03-23-14 Preliminary Draft Comments from Clean Air Scientific Advisory Committee (CASAC) Ozone Review Panel on the Health 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 Do not cite or quote.

Additional Preliminary Individual Comments on the Health Risk and Exposure Assessment

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Review of Ozone REA-Health 2nd Draft.

This REA is a marked improvement over the prior Draft, and over the REAs from years past. It shows a very positive evolution in the approach and the presentation. There can still be some improvements, but it has come a long way from the first one I read during the last review. It also represents a tremendous amount of work. The work that has gone in to the air quality characterization and the development of ozone changes in response to emissions controls as described in Appendix 4 is a remarkable amount of effort. While I might have done a few things a bit differently, and likewise interpreted some of the results a bit differently, that could be from my ignorance and not having spent so much time trying to pull off what was done.

From an air quality characterization and analysis standpoint, my view is that the largest step forward in their analysis is their ability to capture ozone responses to emissions controls using an advanced air quality model-based approach, e.g., using CMAQ with HDDM. This has allowed EPA to capture not only the reductions in peak ozone, but also the increases in lower level ozone levels in response to controls. Both of these have been observed, and this gives much greater confidence to their ensuing analyses. One concern was that too much of the important concepts from the Appendix that supports their approach is left in Appendix 4. It is very necessary to read Appendix 4 to have a reasonable view what is being done, and what are the particular strengths and weaknesses (Appendix 4, discussed below, however, is still a bit rough and could use some work). In this REA, they have also added additional ozone response models.

A major general concern was that the figures/figure captions were often not complete and/or clear. It took me a while to see what was being presented, and I often had to go back and forth between the figure and the text to figure out what was being shown. Make each figure/table almost stand alone, i.e., it could be a single slide in a presentation and need little explanation. The more complex figures, in particular, need a more informative caption.

Chapter 2:

Overall, the chapter adequately conveys the first parts of the conceptual framework for conducting a risk and exposure assessment for ozone.

The end of section 2.2.1 should be modified to note that the NOx-limited conditions are found in the summer/high ozone levels. Much of the year, cities can be radical limited due to the lack of sunlight. You may want to characterize these areas as being where “high ozone levels are NOx-limited”.

Chapter 4:

1. Question 4. The use of HDDM-based adjustment is a major step forward. Reading this chapter and the supporting Appendix demonstrate a considerable amount of work, thought and analysis. It also provides results that are much more in line with observations. Kudos to the staff in pulling this off.

There are some concerns, however. First, many important aspects of the method are relegated to Appendix 4, which I can both justify (it gets very technical and is likely of limited interest to many), but also criticize (there are rather important outcomes of using the method and how specific approaches to using the sensitivities). It is appreciated that the staff was able to develop, and have reviewed, a manuscript describing much of their approach, but there are some differences.

One comment that should be made is that, at present, their approach shows some bias in that they primarily utilize a NOx-oriented control approach (e.g., they prefer using a NOx-only set of sensitivities, not the NOx-VOC results, and do not even provide VOC-only approaches). This should be further discussed and defended, e.g., potentially a few analyses showing that a VOC-only approach is largely ineffective in most locations and/or that a VOC-only approach buys little benefit over a NOx-VOC approach. However, I do support the use of a limited number of non-source specific sensitivities as there are a huge number of source-specific analyses that could be done, the choice of which is not apparent at this time. How close would a VOC-only strategy get New York to the 60 ppb level?

A comment between this chapter and Appendix 4 is that I probably would not have chosen the same approach to estimating sensitivities and ozone levels at intermediate control levels, i.e., when control levels are not 50% or 75%. The current approach appears a bit ad hoc, and shows a few major deviations (though limited, and they have an adjustment approach). I might have done the maximum simulations at, say, 85% controls (more towards the extreme end of the controls) and used a cubic spline fit to provide sensitivities at intermediate levels. The spline could provide each of the first and second order sensitivities. Something to think about next time.

The next question I had was exactly how the sensitivities are being applied at each location in the domain. Are they being applied to the CMAQ-simulated value, or (I think) to the VNA/DS-derived value? Figures 3-1 and 4-6 (and Figure 1 in Appendix 4-3) should be edited to make this clear. Fig. 4-6 should explicitly show how VNA or DS is being used. Both might show how/where VNA and DS are being used in the process. Having calculated the location/time specific ozone value, the next question is how to develop the appropriate sensitivity. Should one use the sensitivities calculated directly as described in Appendix 4 specific to the simulated ozone value, or should those sensitivities be adjusted for the difference between the simulated and observed ozone levels. If the base simulated ozone is 80 ppb (and, thus the sensitivities are consistent with that simulated value), and the observed value is 100 ppb, should the sensitivities

be adjusted upwards? One could give reasons both ways, and this should be discussed as well as support for their choice. I would probably adjust, but it is a tough call. (There are other approaches one might consider as well.) I suspect this would make rather little difference, but it should be discussed.

Section 4-5. Section 4-5 is comprehensive and I generally agree with their assessments of the levels of uncertainties, though with a few exceptions. The uncertainty in CMAQ modeling is probably "medium" based upon the model evaluation (which was very extensive). Likewise, the HDDM sensitivities are likewise about medium given that CMAQ results are about medium, and that they also have undergone less extensive review and you cannot directly evaluate the sensitivities using observations. Further, it is not apparent the best way to scale sensitivities when the simulated observation does not match the observations. On page 4-47, it is stated that "… in general we expect that the that the benefits of reducing high ozone concentrations and disbenefits of increasing low ozone would be underestimated." This should be further explained and supported.

Chapter 4 needs a Summary/Key Observations section consistent with the other chapters.

Chapter 6

I am still not wild about how Eq. 6-2 is shown. The assumption is that C and V do not change over the time period, so they are not really a function of t, which is what is shown. Showing that they are varying along with X is inappropriate. One could just as well use t_0 or t_i, indicating that the choice is for time period I, or show that they choose an average over the time period (use a bar over the term).

The uncertainty discussion in this chapter also needs a bit of work. It is noted that the uncertainties in the MSS model parameters are likely larger than 5%, but with little more discussion. Then, Fig. 6-12 uses 5%. Thus, the uncertainties shown are likely greater than shown. This should be noted in the caption. Also, I do not believe that 6-12 should be labeled as elasticities. Elasticity has a specific definition. Those are responses to a 5% increase (as the caption notes). I think an elasticity would be 20 times what is shown (if you still use %, but I might use a fractional elasticity).

Chapter 9

Question 19. Certainly the Synthesis is useful, and the document would suffer without it. It could be more substantial.

Question 20. The discussion of uncertainty does provide a good context for interpretation of the exposure and risk results. However, I was hoping that the uncertainty discussion would be

deeper and more definitive. In particular, it would be useful if the section (9.5) concluded with more direct statements as to how a reader should interpret the overall uncertainties in the risk and exposure assessments for use in standard setting. They could also identify the specific uncertainties that are most key (e.g., contribute the most to their overall confidence in the results) and that should be targeted for further reduction.

With those two answers in mind, while the synthesis is valuable and insightful, it is not without additional problems.

Chapter 9 should deal more with synthesizing the results from the application of various responses over different seasons and different levels. While some analyses dealt with ozone during the warm seasons and only higher exposure levels, others were over the whole range. The discussions that are present are a good start. However, to state "The implications of this is that our estimates of mortality and morbidity risk reductions… are likely to understate…" should be qualified in that the seasonal application can add bias in the other direction.

Page 9-43: l 10-19. This paragraph says things are different and it is important to understand the differences, but does not provide how they are different and what that means. The next paragraph does similarly. It would be good if both of the paragraphs were more informative as to what "different" really entails. Use of different metrics will lead to different results, but are they meaningful differences? Do they conflict? Are they problematic when the results are interpreted for use in standard setting, e./g., do they raise concerns about uncertainties, or are they consistent and support the use of the metrics? In general, the synthesis could be more definitive.

9-23, l 2: It is not just one reaction that is of importance. The consumption of radicals also reduces ozone.

9-38, l3-5. Does this statement agree with the analysis found in the ISA? Please link to the ISA.

Appendix 4

Appendix 4 (particularly 4-D) represents a huge amount of work and a major step forward. It also needs a fair amount of work to harmonize the chapters. In particular, the figure numbering and equation numbering should be more specific as to the specific appendix. One might even think about an Introduction to the Appendices overall. Certainly, there needs to be an overall Table of Contents for the Chapter 4 appendices either up front and/or in the HREA Table of Contents.

The evaluation is extensive.

In Appendix 4-D, Section 3.2.3 could be a bit more clear in what is being done to modify observed concentrations. Sections 4-5 and 4-6 could use their own flow diagram specific to that component of the analysis, and with more detail.

I might recommend EPA having a more extensive discussion with the modeling community about how to use sensitivities in adjusting ozone values to meet various air quality metrics. There was not time this time.

Helen Suh

Charge Questions for Chapter 5: Characterization of Human Exposure to Ozone

General Comments

The Chapter was generally well written, well organized, and comprehensive, representing a significant improvement over the previous draft. Its presentation of the REA goals and background on the APEX model were very useful, as they helped to frame the discussion of the model outputs and results.

In addition, the Chapter's addition of the targeted evaluation of the quality and relevance of the model inputs was terrific and should be expanded to include an evaluation of the 2000 US Census data and its relevance to the 2006-2010 study period. It is possible that this evaluation could be made from a comparison of 2000 and 2010 US Census data that examines whether and how the number and spatial distribution of the four at-risk study populations have or have not changed. This comparison may help to further characterize uncertainty resulting from population distributions.

6. *To what extent does the Panel find the assessment, interpretation, and presentation of the methods and results of the updated and expanded population-based exposure analysis to be technically sound, appropriately balanced, and clearly communicated?*

Presentations of the methods and results sections were technically sound and for the most part clear. The section describing the model output, however, was often difficult to follow and seemingly overcomplicated. Specific comments and suggestions include:

- A column should be added to Table 5-3 to indicate the number of subjects included in each study. Also, if an activity pattern study did not include data for all microenvironments, it should be noted (either as a footnote if rare or as a separate column if more common).
- The definition of benchmark level should be defined when it first appears.
- As mentioned starting on page 5-20, the discussion of temporal and spatial variability in exposures for each of the five air quality scenarios is complex. This complexity may require the results to be presented in multiple figures, rather than in condensed or summary figures, as represented in Figure 5-3 and 5-4. The summary figures are too confusing and require too much explanation in the text. Further, this explanation in the text is very confusing.
- The figures should be careful to display information consistently. For example, in Figure 5-3 the bottom row of graphs should be presented from left to right for 60, 65, 70, and 75 standard levels to match the order in the top row of boxplots. Perhaps this is to match the column order for Figures 5.5-5.9, for which standard levels are also presented in columns that move left to right from higher to lower standards. It seems contrary to convention to present them this way. Is there a reason for this?
- All results are presented as the percent of the at-risk group. Should the absolute number be presented as well for the city comparisons (e.g., 5-7 or 5-8) , since is possible that the percent for a given city is small but the number is high relative to another city.

7. Chapter 5 includes several evaluations of key APEX inputs and model outputs, including for example analysis of time-activity data and comparison of actual personal exposures with modeled exposures. What

are the views of the Panel on the appropriateness and usefulness of these evaluations and the conclusions drawn from these evaluations?

The addition of evaluations of key APEX inputs and model outputs was appropriate, useful, and extremely important to demonstrate the validity/relevance of the exposure analysis and to address issues related to uncertainty in the model outputs. A factor that is not considered but should be at least discussed is whether activity pattern data should be linked to simulated individuals based not only on age, sex, day-of-week, and ambient temperature, but also on geographic variability, as it seems likely that activity patterns differ by geographic regions. Relatively minor suggestions include:

- In addition to the number of diary days, how many people were included in the database? By geographic region?
- Figure 5-10 needs additional explanation in the text and in its label. To what do the 1, 2, and 3 groupings refer?

8. Chapter 5 includes several scenario-based exposure simulations that focus on specific populations or behaviors. What are the views of the Panel on the design, results, and interpretation of these additional scenario-based exposure simulations?

The additional scenario-based exposure simulations were useful, clearly explained, and an important addition to the analysis.

For these analyses, however, it would be helpful to take into account or discuss whether the amount of time spent outdoors varies by geographic location, for example may be higher in Los Angeles or Houston as compared to New York City. The impact of geographic variability may be important for these scenario-based exposures, since they calculate percent

of Detroit (and in some cases Atlanta and Philadelphia) populations above benchmarks but do so using adjusted activity diary pools from the entire country. This geographic variability may explain differences in the personal exposure comparisons for DEARS participants.

9. *To what extent does the Panel find that the discussion of uncertainty and variability has covered important sources of uncertainty and variability and appropriately characterized their relationship to the exposure estimates?*

Most sources of uncertainty have been characterized as low or low-to-moderate, with a few sources characterized as moderate. While reasons for this categorization are provided, the categorization seems to underestimate uncertainty and to give the false impression that uncertainty in the exposure results is also low or low-to-moderate. This is notable given the fact that comparison of simulated exposures to measured exposures in Detroit showed systematic bias in the simulated exposures. Further, uncertainty characterization was defined based on available data and did not consider data gaps. For example, data on activity patterns and home air exchange rates for individuals of low socioeconomic status (SES) were generally not included in exposure simulations. These data gaps create uncertainty in the risk estimates, especially in what may be an important at-risk group.

Charge Questions for Chapter 9: 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 health exposure and risk assessment?

The Chapter was generally well-written and is comprehensive. Its summary of findings (Section 9.2) from earlier chapters although clear, was too long, repeating each of the findings and reproducing too many figures. Section 9.2 should be condensed to focus only on those findings relevant to the synthesis of findings, which begins with Section 9.3. Section 9.3 is a good start to synthesizing findings across the exposure measure and the three health outcomes. It, however, was confusing at times and/or lacked sufficient explanation. For example, on Page 9-35, the REA states "Additional considerations are important in interpreting the reduction in exposure and risk between the existing standard and alternative standard." However, the impact of these additional considerations (e.g., consideration of peak ozone, US-only reduction in emissions) on the findings was not discussed.

20. To what extent does the Panel find that the discussion of overall uncertainty provides an appropriate context for interpretation of the exposure and risk results?

As discussed in the Chapter, the assessment of the overall impact of uncertainty on the analysis is difficult. Further, the contribution of each source of uncertainty on the overall findings is similarly difficult. Likely as a

result of these difficulties, the discussion of uncertainty is largely a repeat of the discussion in earlier sections, with minimal synthesis. The discussion of overall uncertainty would have been improved substantially with identification of sources of uncertainty that were likely the largest contributors to uncertainty in the risk results and from a discussion of data gaps.

Minor Comments:

- Figure 9-1 legend should read “HDDM” rather than “ddm”.
- The section on Page 9-28 that begins “All of the metrics show substantial variability among urban case study areas…” should be revised to clarify that the discussed variability of interest is the between-city variability.