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***Interactive comment on* “The effect of model spatial resolution on Secondary Organic Aerosol predictions: a case study at Whistler, BC, Canada” by C. D. Wainwright et al.**

Anonymous Referee #1

Received and published: 12 July 2012

This is a straight-forward study and very clearly presented. I have just a couple of suggestions that the authors might like to explore and a few very minor corrections listed below.

SUGGESTIONS

1. Regarding the role of lifetime, I wondered if the authors considered looking at the different SOA types (i.e. monoterpene-SOA vs isoprene-SOA vs aromatic-SOA) as a supporting argument for this? There is a range of lifetime for the different SOA species and one might expect that the differences in this lifetime effect due to resolution would differ as a result. It might be worth investigating.

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2. The impact of resolution on SOA model predictions could be very relevant to the interpretation of previous studies. However these previous studies represent a range of models and environments, and not all may be impacted. For example, the study of Volkamer et al., 2006 (referenced at the end of Section 3.1 and 4) uses a box model and is not subject to resolution errors. The focus here is on the surface measurements (and thus you can certainly point out the implications for any previous surface 3D model-obs comparisons), but one question that is raised in my mind is whether this is a relevant issue for aircraft campaigns? One could imagine that simulations aloft could be more (plumes) or less (well mixed) affected by resolution. It would be very interesting to include a version of Fig 6 for the free troposphere (say 5 km) and a discussion of these issues.

MINOR CORRECTIONS/COMMENTS

1. Page 16027, line 25-26: I'd suggest a re-phrasing here. It's not clear that the major uncertainty wrt to SOA is purely biogenic. The message of Spracklen et al., 2011 is that there is an important missing source of "anthropogenically-controlled" SOA (with biogenic C). So the sentence as written is a little misleading.
2. Page 16029, line 8: grammar and repetitive use of "during". Suggest: "measurement done during the Whistler Aerosol and Cloud Study (WACS2010) in July 2010."
3. Figure 1: the boxes around Whistler are hard to see, especially the 0.5x0.67 box. Perhaps you could show a zoom of the region as an inset on the figure?
4. Page 16031, line 4: reference for MEGAN 2.1 is not Guenther et al., 2006. The best reference here would be for the implementation of MEGAN 2.1 in GEOS-Chem: Barkley et al., 2011
5. Page 16031 line 16, page 16032 line 10 and line 18: The authors should avoid citing webpages (not peer-reviewed or permanent record). There should be literature references for these model developments/issues.

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6. Page 16033, line 7: suggest inserting text to clarify: “Figure 3a shows the high resolution simulated aerosol. . .”
7. Page 16033, lines 12-19: I don't disagree with the authors' point that mixing depth is not likely to be a major factor, but I think the language is a little strong here. This is a VERY qualitative comparison of mixing depth and the figure would suggest that the mixing depth is in fact a little deeper in the model that shown with the lidar obs (i.e. consistently ~ 1.5 km in the model and +1km in the obs). Would be good to acknowledge that, but that as stated in the text, that this isn't likely to be a significant error.
8. Page 16034, lines 16-17: what about the role of topography in the region as well as ocean margins? I didn't see any discussion of the challenges associated with mountain flow and meteorological conditions at Whistler.
9. Page 16035, line 3: remove “below” (you don't know if in final copy-editing the figure will appear above or below this text)
10. Page 16037, lines 10-18: It's not quite clear here what factor leads to a drop in continental monoterpene and isoprene emissions at higher resolution. Could you clarify?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 16025, 2012.

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