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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Organic condensation – a vital link connecting aerosol formation to climate forcing" *by* I. Riipinen et al.

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We thank Reviewer 2 for his/her useful comments. Here are our point-by-point responses to the points raised by the Reviewer:

1.Page 398, Line 21-26. Some additional information on the models and assumptions would be useful here as requested by referee 1, comment 2.

We have added more detail on the modeling approach to the revised manuscript, as requested by the Reviewer(see answers to Reviewer 1 for details).

2. Page 399, Line 27. Soot is generally defined as a mixture of black carbon and organic matter. Is that what you mean here? Would it be more precise to refer to this as black carbon?

The Reviewer is correct - we mean black carbon and have changed it in the text.

3. Page 401, Line 5. Why does the GR of 1.5-3 nm particles not have a seasonal dependence? You also show that sulfuric acid does not fully explain the growth of particles in this size range either so a seasonal cycle would be expected if the missing growth is coming from organics also.

We do not know the exact reason for this. It is possible that in addition to the biogenic organics (that show a strong seasonal cycle) there are some additional compounds that grow the smallest particles and are present in large enough concentrations all year round.

4. Page 401, Line 7. What is the seasonal dependence of sulfuric acid? Since it is driven partly by OH which has a strong seasonal cycle this may also have a cycle that matches the observed cycle in GR.

The seasonal cycle of ambient sulfuric acid concentration behaves similarly to the growth explained by sulfuric acid shown in Fig. 3c. The seasonal behavior is explained partly by the OH-driven production of sulfuric acid and partly the seasonal pattern of the condensational sink (which is the main sink for sulfuric acid vapor). The sulfuric acid concentrations for Fig. 3c are estimated using a proxy introduced by Petäjä et al. (ACP, 2009). The local minimum in the summer is explained by the higher condensational sink as compared with spring (i.e. the shorter average lifetime of the sulfuric acid molecules).

5. Page 401, Line 19. Why were these 7 cases chosen?

The cases were chosen based on the homogeneity of the airmass on the day (i.e. the DMPS displayed a non-interrupted and clear growth of the ultrafine particles) and the availability of DMPS, AMS, sulfuric acid (for Hyytiälä) and VDMPS (for Hyytiälä) data on the days.

6. Page 402. Is it possible to use the AMS observations to further confirm this large

contribution of organics to the growing nucleation mode (> 20 nm particles)?

We had total (integrated) mass AMS data available to us - unfortunately size resolved data was not available for the investigated time period. However, the total OA mass increased greatly during each event and the total sulfate masses were generally stagnant - which supports the idea of organic compounds contributing significantly to the aerosol growth (along with the observation of ambient sulfuric acid concentration being too low to explain it).

7. Page 403, Line 23. Should the following be removed: see auxillary methods?

Yes, we have removed this from the revised MS.

8. Page 403 Line 21 onwards. It would be useful to give some numbers here. For example, what are the globally averaged percentage changes?

This is a very good point – we have added the globally averaged concentration percent changes to the text.

9. Page 404, Line 2. I assume you are referring to the first (cloud-albedo) aerosol indirect effect here. Please clarify.

Yes, we have clarified this in the revised manuscript.

10. The references need careful checking. I found a number of errors but did not do an exhaustive check: Page 389, Line 19. Nel (2005) cited but not in reference list. Page 399, Line 21. Typo. Should be Trivitayanurak et al. (2008) not be Trivitayanurak (2008) P400, Line 4. Spracklen et al. (2006) cited but missing from reference list. P400, L6. Vehkamaki et al. (2001) cited but missing from reference list. Should this be Vehkamaki et al. (2002)?

We have carefully checked the references and included the corrections pointed out by the Reviewer. Thanks for remarking all these!

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 387, 2011.