

Interactive
Comment

Interactive comment on “Combined effects of surface conditions, boundary layer dynamics and chemistry on diurnal SOA-evolution” by R. H. H. Janssen et al.

Anonymous Referee #2

Received and published: 12 June 2012

General Comments: The manuscript presents a new model that combines land surface conditions, boundary layer dynamics and chemistry and the authors use it to describe the contributions of these factors to the diurnal evolution of organic aerosol. The authors evaluate the model for a case study in the boreal forest of Finland. This work shows the importance of entrainment in the budget of organic aerosol and suggests that to adequately interpret the diurnal evolution of organic aerosol the land surface, boundary layer dynamics, chemistry and free tropospheric conditions all need to be considered. It is unfortunate that there is not a more direct comparison between measurements and the model (owing to a lack of measurements) but the paper is clear, well written and is appropriate for publication in Atmospheric Chemistry and Physics

C3537

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



once the authors have addressed the following comments below.

Specific Comments: The authors demonstrate the sensitivity of the model to land surface conditions and subsidence but I wonder about the sensitivity of their results to uncertainties in the chemistry of SOA formation from the terpenes. The reaction rates, yields and gas to particle partitioning of the subsequent products have large uncertainties - what affect do these factors have on the results, please comment?

The authors refer to one of the oxygenated organic aerosol factors as OOA2. This terminology has now been superseded by the term SV-OOA (see Jimenez et al., 2009; Science, 326, 1525-1529) and the authors should reflect this in the paper. I would suggest adding text to page 9343, line 10 where the term is introduced something like "One of them (SV-OOA, formally OOA2)..."

Table 1 shows the reaction rates for the included chemical reactions. What is the origin of this data, it is not clear. The source of the rates should be clearly stated. How were the loss rates of TERP determined, they are not well documented in the literature?

The isoprene flux given in table 3 is similar to that for α -pinene, can its contribution really being ignored when the yield of SOA could contribute $\sim 5\%$ to organic aerosol mass?

As the authors show with their model, the entrainment of free troposphere air dominates the budget of organic aerosol in the boundary layer and in urban areas this is likely to be larger due to the lack of available soil moisture and thus a lower evaporation flux compared to forested regions. This could be important for explaining the high radiogenic carbon (^{14}C) content of organic aerosols in urban areas if the dominant source is entrainment of organic aerosols that are predominantly biogenic in origin. This is potentially an important finding that perhaps the authors should consider alluding to in the manuscript.

Technical Corrections: Page 9334, Line 5 should read "...growth and entrainment is

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

are driven. . .”

Page 9336, Line 22 should read “. . .Hao et al. (2011) show showed that . . .”

Page 9337, Line 24 would be good to state here (and also in the caption for Table 2) that the mass stoichiometric coefficients used from Tsimpidi et al., (2010) are for the TERP category of V-SOA precursor.

Page 9338, Line 22 should read “. . .non-linear, especially at. . .”

Page 9339, Line 10 should read “. . .properties are have been...”

Page 9340, Line 19 should read “. . .evolution of $\langle\theta\rangle$ and $\langle q\rangle$ are reproduced well. This satisfactory agreement of of $\langle\theta\rangle$ and $\langle q\rangle$ evolution demonstrating that the model. . .”

Page 9341, Line 8 – 10 – it is not clear from the description what measurement error is affecting the NO₂ data. If the instrument being used contains a molybdenum convertor to convert NO₂ to NO, then it is likely that the data are biased high due to interferences from partial conversion of PAN and nitric acid to NO₂, it would be good to clarify this. A recent paper (Steinbacher et al., 2007; Journal of Geophysical Research-Atmospheres 112, D11307. doi: 10.1029/2006JD007971) has looked into the bias issues of commercial NO₂ instruments and discusses these issues and could be referenced if deemed appropriate.

Page 9347, Line 7 – should change ‘which’ for ‘what size’ to read “. . .emitted into which what size mixing volume. . .”

Page 9373, Figure 9 – it would be helpful to have a legend that defines the colored contours in these two panels because I had a hard time looking at the plots and then finding what the contour referred to in the caption and then looking back at the plots (same goes for Figure 11).

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 9331, 2012.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

