

## ***Interactive comment on “Evaluation of new secondary organic aerosol models for a case study in Mexico City” by K. Dzepina et al.***

### **Anonymous Referee #3**

Received and published: 23 March 2009

This is an important paper describing the modeling of secondary organic aerosol (SOA) formation in Mexico City. The study follows up on the seminal paper by Volkamer (2006), it updates the modeling in that study and adds several additional mechanisms. The paper should be published after taking the following minor comments into account:

Page 4423, lines 1-2: This sentence is unnecessarily vague. Which tracers for primary emissions and secondary formation were used and what specifically do the correlation coefficients refer to?

Page 4431, lines 3-4: I am not sure the manuscript returned to explain why this specific choice of model was made.

Page 4433, lines 5-8: Ignoring the fragmentation pathways suggests that the model

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provides an upper estimate to SOA formation. If the authors agree, it might be good to add a statement to that effect.

Page 4436, lines 15-19: I suggest using background OOA instead of background SOA. It is likely that this OOA is indeed SOA, but ultimately the AMS only determines the composition and not the source of the aerosol.

Page 4440, section 4.2: The G-SOA model does not seem to be new in this manuscript. If that is the case, can the description of the model and the results be shortened by adding the appropriate references?

Section 4.5.2: As described by the authors, there is a significant difference in the description of the thermal denuder results depending on the assumption of equilibrium or of kinetic limitations to the evaporation. Based on this, are there any recommendations on how to do thermal denuder measurements better?

Section 4.5.3: This section is relatively weak, since there really were no C-14 data for comparison with the model. The authors merely assume that C-14 would have indicated a predominantly modern fraction of carbon like in the MILAGRO study. They may be right about this, and the ensuing discussion is not without merit, but still there does not seem to be enough concrete data to warrant such an extensive discussion.

Page 4450, line 14: I am not sure that “ambiguity” is the right word here. The interpretation of the C-14 data seems very straightforward.

Figure 2: It would good to explain in more detail how the graph was put together using the results of Kleinman (2008) and deGouw (2008).

Figure 8: This graph confused me. Between 6 am and 2 pm, the composition of the particle phase clearly changes, but the relative contribution seems to be rather constant. I was expecting to see the contribution from particles to increase due to gas-to-particle conversion?

Figure 9: While looking at this figure, I kept thinking what could be learned about semi-

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volatiles if an ambient sample were cooled. Perhaps the authors care to speculate about this?

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4417, 2009.

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