Dear Administrator Jackson,

POIROT:
The CASAC PM Panel was generally impressed with the quality and clarity of the second draft UFVA. EPA staff was responsive to previous PM Panel recommendations, adding a detailed logit analysis to better understand similarities and differences in the urban visibility preference studies, recalculating estimated extinction levels using a 90% RH screen, and considering (90th, 95th and 98th) percentiles based on all available daylight hours as well as based on the single worst daylight hour in each day.

The UFVA presents logical and persuasive arguments for use of a PM light extinction indicator, with a 1-hour (daylight) averaging time as a sound basis for a secondary PM standard to protect visibility. The UFVA also presents and evaluates reasonable ranges for the level (20 to 30 deciviews) and form (90th to 98th percentile) that such a standard might take, and clearly illustrates the differences among the many resulting optional combinations of levels and forms in terms of the protectiveness and/or stringency across the 15 diverse urban areas selected for this analysis.

The PM Panel recommends relatively minor revisions in several sections, but does not see a need for major revisions in any sections of the UFVA. The approaches employed here, the ranges of options considered, and the estimates of visual air quality resulting from the many, various combinations of options considered should provide a sound basis for more detailed consideration and refinement in the Policy Assessment.

CASAC Responses to Charge Questions

1) In general, what are the Panel's views on the level of detail provided in the body of the report and associated appendices? Does the Panel agree that all of this information is useful to retain or is there material that the Panel would recommend deleting? Does the Panel have any suggestions regarding the organization and distribution of information throughout the document and in the appendices?

ASHBAUGH:
While a substantial level of detail has been provided in the UFVA, many of the calculations included here are very complex, and a fairly wide range of optional combinations of levels and forms are presented and compared. The report is long, but its completeness and detail are useful particularly given that we are looking at a very different indicator for this secondary standard. The discussion is clear and the associated appendices are valuable for examining questions raised by the main text. The PM Panel would not recommend deleting any of the material. With the few exceptions noted below, the organization is good as it stands, both in the main body of the report and in the appendices.

Chapter 2 may contain more information than is required with respect to the individual cities. Much of this material could be moved to the Appendices. The memo of 2/3/2010 on the statistical analysis of the preference studies could also be placed in the Appendices, and consolidated with related material from Chapter 2 to avoid redundancy. Similarly, much of the material in sections 4.1.3 and 4.1.4 might be better suited to an Appendix. In any case, the transition into future monitoring site characteristics (middle of page 4-2) is rather abrupt.

There are some editorial changes that could be made to make the document more readable. The Table of Contents is hard to read with the mix of all caps and mixed case font that is currently used. The panel suggests making Level 1 all caps, Level 2 small caps or mixed case, and Level 3 mixed case. This would more effectively set off the major and minor sections of the document in the Table of Contents. Alternatively, the format used in the Risk Assessment would be good. The appendices are OK as they are.

In the list of acronyms/abbreviations, the description for IMPROVE should have an "s" at the end, i.e. "Interagency Monitoring of Protected Visual Environments". The description of NARSTO is out of date. It was originally the North American Research Strategy for Tropospheric Ozone, but is now just NARSTO. This could be noted in the description. For SMOKE, "Kernel" is misspelled.

The definition of haziness in deciview units and light extinction in inverse mega meters on page 1-6 is useful and well presented. However, visual range is used later in the document (first appearing on page 1-10) but is not defined here. Explanation

of the utility of each parameter would help the narrative. While visual range may be most intuitive to many readers, it is dropped on page 1-15, in favor of explicit definition of CPLs in terms of light extinction.

The text at the top of page 3-19 is an excellent example of describing how decision making (here regarding reconstitution of hourly PM2.5 data) was undertaken. Similarly on page 1-16, line 21-23, a rational argument is presented for a similar decision. In contrast, on page 1-16, lines 15-17, a decision regarding the acceptability of poor visibility is based on the "beliefs of staff". This is rather loose wording that should be revised; along the lines of these other examples, if possible.
It is disconcerting to find Alaska, Hawaii, and Puerto Rico lumped into the Gulf of Mexico in the maps on page 3-2. There may be room off the Pacific and Atlantic coasts, respectively, if the legend were placed in the Gulf.
The panel is suspicious of the extremely high coarse mass concentrations that contribute to the poorest visibility days at St. Louis and Los Angeles, and recommends using such data cautiously.

The SANDWICH method described in Chapter 3 does not include sea salt, but sea salt is an important contributor to PM at coastal sites. This may be a source of uncertainty in the light extinction calculation at these sites. The SANDWICH approach has other sources of uncertainty that should be mentioned. Relying on mass closure to determine the hourly organic carbon assumes that all other species are accurately determined. This is problematic for nitrate, as pointed out just a few paragraphs earlier, as well as for sea salt. These factors should be mentioned as areas of uncertainty that are introduced by this method.

Setting negative values to zero, as described on page 3-26, introduces a bias in the results. There is inherent uncertainty in both PM10 and PM2.5 measurements. During periods of very low coarse mass, the PM10 and PM2.5 measurements are very close, and their uncertainties may cause the PM2.5 measurement to be higher than the PM10 measurement. It's important to retain negative values as a measure of the uncertainty during low coarse mass periods.

2) In the Panel's view, to what extent does the logit analysis presented in Chapter 2 of the second draft Visibility Assessment and further expanded and described in the supplemental memorandum add value to the urban visibility preference study analysis and provide additional support for combining and comparing the results from the four cities, as shown in Figure 2-16? What are the Panel's views on the clarity and adequacy of the descriptions associated with such a combined assessment and on the conclusions that can be drawn from the assessments? Please provide comments on any additional insights that might be drawn from these analyses or on any additional caveats that should be considered.

FREY:

The analysis presented in Section 2.6 and summarized in Figure 2-16 is quite reasonable. The characteristics of the studies that produced the data are discussed. The modeling approach is clearly presented and the explanation is very good. Figure 2-16 is very useful for comparing the four realizations of the model in comparison to the study data. Table 2-3 provides valuable information regarding the statistical significance of each model coefficient. The results in Table 2-3 are appropriately explained. The model is appropriately critiqued in terms of goodness-of-fit, statistical significance of the coefficients, and the interpretation of the coefficients. The limitations of the model with respect to limitations of the underlying data are appropriately discussed.

The supporting memo by Stratus Consulting evaluates several alternative specifications of a logit model, of which the one presented in the assessment is Model 2. Confidence interval on the predicted mean were estimated using a numerical method, and verified with another numerical method. Models 1 and 2 are generally found to provide similar results. Thus, the results of the assessment are not substantially significant to the choice in functional form between these two models. The mean 50% criteria results were found to be significantly different among the four cities.

Overall, the use of the logit model is reasonable, since the output is a binary variable. The model is appropriately evaluated and reasonable represents the data to which it was fit. The limitations of the underlying data are discussed qualitatively. The overall findings are reasonably supported by the data, model, and appropriately take into account limitations of the data.

On p 2-30 it is suggested that additional studies would be useful. Does EPA have any plans or intent to conduct such studies?

Is a shift in the preference for light extinction from locations with long distances to views (e.g., Denver) versus those with short distanced (e.g., Washington, DC) of significance? Could different preferences for visibility with respect to distance be a factor in explaining inter-city variability?

HELBLE:
The logit analysis described in Section 2 and in the Stratus Consulting memorandum adds considerable value to the discussion. The text in Chapter 2 is well-written, clearly presented, and at an appropriate level of detail given that this approach has not been previously described in EPA visibility documents. The additional information presented in the memorandum should be retained as an attachment or in an appendix.

The charge question asks whether there are any additional caveats that should be considered. The text does a good job of identifying the limitations associated with the sample population in the UBC study (all participants were university students and therefore had advanced educational background), and discusses the broader

socioeconomic distribution of the population participating in the Phoenix study. Absent is a comparable discussion of the demographic distribution in the second Washington DC study, other than noting the relatively small size of the sample population. Of note is the observation that all participants in the two subpopulations were employees of the research firm conducting the study. It is therefore likely that all participants were well educated, and may have understood the methods and goals of the study better than the populations participating in the other studies. Since this is a preference study, this point is worth acknowledging in the text.

3) What are the Panel's views on the extent to which the analysis of the frequency of co-occurrences of hourly relative humidity values below and above 90 percent with other meteorological events such as rain or fog (Chapter 3, section 3.3.5; Table 3-6) provides scientific support for consideration of how to address relative humidity in defining the form of a standard based on a PM light extinction indicator?

POIROT:
An informative, new analysis was presented in section 3.3.5 (as revised by the 3/4/10 memo from Philip Lorang and Mark Schmidt to the PM NAAQS Review Docket) comparing hourly daytime relative humidity (RH) and the incidence of natural weather visibility-impairing conditions (fog, rain, snow, etc.). The PM Panel concurred that this presentation provided a persuasive argument for the use of a 90% RH screen as an effective and efficient way to discard most of the hours potentially affected by natural weather conditions while removing only a small fraction of total daylight hours from consideration.

The Panel also found that a 90% RH screen was not only a logical revision for use in the UFVA assessment, but would also be an appropriate component of a new secondary NAAQS based on PM light extinction. It would be an effective way to remove periods when weather influences might be the dominant cause of visibility impairment from the regulatory metric, and would also have other benefits relating to PM light extinction measurements, such as accommodating spatial variability in RH and weather influences, minimizing effects of RH measurement errors at high humidity, and allowing for measurement modifications (such as a "smart heater" on a nephelometer) that could substantially reduce instrument maintenance needs and improve data quality.

4) In response to CASAC recommendations, descriptions of current conditions and results of just meeting NAAQS scenarios that considered all daylight hours were added to those based on maximum daily 1-hour indicators. The 98th percentile form was also included along with the 90th and 95th percentiles. Tile plots of hourly PM light extinction (Figure 3-12) and composition bar graphs of the top 10% of days for maximum daily 1-hour and aggregated individual daylight hours (Figure 3-13) were shown in part to help illuminate the similarities and differences between these various indicators with

respect to current conditions. Similarly, additional figures and table entries were generated to illustrate the characteristics of various PM light extinction NAAQS scenario forms (Tables 4-2, 4-3, 4-5, and 4-7; Figures 4-1 through 4-3).
What are the Panel's views regarding EPA staff interpretations of these displays included in the text? Are there supplemental or alternative interpretations the Panel would suggest? Are there additional approaches the Panel would suggest regarding ways to summarize, display, or assess the results of these analyses, including similarities and differences between the various scenarios?

POIROT:
The revisions to accommodate the 90% RH screen, the inclusion of a higher (98th) percentile form, and consideration of an alternative way of calculating percentiles – based on all daylight hours, rather than just the worst hour in a day – were valuable additions to the UFVA. When these are combined with other options including 90th and 95th percentiles and different levels (64, 112, 191 Mm^{-1}) of PM light extinction, the many resulting options are complex and difficult to communicate and compare. The various new graphical displays are quite helpful in communicating this complex new information clearly and illustrating the differences and similarities among the many options.

The PM Panel found that the tile plots were especially informative and really helped to "show" the seasonal and diurnal patterns and similarities and differences across the different study areas. The stacked bar charts of maximum hourly extinction on top 10% days vs. compositions on top percentiles considering all daylight hours are useful for seeing differences between cities, and the accompanying interpretation and discussion are reasonable and informative. The plots also illustrate interesting differences in hourly compositions within individual cities for the (relatively few) hours which represent the single worst hours in a day. However, these comparisons don't really allow a direct "apples vs. apples" comparison of the effect of calculating percentiles based on the single worst daylight hour each day, vs. using all daylight hours to calculate the percentiles.

It seems likely that the two optional approaches may tend to focus on different kinds of visibility impairing PM species, sources, meteorological conditions, times of day, and/or seasons. It would be useful to have a clearer picture of what their similarities and differences really are before recommending one approach over the other. Additional suggestions on approaches for this are included in comment from individual panelists. Several panelists also expressed concerns that some of the occasionally very high coarse particle contributions in a few of the cities (St. Louis and Los Angeles appeared questionable, and might benefit from closer scrutiny or warrant some cautionary language in the UFPA.