Atmos. Chem. Phys. Discuss., 9, C190–C193, 2009
www.atmos-chem-phys-discuss.net/9/C190/2009/


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Evaluating simulated primary anthropogenic and biomass burning organic aerosols during MILAGRO: implications for assessing treatments of secondary organic aerosols" *by* J. D. Fast et al.

**Anonymous Referee #1**

Received and published: 14 April 2009

This manuscript compares a regional model simulation of POA by WRF-chem to the MILAGRO surface and aircraft observations. The authors first examine the model simulation of meteorological conditions as well as inert tracers CO and EC and then go on to investigate the model simulation of POA compared to the PMF breakdown of the AMS observations, in particular the HOA and BBOA classes. The objective of the study is to investigate the model performance on primary organic aerosol before exploring the more uncertain contribution from SOA, which models have had limited success in simulating. Finally, the study goes on to investigate the Total Observed

Organic Carbon (TOOC) measured and simulated for MILAGRO and the VOC breakdown. The manuscript is attempting to address an important issue of primary organic aerosol contributions to field observations of OA by using the information contained in the AMS observations. However, the authors are unable to firmly conclude on the strengths and weakness of the POA inventories used in the study. A more in depth examination of the model should be able to address this issue more fully. I include specific recommendations below.

Major comments

1. The manuscript highlights the ability of the AMS to separate HOA and BBOA. These are also trivial separations for a model study. The authors could therefore make a direct comparison between HOA and simulated POA from fossil fuel sources and BBOA and simulated biomass burning POA. The authors may have already performed such a sensitivity study (by tagging emission sources or turning off individual emissions) (page 4831, line7-8), if not such a sensitivity simulation should be performed. The authors can then compare to HOA and BBOA separately with the source-specific POA simulation on all the platforms and conclude as to which inventories may have weaknesses in which regions / time periods. This also applies to the CO and EC simulation – the percentage contribution of sources at different sites/times may clarify the source of simulation errors.

2. There is no discussion of the carbonaceous aerosol simulation. Standard 3D model simulations include hydrophobic and hydrophilic organic aerosols (and EC) which are "aged" with a characteristic e-folding time. Is this the case for WRF-Chem? If so, are the authors limiting their comparison between POA and HOA+BBOA to the hydrophobic component? The aged hydrophilic component may be more directly comparable to OOA in the AMS PMF scheme. If there is no separation in the model, can the authors comment on what fraction of the POA might appear to be HOA+BBOA and what fraction might appear to be aged?



ACPD
9, C190–C193, 2009

3. Page 4815: How are the PM2.5 emissions separated in the inventory? Is this breakdown (OC, EC, sulfate, nitrate, ammonium) provided in NEI or was it determined as part of this study? Is the breakdown a constant?

4. Section 5.1: I agree with the comment posted by Karl that the TOOC discussion is a little unfocused. As the authors do not have a complete organic carbon simulation (neglecting SOA) the overall TOOC comparisons are not very useful. If the authors are attempting to address the potential implications of the carbon budget for SOA formation, it would be useful to see a breakdown of TOOC targeting SOA precursors.

5. I'm curious if the authors considered the potential POA source for trash burning? This seems like something that could be quite important in a city like Mexico – was it included in the inventories? Could the authors comment on it?

Minor comments

1. Page 4814: Does the MOZART POA simulation match the WRF-Chem simulation? (tracer types and emission inventories)

2. Figure 1d: I recommend adding text to the caption to clarify that the fire hotspots were obtained by MODIS

3. Including both Figures 4 and 5 seems a bit excessive. I'd recommend removing Figure 4 and abbreviating the discussion (as this is a comparison with assimilated winds).

4. The correlation coefficient and bias numbers should be added to Figure 5.

5. Figures 9 and 16 are difficult to read. I'd recommend either replacing all the bars with the mean comparisons for each platform or greatly expanding the vertical axis to make the quantities more legible.

6. Page 4826, 1st line: I believe there's a typo and the text should indicate a negative bias (an underestimate by the model @ Paso). Also, comparisons at T2 are not



ACPD

9, C190–C193, 2009



included in Figure 7. Please add to the figure or modify the text.

7. I recommend adding the correlation coefficients between the model and HOA+BBOA for the entire timeseries (and the individual HOA and BBOA with the fractions of the relevant simulation after the sensitivity simulations are completed, see point #1 under major comments) to the top of Figure 13.

8. Page 4835, line 6: SVOC and IVOC are not included in the model, but nor are they measured. This should be clarified.

9. Page 4838: It would be useful to indicate the size range of POA used for model comparisons far before the Discussion text. I was wondering about this in the model description section.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4805, 2009.