Responses to CASAC Questions on the Draft Ozone PA from Consultant Mr. John J. Jansen

Given the narrow range of the questions posed by the CASAC members, I read only portions of the draft Ozone Policy Assessment (Chapters 1, 2, 3, Appendix 3C, & portions of Appendix 3D). I have responded to most of the CASAC member questions and also offer some general comments.

General Comments

For the most part I will not repeat comments made on the draft ISA but many of them apply to this document as well. For example, on page 2-3, implying mobile NOx is largely responsible for decreasing NOx emissions ignores substantial reductions in EGU NOx.

I continue to be concerned over the lack of a quantitative integrated uncertainty analysis (IUA) both in individual sections as well as overall for the risk numbers presented. Instead, EPA conducts a qualitative analysis by characterizing "the magnitude and direction of the influence on the assessment for each of these identified sources of uncertainty" page 3D-135 (see also pages 3-64 and 3D-87). The document mentions this qualitative approach several times in the document without any "results" being presented nor a reference to where they are (i.e., Table 3D-61 on page 3D-136 (referred to, confusingly, as Table 6-3 in the text)). I am at a loss as to how to use the table to determine if the risk estimated for various scenarios are different from each other. I recognize the task is difficult and getting more so as the tools become more complex (e.g., the APEX, HDDM-CAMx, and Voronoi models). Nevertheless, quantitative uncertainty estimates can be estimated for most if not all steps in Table 3D-61 and IUA methods are available, and have been applied to support comments on past NAAQS reviews. See references provided in ISA comments.

I agree with the preference for the human exposure studies over the epidemiological studies in terms of having more confidence in their use. This stands in stark contrast to the approach used by EPA in the PM PA. As I stated in my comments on the ISA as it pertains to causality characterization, "quality human and animal experimental studies at relevant exposures need to be weighted over suggestive epidemiological (associational) studies to establish causality."

Questions from Dr. Masuca

Questions 1: The science on stratospheric tropospheric exchange of ozone is well known and discussed briefly in section 2.5.1.1 on page 2-27. While it can lead to a general increase in background ozone, the question is whether and how much it contributes to ozone NAAQS exceedances. The magnitude, frequency, and timing of such events become important. The effect is more pronounced and observable in high elevation western monitors. Much of this is discussed reasonably well in the rest of section 3.5. Since exceptional events are excluded from the risk analysis, it seems there should be more discussion of the exceptional events policy and the difficulty in making such demonstrations to EPA.

Question 2: All of the networks mentioned, including the road-side monitors provide important data for model performance evaluation and development. The road-side monitors should be mentioned in this regard. I would note that the 3 summer months operating schedule for PAMS sites needs to be re-evaluated. Peak ozone is occurring outside this three months more frequently and data on other seasons is needed.

Question 3: I am assuming you are referring to what is used in the health studies. Although other exposure periods have been used, the human exposure studies summarized in the PA use 6.6 hour exposures. The epidemiological studies have used a variety of averaging time from 1 hour to annual average, including a max daily 8 hour.

Question 4: The diurnal patterns are driven by the relative magnitude of production and loss processes and the relative magnitude is variable across urban, rural, coastal, and elevation locations and time of day. Production is influenced by sunlight, temperature, humidity, etc. Losses include deposition (enhanced under a nocturnal boundary layer) and destruction through fresh NO emissions. While not comprehensive, this section describes the issue adequately.

Question 5: There is always locally generated and transported ozone from various distances. As mentioned above, the issue is how much background ozone contributes to ozone NAAQS exceedances. The magnitude, frequency, and timing of such events become important. The effect is more pronounced and observable in high elevation western monitors.

US background ozone is a term of art and, as such, is virtually impossible to measure. Even sophisticated monitoring using filtering is problematic from a source oriented point of view (see discussion on page 2-33). Models are uncertain but do track specific sources making them more amenable to a definition of what is included and excluded from US background and can do so for all locations. That said, which method is more accurate is not demonstrated in the document. An uncertainty analysis of the models is possible but explicitly excluded (see page 2-38). I find this paragraph completely inadequate, especially relying of a seasonal mean uncertainty of =/- 10 ppb when the risks estimates rely of hourly concentrations at specific locations. Uncertainty estimates need to be estimated in the context of the intended use of data or model results.

I believe the discussion on international emissions is reasonable and both anthropogenic and natural sources are discussed. All natural sources both foreign and domestic are included in USB. Only foreign anthropogenic are included. All US anthropogenic sources are the other side of the coin and this includes interstate transport (or other US state contributions to a given site).
The discussion of methane is warranted as it contributes to USB. As it is well mixed, its contribution is not very variable in space or time.

<u>Questions from Dr. Boylan</u>

Section 2.1: This section seem to be accurate and complete.

Section 2.2: See comments on same subject on ISA.

Section 2.3: See my response to Dr. Masuca's question 2 above.

Section 2.4: See my response to Dr. Masuca's question 4 above.

Section 2.5: See my response to Dr. Masuca's question 5 above. In addition, I continue to be concerned that model performance evaluation is less than robust. See my comments on the PM PA. EPA uses the old justification that performance is in line with the published literature (see page 2-37). The purpose of model performance in the literature tends to be different than the context of regulatory development. I

realize this ship has sailed but it is still bothersome. Maybe if we ever get serious in conducting a true IUA, air quality model performance could be conducted in the context of driving the risk assessment. For example, how likely is it that an estimated exposure to one day above the benchmark concentration would actually be exposed to 2 or 4 days, or the reverse, assuming the activity patterns for the population are perfect?

Section 3.4.1: I have several concerns with this section. On page 3-48, the criteria for selecting the 8 areas are vague and not quantified. What are "exposure variation" and "population exposure conditions?" How do the eight selected areas vary in these parameters? Both definition and a summary table are needed. There needs to be a concise, simple summary (with examples) on how the ozone concentrations for the micro-environments are derived (page 3-49). I searched and found more detail on page 3D-56. The modeling is quite complex and data intensive. The sheer number of scenarios to be calculated seems quite burdensome and begs the question how accurate these are. What are the uncertainties? Finally, it is not clear how the benchmark concentrations (from the human exposure studies) and dose response relationships (from the epidemiological studies) were derived. This should be included.

Section 3.4.2: Since this is not a national assessment, like the previous section, more information is needed to understand the "diversity" represented by the 8 areas (see page 3-56).

Section 3.4.3: The messages in this section is confusing. Contrast the statements on page 3-61 with the summary paragraph on page 3-62. The latter is consistent with the message regarding the just meeting the current standard on page 3-57. EPA should eliminate the modifiers (e.g., markedly) on page 3-61.

Section 3.4.4: See my comments above on the need for a more quantitative integrated uncertainty analysis. The first paragraph describes a qualitative approach but does not direct the reader to where the parameters are summarized. The discussion on page 3-65 implies the adjustments to just meet the various levels is more certain but this is not demonstrated. It needs to be. Similarly on page 3-66 the statement "expected to more realistically estimate activity-specific energy expenditure" needs to be demonstrated. Similarly the last paragraph on page 3-68.

Section 3.4.5: No comments.

Section 3C.2: This section is even more meager than what is in section 3.4.1 (see above) and needs to be expanded to more completely justify the areas chosen and characterize their "exposure variations" and "population exposure conditions."

Section 3C.3: Page 3C-22 states all monitors were used whether they met data completeness or not. The rationale is based on Appendix U allowing nonattainment designation based on a monitor not meeting data completeness. While I can understand the Appendix U decision, I am not sure it justifies the use of that site for these purposes (i.e., APEX modeling). It would help if the method for filling in missing data were described.

Sections 3C.4.1 & 3C.4.2: On page 3C-23 EPA states "Differences in predicted O3 concentrations between the CAMx-HDDM configuration described here and a standard CAMx v6.5 simulation with full treatment of aerosol-O3 interactions did not influence O3 predictions in the urban study areas examined in this assessment." This implies that the sensitivities of the version used in the analysis were also not influenced. First, what does not influenced mean? Identical concentrations in every place and

hour or something less rigorous? Second, was a comparison made of the sensitivities derived from both models (I recognize the CAMx v6.5 was probably not run in HDDM mode)? If not, I am not sure I would agree with their implication. EPA should demonstrate that the sensitivities were unaffected by the lack of tracking aerosol and cloud processing on the ozone sensitivities.

EPA did not included agricultural NOx but did include agricultural ammonia (see page C3C-27). For an ozone assessment this seems odd. And yet Table 3C-4 shows an entry for agricultural fire NOx but nothing for agricultural soil NOx. An explanation is needed.

Again model performance evaluation is not very robust and is much to aggregated. The data used in the risk assessment (the APEX model) is hour and location specific. Only regional/seasonal statistics are presented. How well does thee model do in a specific study area, at individual monitors, across gradients in a given hour or day? Model performance in the context of its use is needed. How does the performance affect exposure estimates?

Statements such as "reasonably captured general patterns of O3 transport within the northern Hemisphere" (page 3C-28) and "generally reproduce patterns of observed O3" (see page 3C-29) are subjective and should be backed up with quantitative information.

Section 3C.5: No comments.

Section 3C.6: Why was Voronoi Neighbor Averaging chosen over other methods? A rationale should be given and its uncertainty quantified.

Section 3C.7: No comments.

Questions from Dr. Lange

Question 1: While there may be exceptions, I would expect any changes in the annual averages to be small and could go in either direction. One question I would ask is what the epidemiological studies do when the monitors do not operate for the full year, which is the case of most monitors.

Question 2: I am not a statistician but I do not see how it could "protect against" confounding etc. Confounding exists or it doesn't. If one tests for confounding then maybe the higher statistical power allows it to be demonstrated more reliably.

Question 3: Yes, I believe those statements to be correct. I believe the statements are generally true and the caveat should apply generally, not to just ozone. I suspect the reason it is highlighted here in the ozone proceeding is because ozone concentrations may be more variable than, say, PM among micro-environments. Exposure is very dependent on the integrated levels of ozone in those micro-environments, thus the use of the highly complex and data intensive APEX model. That said, it is not clear that why similar efforts are not done for PM and the other NAAQS. Studies have shown differences in PM and their species between the ambient and homes, restaurants, groceries, etc. In many cases PM is higher indoors due to numerous sources (e.g., cooking, dust, pet dander). Note that indoor sources of ozone (e.g., air purifiers) were explicitly excluded in this assessment. I find it curious that EPA expends so much effort with APEX on ozone and not PM. Finally, the whole APEX discussion

implies but does not demonstrate that the complexities added to APEX result in a more accurate exposure estimate.

Questions 4 & 5: I do not have the expertise to address these questions.