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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The sensitivity of Secondary Organic Aerosol component partitioning to the predictions of component properties: part 2; determination of particle hygroscopicity and its dependence on "apparent" volatility" *by* D. O. Topping et al.

Anonymous Referee #2

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Review of "The sensitivity of Secondary Organic Aerosol component partitioning to the predictions of component properties: part 2; determination of particle hygroscopicity and its dependence on "apparent" volatility" by Topping et al., submitted to ACP

General comments

The paper investigates how treating semi-volatile organic compounds affects the particle hygroscopity and, ultimately, the predicted climatic effect of aerosols (first indirect

effect). In their modeling approach, the authors used several different methods for calculating the saturation vapour pressure and activity coefficients of semi-volatile organics. Also, they authors considered differences between to cases: 1) semi-volatile organics re-equilibriate when RH is decreased to 0% from humid conditions, and 2) the organics do not re-equilibriate upon drying. The difference is concluded to be substantial and this is something should be considered in the measurements of aerosols containing a high amount of organics. Overall, the paper is well written, the methodology is sound and the conclusions are significant enough to warrant publication in Atmospheric Chemistry and Physics. Before the publication, however, I wish that the authors consider the following relatively minor issues.

Major comments

1. Introduction/Methodology, page 4. The authors based their equilibrium calculations on equation (1) which contains an implicit according to which semi-volatile organics are absorbed by the organic phase present in aerosols. However, many atmospheric organics are highly-water soluble and may therefore condense preferably on the aqueous phase (if present). Partially due to this, to my understanding, the absorbing mechanisms of aerosols in atmospheric conditions are still uncertain. Can the authors assess, even on a qualitative level, how sensitive the conclusions of the study are to the assumed absorbing phase?

2. Methodology, pages 6 and 7. The authors outline their approach quite nicely in 9 separate steps. However, I'd wish that the authors describe explicitly how the particle water content is calculated. This is certainly central to the paper and at the current form, this is hard to grasp because only a reference to a previous study is given.

3. Conclusions and future work, pages 18-21. Are the authors aware any previous studies that investigate the timescale of re-equilibriation of semi-volatile organics when the particle water content changes suddenly? The authors could make a literature survey and report briefly their findings (even negative ones).

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Minor comments

1. Page 9, last paragraph of the section. "...Hence on growth factor", not "...thus growth factor".

2. Equation 4, page 10. Please define S(D). 3. Page 11. Why 90% RH was chosen as a reference case?

4. Page 16, last paragraph of 3.2. Please define S_crit.

5. Figure 5. The authors should consider if imposing results for levoglucosan and fulvic acid is really needed here. Right now the figure is rather cluttered which makes it difficult to grasp the essential information from it.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9019, 2011.