Geosci. Model Dev. Discuss., 5, C1478–C1481, 2013
www.geosci-model-dev-discuss.net/5/C1478/2013/

Geoscientific
Model Development
Discussions

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 #2

Received and published: 14 February 2013

The manuscript by Mahmud and Barsanti describes an alternative parameterization of SOA formation in the MOZART-4 CTM. The authors used a modified 2-product model formulation, with parameters taken from the volatility-basis set (VBS). Nevertheless, it is not clear what exactly they did to extract these parameters; the description is fairly fuzzy. The authors explain that they used a methodology found in the literature, but it should be described in greater detail in the text, since in its current form the reader cannot distinguish between the different formulations. It is well known what the 2-product model is, as well as what the VBS is. The merging of the two though, is not clearly explained. Are the authors using the VBS to calculate the 2-product model parameters, and then they use the 2-product model formulation? If yes, they

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

should not name their method VBS, since it is not. Maybe they indeed use a simplified VBS approach with less (probably 2) bins? This has to be described in greater detail. How many bins do they use? What assumptions do they make for the aging of the semi-volatile gases and conversion to less volatile species? How do the volatility bins correlate with the 2-product model values in Table 1? On top of the model description, the evaluation section that includes comparisons with measurements and other models is very poor. There are many more datasets available in the literature for comparison with model results, as well as many more models to compare with. The units used are also very confusing: the authors almost everywhere mention ug/m3, and in a few cases (e.g. figure legends) they mention that they really mean ugC/m3. If ugC/m3 is meant, this is how it should appear in the whole manuscript, not ug/m3. All of these topics have to be addressed before the manuscript is accepted for publication.

Specific comments

1) P. 4192, l. 5-7. The model is not having dust? In figure 7 it appears that it does.

2) Eq 1: There are no gas-phase products from this reaction?

3) P. 4193, l. 14: How is "small amount of Mo" defined? How small? How does the choice of this amount affects results, if at all?

4) P. 4193, l. 20: Why these two reactions are excluded from the 2p-VBS?

5) P. 4194, first half: This discussion must be moved earlier.

6) What is the spinup time of the model?

7) P. 4197, l. 22-23: temperature is also greatly important.

8) P. 4198, l. 1: Do BIGENE and C3H6 play any role, even minor, compared to isoprene? I would expect them to be negligible, given their low emissions and not very high aerosol yields.

9) P.4198, middle: What happens downwind Australia in December?

GMDD

5, C1478–C1481, 2013

Interactive Comment

10) P. 4199, l. 15: the 1-sigma includes both temporal and spatial variability?

11) The text from p.4200 l-6-10 should be moved in the middle of p. 4199.

12) P. 4200, l. 12-18: what is the temperature effect on the adopted Kp?

13) P. 4201, l. 4: this is valid only if there was enough time for the model to spinup.

14) P. 4201, l. 16: These are pretty long lifetimes. Are there a lot of SOA above clouds? What is the boundary layer vs. free troposphere lifetime? How about the load?

15) P. 4202, l. 2: "little impact" is not correct, since figure 4 shows a factor of two difference in many places.

16) P. 4202, l. 5: It is ok to compare only one simulation, if this is considered to be the best. If it is, it has to be stated.

17) P. 4203, l. 6: There are data available: Zhang, X. Y., Wang, Y. Q., Niu, T., Zhang, X. C., Gong, S. L., Zhang, Y. M., and Sun, J. Y.: Atmospheric aerosol compositions in China: spatial/temporal variability, chemical signature, regional haze distribution and comparisons with global aerosols, Atmos. Chem. Phys., 12, 779-799, doi:10.5194/acp-12-779-2012, 2012.

18) P. 4203, l. 13-15 and p. 4204, l. 8: The OM/OC ratio is also an issue. In addition, the parameterization itself can be a major source of uncertainty that can contribute to the discrepancy.

19) Why section 3.2.3 is different from section 3.1.3?

20) P. 4206, l. 10-15: This is a very strict statement. There is no reason for models to be exactly identical in order to be compared with each other. There are many useful comparisons that can be extracted from different models, when their differences are known and understood. A great example is the several AeroCom intercomparisons.

21) P. 4207, l. 1-2: The removal does not influence the production, at least not directly.

GMDD

5, C1478–C1481, 2013

Unless the authors claim that the removal affects the pre-existing aerosols that new SOA can condense on, this statement is incorrect, no matter how the aerosol lifetime is being calculated in the model (p. 4201, l. 4).

22) P. 4209, l. 1-2: How about other regions?

23) Table 1: Are these numbers the original ones, or the updated ones, before adding the new species?

24) Figure 5: maybe it is better to show v2-v1?

25) Figure 6: since the authors have measurements, why not show them here?

Technical corrections

1) P. 4196, l. 18: “SOA formation” should be “SOA concentration”.

2) P. 4199, l. 18: “global land mask” should be “model’s land mask”.

3) P. 4207, l. 14: “and sulfate” should be “sulfate”.

4) Table 4 appears out of order in the discussion.

5) Figure 1 is not needed.

6) The color scale of Figure 2 is not very good. Bby eyeballing it, a maximum of 1ugC/m3 might be better.

7) Add a/b in Figure 3.

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