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Comment

Interactive comment on “Improving the representation of secondary organic aerosol (SOA) in the MOZART-4 global chemical transport model” by A. Mahmud and K. C. Barsanti

Anonymous Referee #1

Received and published: 2 January 2013

This manuscript describes recent efforts to update the SOA treatment in the MOZART-4 model. The work is not strongly original, but it is appropriate to publish in GMD to document some significant changes to the model scheme for organic aerosol. Below I detail some major concerns regarding clarity and approach along with some minor comments. After addressing these comments the manuscript may be acceptable for publication.

Major

1. It is unclear how MZ4-v1 compares to the baseline simulation and indeed why exactly this update was implemented. Are the new 2p yields based on new experimental

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results? It would be useful to compare 2p yield parameters for the different precursors in the baseline and MZ4-v1 scheme. How do the volatilities compare?

2. The comparison with observations (3.2) is uneven and inadequate. The authors are largely citing a random handful of previous studies in the literature. The paper cites no surface observations over North America or Europe! I suggest that the authors start by taking the global OA observations reported by Zhang et al., 2007 and quantitatively compare those with their simulation (scatter plot?). They could then add to these values the measurements from Asia and South America that are cited in the manuscript (regions not necessarily well represented in Zhang et al., 2007).

3. High NO_x yields were used in this work and some discussion of the impact of this assumption should be included; particularly in tropical regions, where high NO_x is generally not appropriate.

Minor

1. Abstract, line 6: clarify that these VOCs were ADDED to the new scheme and are not the only VOCs under consideration in the MZ4-v2 scheme.

2. Page 4189, line 20: “double estimated” is confusingly phrased. The 16.4 Tg/yr number cited from Henze and Seinfeld is clearly not double the production from Chung and Seinfeld of 11.2 Tg/yr. Suggest you re-phrase to something like “Henze and Seinfeld . . . isoprene makes up over half of total SOA production (16.4 Tg/yr)”.

3. Page 4189, lines 20-22: It should be clarified that the estimates from Spracklen et al. are based on observations and are not bottom-up as in the previous cited studies. Indeed, the sentence that follows in lines 22-24 incorrectly implies that the difference between Chung & Seinfeld and Spracklen et al. has something to do with improved estimates of VOC fluxes and SOA parameters. This is not the case.

4. Page 4192, lines 5-14: References for aerosol modeling in MOZART-4 required.

5. Page 4192, line 26: “C > 3” is technical, would be helpful if you spelled out “more

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than 3 carbons”

6. Page 4193, lines 11-12: Is the baseline scheme that described by Lack et al., 2004? If so, clearly cite at the beginning of section 2.2. (If not, indicate the origin of baseline simulation). Also clarify if an iterative solution was obtained for SOA when SOA is included in Mo, and whether irreversibility was assumed.

7. Page 4194, lines 3-5: Reference experimental data used to obtain these fits.

8. Page 4195, line 1: what are “major stable species”??

9. Page 4195, lines 15-16: This comparison is perhaps limited by using only monthly output, but this is not an inherent characteristic/limitation of global CTMs (output timescales limited only by model time step).

10. Page 4195, lines 17-20: This is unclear and poorly worded. It appears that the authors are discussing limitations associated with estimating SOA using OC-EC tracer methods. This is not the only method for estimating SOA, so a more complete discussion is required with respect to challenges in reporting SOA. In fact one could also point out here some of the inherent measurement errors for OA (detection limits, artefacts, size cuts, etc) that are also relevant.

11. Page 4200, lines 12-14: Heald et al., 2011 actual shows that their model reproduces the vertical profile of OA in most locations.

12. Page 4200, lines 20-24: Henze et al., 2006 should be cited here – they first showed that including isoprene SOA increases OA loading aloft (and discuss why).

13. Page 4201, lines 17-27: The authors could (and should) verify their hypothesis for the change in lifetime of total SOA by calculating the lifetimes of the anthropogenic and biogenic SOA separately.

14. Page 4205, line 9-12: It might be worth noting that this model simulation (and that of Lin et al.) still considerably underestimate the ACE-Asia observations, despite an

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overestimate of surface observations in SE Asia that is noted in Section 3.2.1. It's not clear how this model performs at the surface in East Asia (but perhaps after adding comparisons with Zhang et al., 2007 this could be addressed).

15. Section 3.2.3: There are several other global models studies to which this one could be compared, including Hoyle et al., 2007 and several of the Tsigaridis et al. papers.

16. Page 4207, line 11: "Figure 7 shows a measure" is incorrect. The figure shows a simulation, not a measurement.

17. Section 2.1/3.3: The model description needs to include discussion of the optical and size assumptions and formulation of the AOD calculation in MOZART-4 with appropriate references.

18. Section 3.3: Given the relatively small role that SOA plays in the global AOD simulation, I suggest that the authors trim this section. Table 4 and one of Figure 7/8 could be removed.

19. Section 4: The conclusions largely repeat numbers given in the Results section. I suggest that these be summarized more succinctly.

20. Figure 2: The comment on mass of C is confusing. Is the figure actually showing total mass or mass of C only? If the later, the label on the color bar should be changed.

Interactive comment on Geosci. Model Dev. Discuss., 5, 4187, 2012.

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