Compendium of Preliminary Pre-Meeting Comments
CASAC Particulate Matter Review Panel on
PM NAAQS: Scope and Methods
Plan for Urban Visibility Impact Assessment (Feb. 2009)

Comments from Dr. Ted Russell

Review of EPA PM Scope and Methods Plan-
Urban Visibility Impact Assessment
Armistead (Ted) Russell

I am generally pleased with the PM NAAQS Scope and Methods Plan for Urban Visibility Impact Assessment (hereafter, SM-Welfare), though when they consider welfare impact, they should go beyond urban visibility to include climate. As noted in the SM, there are significant uncertainties as to how PM will impact climate, but climate impacts have the potential to significantly impact our assessment of potential risks, particularly in terms of how specific components impact climate versus other components. As such, I am currently concerned that the SM-Welfare may miss a major piece of the necessary analysis. On the other hand, if one is just dealing with urban visibility, the current SM does provide a good roadmap to developing the information for providing advice on the potential revision to the secondary PM NAAQS. On the other hand, without consideration of the potential climate impacts, I would worry that the advice may not be fully informed. Dealing with the issue more fully here can help lay the foundation for the next review as well.

Like the SM-Health, I do note a few deficiencies, both in the document as well as the plan, e.g., (as noted in the SM-Health) it would have been very nice if the document had a section summarizing criticisms by CASAC and others on the prior risk and exposure assessments, and how they have responded. This could be done by grouping the types of comments made, and how they plan to address them, and where in the current document the planning takes on those criticisms, very much like a typical response to review document. This should become standard in the process.

Chapter 1

While I think that Chapter 1 does a fine job of laying out the issue, and noting that there is a relationship between PM levels and light extinction, it is written too much as the same chapter for the SM-Health in that it is oriented towards saying that strong relationships have been observed between PM and visibility. Unlike health, there is much less reason to address this issue by relying on statistics/finding associations. The underlying physics is well known. This should be the major focus. For one, it will focus the uncertainty assessment, and it will shift the discussion more to how certain are the effects. After laying out that the physics are well known, and that this can be addressed by first principle analyses if desired, one can then say that the correlations follow the physics. Indeed, after going through the rest of the SM-Welfare, I get the feeling that the problem is being made too big, and that the analyses are going to be overly complex, in part because we know so much more about the physics. I would reconsider what needs to be done here to provide the type of analysis that would lead to a different standard than

for the primary NAAQS, and recognize that there is going to be much more known about the physics here, so a certain amount of less detailed analysis can be tolerated because the uncertainties will still be quite reasonable.

On page 1-19, line 27, there is the comment that visibility effects are less well understood at night. This is true, but it misses something larger. We still understand the optics, and the visibility effects are probably understood quite well enough. What is less well understood is the importance/value. Most folks, I suspect, would say that visibility at night is less important, and that is what drives how to proceed, not our lack of understanding.

On page 1-20, it would be best, if it is not too much trouble, to incorporate both composition and RH, and this should be relatively straight forward using CMAQ results. On the other hand, if this is a very burdensome analysis, (though I would think it actually might be easier), then the former approach is fine.

I am positively disposed to having a PM light-extinction-based standard and using a nepholometer/aetholometer pair to provide PM light extinction. This would minimize some issues in computing light extinction. However, it also adds an issue about how to simulate attainment as some additional uncertainties are added, though I think those are quite reasonable.

Chapter 2:

The plan to assess urban visibility conditions is reasonable in most cases, though it does get a bit unclear, and it reads as though what level of uncertainty is allowable has not been decided, leading to some areas where the levels of analysis do not match.

In regards to the PRB, they suggest using CMAQ results, but if that is too time consuming, they might use the prior review's analyses. Using CMAQ results should not be overly complex, and is preferable. They plan to use those results in many other ways, so they should make it so doing the PRB-light extinction calculation just part of what is routinely found. There is one problem with relying on CMAQ results, and this is found (I think) in the ISA. CMAQ will have considerable uncertainty in simulating PMcoarse from soil, and this can complicate finding the PRB. Table 3-26 in the ISA does not provide an evaluation of CMAQ PMcoarse results. Note, like the SM-Health, I would still be cautious about laying the poor performance of CMAQ in the west to grid resolution problems.

On page 2-4, they start dealing with how to address PM10-2.5. I am not convinced that much ado about nothing is being made. How important is the anthropogenic component of PM10-2.5 to visibility? I did not see this in the ISA (I saw course contribution, but not anthropogenic coarse contribution). Next, I think the approach being proffered in lines 28-29 is going to add unnecessarily to the uncertainty as it integrates two relatively large

uncertainties. The first is the uncertainty in the source apportionment. The second is in the coarse-to-fine ratio. Both can be large. I would like to see how much anthropogenic PMcoarse adds to VAQ issues in urban areas. If anthro-PMcoarse is high on days when dust is high, one is much less concerned given the optical relationship.
Section 2.1.3 assumes that the standard will be based on PM-mass, possibly with compositional information as well. A deficiency is that it glosses over how compositional information will be used as few places will have sub-daily compositional information. Unlike the SM-health analysis, compositional information plays a much larger role here. They should also add a section on the approach if they use aetholometer/nepholometer pairs to measure light extinction directly. In that case, meeting an alternative secondary PM standard will be much less uncertain: a whole set of calculations need not be done.

In response to the specific charge questions associated with Chapter 2:

1a (or c): Role of PMcoarse: As noted above, you should consider the importance of this component to visibility, noting that we are interested in the anthropogenic component, and that the anthropogenic component may be large when the natural component is large, so that has much less importance.

1b. As noted above, I am positively disposed to this approach. It does shift some uncertainties, but I think it reduces them and simplifies the process. In general, given our knowledge of the physics and chemistry of the system, we have a pretty good way of relating emission changes in the more important species to visibility changes.

1c. Revising the IMPROVE algorithm may not be necessary. A first assessment can be to see what might be gained, and see if much effort is required. Is a reanalysis going to reduce an uncertainty or bias to the degree it will influence the process. I suspect it is relatively straightforward, so it may be deemed appropriate even if it only slightly modifies the results.

2. For the most part, following the same method to estimate PRB is fine, though more attention to compositional information and PMcoarse must be included, including model evaluation and how high levels of PRB-light extinction correlate to high levels of anthropogenic light extinction.

3a. I applaud the use of more high resolution compositional information. You have identified SEARCH as providing such data. There are other high resolution data (e.g., from the Supersites and other special studies) that can provide information on how composition changes with time, and CMAQ can as well (though this will add other uncertainties).

3b. The uncertainties section is rather short at present, though correctly notes that there will be uncertainties in the light extinction calculation. However, I suspect those will be

small. How CMAQ results are used, and uncertainties in those results, will be significant and should be assessed. How (and if) source apportionment is done on ambient data is also an issue. They need to address the uncertainty in how well visibility improvement can be calculated and how well they can predict how PM composition will change with controls.

Chapter 3.

The central theme in Chapter 3 is the development of how to quantitatively value the impact of the perception of urban visual air quality on individuals, citing the lack of information about public preferences.

Appendix A.

Again, climate may be the single biggest welfare (and it is also linked to health) concern from PM. In the REA I think that much more emphasis on climatic impacts, positive and negative, and compositionally dependencies, should be given than is indicated here. It is actually quite possible to do a quantitative analysis, though such an analysis may be fraught with uncertainties. However, it the conclusion is that certain species that increase urban light extinction also enhance global warming, one might be very tempted to develop a standard that addresses those components. For example, if an aethalometer/nephelmeter pair is used, one might be tempted to weight the light absorption component, or if it is done by component based on speciated PM mass, one might provide additional control on light absorbing species. Might one have a standard that sets a limit on total extinction, however, doubling (or more) the absorbing component?

Comments from Dr. Lowell Ashbaugh

Ashbaugh Review of Scope and Methods Plan for Urban Visibility Impact Assessment
Chapter 1 – Introduction

Charge questions

1. What are the Panel members' views on the general structure and overall design of the planned analyses?
2. Is the plan clear and transparent in its description of the planned approaches? Are the various assumptions and judgments that must be made in carrying out the planned assessments clear and transparent?
3. Given the goals set forth for the planned analyses, has the plan appropriately drawn from the existing scientific and technical information in developing the overall approach? Are there relevant features that should be added or modified in the planned approach?
4. In addition to the sub-daily PM2.5 alternative standard considered in the last PM NAAQS review and summarized in this chapter, an alternative standard structure is being considered in this review. This alternative structure would use daylight hourly PM light extinction, which can be measured either by a combination of instruments (nephelometer – PM light scattering and aethalometer – PM light absorption) or calculated from PM speciation and concurrent relative humidity data using a linear algorithm.
    a. What are the Panel members' views regarding this alternative structure and its utility in the context of this PM NAAQS review?
    b. What are the Panel members' views regarding advantages and disadvantages of this alternate structure compared to the sub-daily PM mass concentration approach?

The general structure and overall design of the planned approaches to the "standard" assessment of the public preferences for and value of urban visibility are good. I particularly like the idea of assessing whether these preferences vary across the country in cities with differing backdrops. However, it's not clear what the EPA is planning to do regarding the alternative secondary standard. The discussion of the "standard" assessments in sections 1.3.1 and 1.3.2 uses declarative sentences that start out "we are planning to …", but the alternative standard discussion in section 1.3.3 is much less clear. The foundation for the alternative standard is presented well but the section does not contain a clear plan. It contains phrases such as "could include" or "would then be specified."

The plan for the "standard" assessment draws appropriately from the existing scientific and technical information, building upon prior studies of visibility impact in remote areas to investigate urban visibility impact. The discussion of the alternative standard also draws upon earlier work to lay out the foundation, but does not go further to set forth a plan.

Overall, I like the concept presented in the alternative secondary standard structure. PM mass measurements for a sub-daily 4-hour period, and especially speciation of such

measurements, pose difficult technical challenges. The amount of PM collected in such a short time is hard to measure gravimetrically and even harder to analyze for speciated components. I am concerned about the use of “known relationships between PM mass and speciated components” to relate PM mass to PM light extinction. This concept assumes the speciation fractions during the daylight hours (or the 4-hour period) are the same as those measured during a 24-hour period. This is clearly not the case with nitrate particles, and is likely not the case for other species, either. Further investigation or review of existing information is needed here.

Comments from Dr. David A. Grantz

COMMENTS of David A. Grantz, University of California, 25 March 2009

REGARDING February 2009 draft of

USEPA PM NAAQS: Scope and Methods Plan for Urban Visibility Impact Assessment

This plan provides a good distillation of the newly released Integrated Science Assessment for PM, First External Review Draft (December 2008). It therefore reflects both the strengths and weaknesses of that document. The following represent suggestions for revision of the Plan, not in order of importance nor priority, and focusing on Section 1 (Introduction) but not limited to that section.

1. Better define the meaning and mode of application of the "policy relevant" filter to be applied in all aspects of this review.

The recurring restatement of the "policy relevant" caveat (beginning with three mentions on page 1-1) is an indicator of a problem throughout this document (and the ISA upon which it is based). Once the focus on visibility as the (only) surrogate for welfare impacts of PM was established, other information seems to have become not "policy relevant". The previous Criteria Document for PM (US EPA 2006), and related agency actions, did not find sufficient data on non-visibility parameters to inform a secondary standard (page 1-4, line 8-13). This might have initiated focused research in the intervening period but did not.

The current Plan, in its approach to these other impacts, does not ascribe to them sufficient gravity to encourage the research needed for the next PM assessment cycle. This contrasts with similar material related to human health endpoints of $PM_{10-2.5}$, some of which are appropriately to be addressed in the current review cycle (see 2009 Health Plan, Section 3.6, first paragraph).

It would be useful near the beginning of the discussion to insert a comprehensive definition of "policy relevant", its mode of application in the current review process, and a brief consideration of the likely consequences of doing so. This filter is an important behind the scenes driver of the review, is a new addition to the process, and should receive some discussion.

2. Combine and expand the discussion of "other" welfare effects of PM (not related to visibility), pointing to data and approaches that while outside the current scope of work may be useful in the future.

The minimal discussion of non-visibility impacts in the text (page 1-4 and pages 1-10 to 1-11), is augmented by a slightly more complete presentation in Appendix A. I suggest that these be combined, and placed within the text. This section should be expanded, to

more closely parallel the visibility sections, to call attention to data that are feasible to obtain but currently lacking, that might usefully inform the secondary standard setting process. These data could address a) ecosystem impacts of locally relevant particles (analogous to the local approach taken with respect to humidity/PM relationships to visibility), b) effects on climate (considerable data now reaches the stage of likely causality, and more is known about climate forcing and PM than is indicated (pages A7-A9), c) fouling of leaf surfaces and possible interference with gas exchange and radiation interception, particularly in urban locations, d) interception and scattering of incident radiation at the vegetated landscape scale.

The text (1-12, line 16), refers to an "initial qualitative approach" to non-visibility impacts, to be found in Appendix A. However, there is no approach, nor goal, nor suggestion of needed data, to be found in Appendix A. In combining the Appendix with the main text, such approaches and goals should be added.

In the summary of ecosystem affects (page 1-4, line 3-5), the review of the previous CD for PM is missing the previous emphasis on transport. While the previous review concluded that chemical composition dominates ecosystem effects of PM, the transport to and within ecosystems depends strongly on particle size and distance between source and receptor. This should be included in the discussion.

3. The focus on visibility, an aesthetic parameter, should be linked more closely to other potential impacts of PM that are more representative of damage, otherwise reliance on visibility introduces a number of unintended consequences..

The exclusive emphasis on visibility may have unintended consequences in the future. Proposed solutions to the temporal mismatch (page 1-19, lines 20-29) between health related and visibility related averaging periods, will require development of FRM hourly monitors and lead to the discounting of nighttime PM values. The suggested focus on daylight hours is primarily to bring cohesion to the visibility (rather than PM) data in the face of varying RH. This leaves the dark period as a potential window of opportunity for dumping of PM-rich emissions, to avoid a more restrictive potential secondary standard. The skewing of the averaging period by use of visibility as a surrogate for welfare effects should be resisted as much as possible. An approach is to use calculated visibility, once fully validated (see #3, below), in daylight and nighttime hours, or over 24 hour averaging periods e.g. page 1-20, lines 1-7).

The acceptance of a secondary standard by regulated communities will depend on its being relatively simple and intelligible to these communities and to the public. The proposed regulation of calculated visibility, rather than of PM concentration, is problematic in this regard. If visibility is to be regulated, then appropriate FRMs and appropriate PRB levels must be established. As PRB levels likely vary across the country, it must be established that regionally valid PRBs for visibility reflect similar concentrations of PM. The alternative (page 1-20, line 8-22), to set a uniform national visibility standard may prove untenable because PRB visibility is expected to differ

regionally. This situation is further complicated by seasonal trends in RH, which will lead to seasonality of NAAQS for PM. It appears unlikely that regulated communities will embrace a standard that implies regionally differing levels of PM, to achieve differing levels of visibility.

Given the likely regional variability in PRB for visibility, the use of locally relevant scenes for valuation should be given greater consideration (page 1-19, lines 5-12), despite the complexity of this approach. Fewer total scenes might be evaluated, but with greater precision and local relevance, than is suggested in the plan. This might allow scope for locally relevant "valuation" (page 1-19, line 10) which could be a powerful metric.

4. Define UVA and its relationship to the REA and ISA, at the point of its first introduction.

It is not clear from the text what the relationship between the urban visibility assessment (UVA) and Risk and Exposure Assessment (REA) is. This may only be semantic and can be easily addressed with a few additional words on page 1-1, near line 21.

5. Greater attention should be paid to linking reconstructed (calculated) light extinction to measurements of visibility.

The generality of the relationship between reconstructed visibility parameters, (calculated as functions of $PM_{2.5}$ and RH), is documented by reference to a strong correlation of such parameters with $PM_{2.5}$ (page 1-7, lines 5-12). The reasoning is somewhat circular. Better to focus on relationships between measured visibility parameters and PM concentrations to demonstrate these relationships, and on relationships between measured and reconstructed visibility to support the methodology. To this end, an appropriate goal of this review cycle (page 2-7) is the proposed development and validation of an urban optimized IMPROVE algorithm.

The discussion of needed visibility parameters, use of models to obtain them, and suitable averaging periods, is spread over much of the existing text. This should be consolidated into the text preceding introduction of the urban optimized IMPROVE algorithm, possibly prior to discussion of appropriate PRB for visibility (section 2.1.1). This would allow determination of what is needed for each purpose, and then how best to achieve it.

6. Maintain effort to provide quantitative valuation of welfare effects, including in this case visibility impairment.

The welfare effects and associated secondary standards have not been as influential as primary health standards for a variety of reasons. Valuation can be an important tool for welfare impacts. It is a mistake for the current plan to dismiss this goal as too complex (page 3-6, line 11-12), even if only incremental progress can be made in the current review cycle.

7. Minor error.

It is stated that both average radius and number of cloud droplets declines with increasing PM derived CCN (page A-9, lines 9-10). Droplet number is likely to increase with decreasing radius under these conditions.

8. The summary of previous rulemaking should be revised to include the recent court decision of 24 February 2009.

The discussion (page 1-5, lines 1-10) is incomplete due to court action after preparation of this draft Plan. This should be addressed for completeness, as the current treatment is now somewhat misleading.

9. Revise text to condense and focus arguments.

The lengthy discussion on visibility impairment and its relationship with fine PM (page 1-6, line 16 to page 1-8, line 4) is somewhat diffuse and does not lead to a clear conclusion. The objective of the section should be laid out initially, and the key conclusion summarized at the end. This might be facilitated by bringing it all into a single subsection under 1.1.1.

10. Elaborate upon key elements that are mentioned in passing.

Important other protections of visibility contained in the Regional Haze Rule and elsewhere receive passing mention (page 1-11, lines 13-20). These are key to understanding the goals of this Plan and should be expanded upon.

Certain states have enacted visibility rules (page 1-8, line 18). As above, it would be helpful to know which states, what rules, and some indication of successes and shortcomings of the approaches taken.