functionalities, such as early successional vs late, or by those species that are determinate vs indeterminate, may help: e.g., early successional species tend to respond more than late successional species on average, and perhaps those with indeterminate growth can compensate through an ozone season for decreased leaf function whereas determinate ones cannot.

17. Regarding the analysis of urban forest impacts:

a) To what extent does the Panel feel that the i-Tree model is appropriate for assessing changes to urban forest ecosystem services based on O<sub>3</sub> exposure?

Correct me if I am wrong, but wasn't this model developed using mostly southern tree species. Has it been re-parameterized for additional tree species? If so, I would not have a problem. If not, perhaps that should be done.

b) In order to increase the number of tree species covered by the iTree model, does the Panel have recommendations for additional species that could be included, based on estimates from similar species?

I do not have any suggestions for additional species at this time.

18. Regarding the use of PnET-CN:

a) What are the views of the Panel on the potential use of the PnET-CN model in the 2nd draft to assess impacts on larger scale ecosystem services (e.g. hydrologic changes, c sequestration)?

My only question is whether PnET-CN is parameterized well enough to use in forests that differ greatly in structure, species composition, and climate from the ones it was first developed for: i.e., Harvard Forest and Hubbard

Brook. Has it been validated in any other forests aside from these two?

My other concern is whether this model accounts for within canopy variation in ozone deposition and structure? That is, does it have any sort of weighting factors for leaves at different depths in the canopy, or is it a “big-leaf” type of model?

- b) Does the Panel have recommendations of other models that are accessible to EPA that could be used instead of PnET-CN?

The only other one that seems to be getting a lot of publicity lately is DLEM, and the REA noted that it was not practical to use this one. Is there a particular reason or set of reasons why DLEM cannot be used?

19. Regarding ecosystem services related to foliar injury:

- a) To what extent does the Panel agree that potential visible foliar injury is appropriate to use as a metric to assess potential loss of cultural services associated with recreation in national parks?

I don't think there is as clear a relationship between foliar injury and loss of cultural services as implied in the REA. My experience is that most people would not recognize foliar ozone injury unless it was pointed out to them. Further, if there is foliar stipple on large trees, most of those leaves are out of sight of the general public, and hence, not a concern. Are there any studies that demonstrate that people “see” foliar injury, and more importantly, that they “recognize” it to be caused by ozone. And further, do they consider it, once pointed out, a noticeable detriment to their experience in the Park? Without those studies, I am not confident that foliar injury is a really good metric for assessing cultural impacts.

- b) Does the Panel feel that there are O<sub>3</sub> benchmarks that could be used to assess changes in foliar injury potential between exposure scenarios similar to those used by Kohut (2007)?

I don't believe I have enough expertise to answer this particular question at this time.

## Chapter 7: Synthesis

20. 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 analysis?

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Generally, the synthesis is well done. On page 2 though, there is a bullet explaining the biomass losses in Great Smoky Mountains National Park (GRSM), which implies that the magnitude of response is quite large for a given reduction in ambient ozone, yet in the earlier chapter where the Kohut assessments are discussed, there is explicit mention of how GRSM ranks in the low risk category. How do we reconcile these two disparate ratings?

## *Comments from Dr. David Grantz*

### **Responses to Charge Questions--Welfare REA**

#### **Chapter 5: Ecological Effects**

7. Regarding the assessment of relative biomass loss (RBL) for individual species:
- What are the views of the Panel on the use of the linear model forced through the origin to assess the proportional relationship between the relative biomass loss (RBL) values for each species comparing the RBL at recent ambient conditions to the RBL under the scenario modeling O3 just meeting the current standard?

**I think the use of a linear (is it regression or correlation?) analysis is a poor choice because it implies a relationship that has no real basis. The text suggests several alternative approaches, including a mean ratio which would be superior. In the end, the only number retained from the linearization approach is the slope, which is taken as the mean ratio.**

- To what extent does the Panel find that this an appropriate analysis to compare the proportional changes in RBL? Does the Panel have suggestions for alternative approaches that provides a comparable result and maintains the cell-by-cell approach to help control for environmental variability?

**As above, the mean of within-cell ratios is more straightforward.**

- To what extent does the panel agree with the approach used to combine the 11 tree species into one analysis?

**For purposes of risk assessment it is essential to aggregate across species and landscapes as done in this document. The combination in Figure 5-7 appears straightforward and meaningful. The calculation of the statistics in this figure was not clear in the text, but I am assuming this was the distribution of ratios from the linearized plots discussed above.**

8. Regarding the assessment of RBL for combined species:
- To what extent does the Panel support the use of the Importance Values from the U.S. Forest Service to weight the RBL values in extrapolating from individual trees to larger ecosystem level effects?

**I think this is very important, and well realized in the document.**

- What are the views of the Panel on the use of the summed-RBL as a metric to use for assessing effects at the larger ecosystem scale?

**I see this as a first cut at the problem. There are certainly more precise measures that could be devised, but current data limitations restrict their application. At the current time I think this approach is a good place holder for aggregating these types of impacts, and a potential spur to relevant research.**

- Does the panel have any recommendations for methods to include a wider range of tree species (beyond the 8 species included)?

**The limitation to including more species is availability of C-R functions. When existing C-R functions are extrapolated to novel species, it is important to provide a range of possible sensitivities, rather than assume the sensitivity of the most sensitive species. This will provide a range of possible impacts, and is more conservative and more credible when**

**uncertainty is propagated. An exception is if for taxonomic or biochemical reasons one can suggest similarity to the C-R relationship with a known species. The only alternative to the preceding approach is to find existing or create new C-R relationships with new species.**

9. What are the views of the Panel on the use of federally designated Class I and Critical Habitat areas as endpoints for this analysis? Does the Panel have any suggestions for additional parks beyond Rocky Mountain National Park and Sequoia/Kings Canyon National Park that should be analyzed? Does the panel have recommendations for additional or alternative geographic analysis areas that could be used?

**Class I areas by their definition deserve as much scrutiny as possible. They are very appropriate areas for analysis (not sure they are “endpoints”). Critical habitats require much more justification and analysis to justify their inclusion—both as to target species and nature of criticality. This may not be possible in this document.**

**The choice of parks seems excellent, but isn’t Great Smoky Mtn. NP in the analysis now? Acadia would be the most likely next one to add.**

10. To what extent does the Panel find that the vegetation mapping data from USGS and NPS used to generate a scaled-RBL surface in Great Smokey Mountain National Park is appropriate?

**Conceptually very appropriate.**

11. Regarding the screening level foliar injury risk assessment:

- a) To what extent does the Panel find the updated assessment of foliar injury risk in national parks originally performed in Kohut (2007) to be an appropriate screening level risk assessment?

**I think it is limited, but as good as anything now available. It is an efficient way to screen a large number of sites. I think the use of multiple ozone metrics to screen is not useful. For this document I recommend that EPA make a choice (presumably W126) and stick with it throughout the analysis.**

- b) What are the views of the Panel regarding the potential methods for estimating O<sub>3</sub> exposure at additional parks?

**Use of the highest concentration monitor in a park is not very desirable. The original kriging method makes some sense, and the use of the data-model fusion approach suggested here may be better if validated. In future, the impacts of elevation and terrain will be required, and national parks provide a good set of locations to test new approaches.**

- c) What are the Panel’s views regarding the appropriateness of requiring that two criteria must be satisfied (i.e., based on both W126 and N100) in order to receive higher risk ratings?

**Not appropriate. See above.**

- d) Is the Panel aware of any assessments of foliar injury in national parks conducted between 2006 and 2010 that could potentially be used to validate the updated risk ratings?

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No.

12. Regarding the assessment of cover of O<sub>3</sub> sensitive species:

- a) To what extent does the Panel find the preliminary analysis of sensitive species cover to be an appropriate and useful approach to highlight areas of potentially higher risk due to the presence of sensitive species?

**Unclear. Is this Figure 6-8?**

- b) To what extent does the Panel find the vegetation mapping data appropriate to assess the cover of O<sub>3</sub> sensitive species in GSMNP?
- c) What are the views of the Panel on the decision to not distinguish between vegetation strata (i.e. herb, shrub, tree)? To what extent does the Panel agree with this methodology relative to analyzing the strata individually?

**I think there is a high priority in this document to aggregate results for ease of communication to end users. Therefore I grudgingly approve of the use of single stratum analyses, even though they are clearly and demonstrably wrong. Ozone is depleted vertically through canopies, understory species are in dim light but improved water status leading to uncertain and site-specific differences in stomatal conductance to ozone, and the species are different with contrasting leaf morphologies. There is little reason to believe they will respond as do overstory trees.**

- d) What are the views of the panel on using benchmarks, similar to those used in the Kohut analysis of foliar injury risk, to allow estimates of change between exposure scenarios?

**Unclear. Benchmarks or air quality thresholds for injury are a short-hand means of moving types of vegetation between risk classes. If continuous functions can be developed this would be superior, but data may not be sufficient. The specific thresholds (benchmarks) suggested by Bob Kohut may not be invariant on a national scale, whereas a continuous function might be evaluated locally.**

## **Chapter 6: Ecosystem Services**

13. To what extent does the Panel find that EPA has adequately characterized the range of ecosystem services that are potentially adversely affected by O<sub>3</sub>?

**I think this is a pretty good job and consistent with previous analyses.**

14. To what extent does the Panel agree with EPA's ecosystem services framework, connecting O<sub>3</sub> exposure, through ecological effects to ecosystem services?

**The framework is conceptually fine. I found the presentation of Chapter 5 and Chapter 6 to be confusing and somewhat tedious to read. Is it possible to combine, so that the thread between exposure-effects-services is accomplished all at once for each combination of factors?**

15. Does the panel agree with EPA's use of combined O<sub>3</sub> exposure data with other data

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sources (e.g. fire data, bark beetle maps, trail maps) to link areas of concern or interest with areas of higher vegetative risk due to O<sub>3</sub>? Does the Panel have any recommendations for additional datasets and ecosystem services that could add to or improve these analyses?

**I think incorporating anything linked to ozone is appropriate. I disagree with the conclusion that bark beetle damage (monetized) cannot be disaggregated into an ozone-induced component. It may not be quantitative at this point, but it would be a useful placeholder for further analysis. It is not accurate to state that because total damage is included in the bark beetle estimate, that this precludes teasing out the ozone-induced component.**

16. Regarding the analysis of forest yield impacts:

- a) To what extent does the Panel agree that the Forest and Agricultural Sector Optimization Model (FASOM) model is appropriate to assess timber and crop yield changes and the effects of those changes on additional ecosystem services?
- b) What are the views of the Panel on the extrapolation of concentration-response functions across similar species?

**See comments above. I think as much generalization as possible is useful, but with uncertainties propagated through the analysis. Use of similarities among species to assign highest likelihood sensitivities could be useful.**

*[The appendix detailing the FASOM analysis will be submitted for review in August]*

17. Regarding the analysis of urban forest impacts:

- a) To what extent does the Panel feel that the i-Tree model is appropriate for assessing changes to urban forest ecosystem services based on O<sub>3</sub> exposure?
- b) In order to increase the number of tree species covered by the iTree model, does the Panel have recommendations for additional species that could be included, based on estimates from similar species?

*[The appendix detailing the i-Tree analysis will be submitted for review in August]*

18. Regarding the use of PnET-CN:

- a) What are the views of the Panel on the potential use of the PnET-CN model in the 2nd draft to assess impacts on larger scale ecosystem services (e.g. hydrologic changes, c sequestration)?
- b) Does the Panel have recommendations of other models that are accessible to EPA that could be used instead of PnET-CN?

19. Regarding ecosystem services related to foliar injury:

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- a) To what extent does the Panel agree that potential visible foliar injury is appropriate to use as a metric to assess potential loss of cultural services associated with recreation in national parks?

**The types of canopy death attributed to ozone-enhanced bark beetle infestation are clear aesthetic degradations noticeable to visitors. I think visible ozone injury is also negative but perhaps not so much so. It is however, a reasonable endpoint for quantifying loss of cultural services. It has been used for a long time as a surrogate for many ozone impacts, even though the quantitative correlations are known to be only moderate.**

- b) Does the Panel feel that there are O<sub>3</sub> benchmarks that could be used to assess changes in foliar injury potential between exposure scenarios similar to those used by Kohut (2007)?

**See above.**

### **Chapter 7: Synthesis**

20. 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 analysis?

**The synthesis was mercifully brief and to the point. I thought it provided a clear wrap-up for the reader. I would have liked some numbers to appear, in effect highlighting the few quantitative results that were obtained in the document.**

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### ***Comments from Mr. George Allen***

September 3, 2012

Preliminary Response to Charge Questions for Welfare REA, Chapter 4: Air Quality Considerations

#### **4. To what extent does the Panel consider the years of air quality data to be appropriate for use in the exposure and risk assessment?**

The two overlapping 3-year periods of 2006-2008 and 2008-2010 are appropriate. While it is desirable to use the most recent 3-year data period, 2009 and 2010 were relatively clean ozone summers in the eastern US, making 08-10 a period that may not represent current ozone trends. Ozone during the summer of 2012 has reinforced that concept. 2006-2008 is relevant in that it is the most recent 3-year period where GEOS-Chem model run data for background O<sub>3</sub> data are available - an important component of the REA process.

#### **5. What are the views of the Panel on the approach used to develop a national scale surface of W126?**

Section 1.2.3 of the Wells memo describes the data fusion approach using CMAQ and monitoring data to create fused spatial 12x12 km grid surfaces for each of 2006, 2007, and 2008 as well as the 3-year average. This is likely to work reasonably well in the eastern US where the ozone monitoring network is more dense. In the western US, there are large areas with no monitors as well as more complex topography; both of these present challenges for any attempt to model a national ozone surface.

#### **6. Regarding the methods for simulating just meeting the ozone standard:**

##### **a) To what extent does the Panel find that the quadratic rollback approach used in the first draft REA for simulating just meeting the current standard is a reasonable approach?**

With the application of US background as a lower-bound, the quad-rollback is a reasonable approach, but does have limitations inherent in its simplicity and lack of any chemistry.

##### **b) Does the Panel have suggestions for alternative approaches for simulating just meeting the current secondary standard or alternative standards based on the W126 metric?**

Not at this time. The Higher-order Direct Decoupled Method or other possible air quality model based approaches have not yet been evaluated for this metric.

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### ***Comments from Dr. Armistead (Ted) Russell***

Review of Ozone REA-Welfare 1st Draft.

Executive and Integrative Summary: I hope both are coming. Also, parts of the Welfare REA suggest that different, or extended, analyses will be done for the next draft. It would have been nice for these to be mentioned up front. The last paragraph of the synthesis actually does this well... though should have been more detailed in the synthesis chapter.

Chapter 1:

The Minor issues:

1-

Chapter 2:

Overall,. The Conceptual Framework should provide more discussion on how the risks are determined, including discussions about assessing current risk, just meeting the standard, and assessment of the risks from meeting alternative standards. At present, it does not lay out the conceptual part of conducting a risk and exposure assessment (e.g., methods and tools and how the results would be analyzed). This is done, but in too much detail, in the SCOPE.

The Minor issues (similar to Health REA):

2-1,. L4 remove “,”

2-2, 117 (and elsewhere): Use chemical subscripts correctly

2-3, 14 Use of “valleys” here can be misleading (some will think topographically). Change to ... local decreases where ozone...”

L6: ..., and the NO<sub>2</sub> formed can lead to O<sub>3</sub> formation...” (it can form HNO<sub>3</sub> as well)

L8: Likewise, don't use “valleys” in this context.

2-3, 124 replace relatively insensitive with less sensitive., and “both” with “either”

2-5, 17: Do you mean intrusions?

2-6 need a space in “Below ground” in the figure.

2-8 “e”? (At the end of the sentence.)

2-14, 113: You bring up in the conclusions that “air quality models ... show that important ecosystems...” If this is part of the conclusion, those should be discussed somewhat more in the chapter along with what has been shown.

Chapter 3:

3-8, 16 Maybe replace extrapolate with estimate?

3-8, 123-25: What alternatives? Where are they described?

Chapter 4:

For what is there, Chapter 4 provides a reasonably clear discussion of the air quality considerations involved in the Welfare REA (WREA). However, it ends abruptly, and how you would deal with meeting alternative standards is not addressed (if that might ever be done... if not, you might actually explain why the difference with the Health REA). If the plan is to evaluate W126-based standards, doing a national analysis where the ozone is adjusted to meet a specific W126 level would have been instructive. The chapter does not do a good job of describing the types of future analyses being contemplated, including what alternative types of rollback approaches are planned or which types of standards to be evaluated are envisioned.

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1. Question 4: Appropriateness of years of air quality: Fine choice. You want to avoid 2009 as an abnormally low year in parts of the country.
2. Question 5: Approach used to develop W126 surface: Fusing CMAQ results with observations is good. Looking at the three year average, and the individual years, is good. You should more clearly explain how this works in to the “just meeting the standard” scenario.
3. Question 6. Methods for simulating just meeting the standard
  - a. Quadratic rollback approach: A problem with the quadratic roll-back (or linear or Weibull, I think) methods are that they do not allow for lower levels to actually increase in response to controls, and a few other issues are of potential consideration as well. Indeed, the health REA proposes using an air quality model-based method (HDDM) to quantify how levels will go down. Further, only having the monitors experience exceedences go down is also problematic, with a specific example captured in the text (if two sites are close, and one is exceeding, only that monitor is reduced: page 4-7).
  - b. Alternative approaches: Given that a more comprehensive approach (CMAQ with HDDM) is being used in the health REA, its use should be examined for application here. You could use the HDDM sensitivities to reduce ozone at the specific monitors, which are then applied in MATS.

Minor Comments:

4-4, 113 “... sum the weighted hourly...”

4-5 13: You should explain, at least to some extent, what the Kohut analysis is, and how it will be used, before saying “For the specific application of the Kohut analysis...” Further down the page it is then mentioned the Kohut analysis will be described “in more detail” in chapter 5. Up to this point, there has been no detail. I would give a few sentences at the beginning of this paragraph, or possibly even on page 4-3, about the Kohut analysis.

Chapter 7

Not a bad start at all. One comment from above: a more detailed discussion of the different analyses to be done as part of the second draft would have been useful.