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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of chemical transport model predictions of primary organic aerosol for air masses classified by particle-component-based factor analysis" *by* C. A. Stroud et al.

Anonymous Referee #1

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The authors have taken on a challenging problem of trying to evaluate their model predictions of primary organic aerosol (POA). As the authors note, POA is an important component of fine particulate matter but it has received much less attention than secondary organic aerosol (SOA) in the modeling literature. The authors simulate air pollutant concentrations in and around Windsor, Ontario, and compare their results against field measurements collected during a 3-week period in the 2007 summer. Overall, the authors should be commended for their modeling effort and detailed analysis of the Border Air Quality Study (BAQS) data set. However, the conclusions drawn from this

modeling exercise do not clearly and substantively represent scientific progress in the area of atmospheric chemistry or physics.

In its current form, the manuscript reads more like an exhaustive technical report than a research paper. The 43 pages of text are accompanied by 13 tables and 12 figures which do not tell a clear story about model performance for POA. I urge the authors to identify 3-5 important conclusions from their entire analysis and rewrite the paper in a way that supports and accentuates those key points. To represent substantive progress in the field, the model evaluation should accomplish some combination of the following, for example: (i) demonstrate a systematic bias in the emission estimates from a certain POA source category; (ii) demonstrate a persistent error in the meteorological inputs that results in poor AURAMS model performance for POA during specific times of day or at specific locations; (iii) diagnose an error in the temporal allocation profile of an important source; (iv) diagnose an important error in the spatial allocation of a source; (v) diagnose an error in the speciation of a source sector. In general, the authors should focus on conclusions that are potentially generalizable to other locations and time periods. Any text that is extraneous to the key points ought to be mentioned very briefly in the main paper and moved to the Supplement.

For example, Section 3.3 can be captured in a couple of sentences conveying that the modeled CO is always biased high and modeled BC is always biased low at the Windsor site. If there's a key lesson that these two things teach us about the model performance for POA, I completely missed it. Similarly, the Introduction contains a lot of extraneous information. The 6th paragraph of Section 1 (p5944) looks like a good summary of past results, but the overall message that readers should take from this is unclear as is the relevance of that paragraph to the present study. The very next paragraph is written well, but it's not clear why a full paragraph is devoted to the study by Zhang and Ying. Likewise, the Abstract is extremely long. The 2nd and 3rd paragraphs ought to be condensed to focus on a few key results.

In Section 2, the authors have done an excellent job of documenting some important

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methodological details that are normally omitted in the literature. For example, the explanation of which model bins are compared with the AMS measurements (p.5951) is very good. So is the documentation of the PMF methodology (p.5955).

The discussion of emissions (Section 3.1) is extremely long and meandering. In their revised manuscript, the authors ought to shorten this to discuss the source sectors that are closely associated with important conclusions of the model evaluation. Any extraneous discussion can be moved to the Supplement for perusal by the more specialized readers. Sections 3.2 through 3.8 are also unnecessarily long, thus making it difficult to grasp the most important conclusions.

Finally on a more technical note, I disagree with the authors' statement (p.5961) that "... the PMF HOA factor is currently the best measurement-derived quantity to compare with model POA." The sentences preceding this statement point to numerous shortcomings that are inherent to this comparison. In contrast, source-specific markers for the most important POA sources in an airshed (e.g., levoglucosan, hopanes, cholesterol) provide a more direct measure of POA. Isotopic measurements of carbon in conjunction with the EC-tracer technique have also been shown to provide more reliable measures of POA. Section 3.9 indicates that the authors are aware of these other approaches to evaluating POA, so the statement on p.5961 really needs to be reexamined.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 5939, 2012.