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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigating organic aerosol loading in the remote marine environment" *by* K. Lapina et al.

Anonymous Referee #1

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In this paper the authors evaluate different organic aerosol parameterizations in the GEOS-Chem model against comparison to ship observations. The authors find that the observations are consistent with a marine OM source of 9 Tg/yr or smaller. The authors also find that the model underestimate AOD by 13-30% but that this is not due to organic aerosols. This is a generally well-written study with interesting results. My main concern with the manuscript is that the authors do not place the 30% model underestimate of MODIS AOD within the context of uncertainties of the satellite retrievals, which are at least as large as the difference between model and observations. I also have a number of minor comments that are listed below.

Main concern:

The authors insist at length on the 30% model underestimate of the MODIS AOD, with-

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out any satisfactory explanation. The difference corresponds to a mean underestimate of 0.03 in AOD (page 10985, line 1). Such attention doesn't seem warranted given that this difference is within the accuracy of the MODIS retrieval over Oceans which is +/-0.03 +0.05AOD (see any MODIS retrieval paper). For AOD $\sim$0.1 this corresponds to an uncertainty of 0.035 in AOD. Thus the difference between model and observations is within the reported uncertainty. In addition, as noted by Remer et al. (2008) the TERRA AOD is biased high by 0.015 (which accounts for half of the difference between model and observations). Finally, when the authors compare the model AOD to AERONET MAN AOD, they find only a 13% underestimate. Clearly a small difference. I recommend that the authors discuss these AOD differences with more attention to the AOD uncertainty in both MODIS and MAN.

In addition, many uncertainties are associated with the model calculation of AOD, from estimate of the burden of the different aerosols (related to sources and sinks) to the optical properties used. The authors have clearly demonstrated that whatever the scenario used, OM accounts for a tiny fraction of the AOD. However the model AOD over these remote regions is dominated by sea salt and sulfate. It seems that small changes in the assumed optical properties of either one of these components could easily account for the 30% difference in AOD. A discussion of this would be useful.

Minor comments:

1) Abstract, line 14 "mean OM concentrations. . ., are largely underestimated". Please be more quantitative. By how much are the observations underestimated? Also, the authors should note that OM concentrations are underestimated if a marine OM is not included. This isn't clear in the abstract.

2) Page 10979, lines 1-5. It seems that the authors use SeaWIFS chlorophyll for 2006 and 2007, but MODIS chlorophyll for 2008. For consistency why not use MODIS chlorophyll for the entire 3 years? Are the 2 datasets identical? It would be useful if the authors compared MODIS and SeaWIFS chlorophyll in 2006 and 2007 and the resulting

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OM emissions. This would help evaluate uncertainties in OM source associated with the chlorophyll dataset used.

3) Page 10979, line 5. ". . . re-gridded to 1 degree resolution". I thought the model was run at 2x2.5 resolution (page 10977, line 4). Please clarify.

4) Equation 1. What are the units of [Chl]?

5) Page 10980, line 22-28. It appears that even though the authors are using the same OM parameterization as in Langmann et al. (2008) and in Vignati et al. (2010), the resulting OM sources are very different in these 3 studies: 8.2 TgC/yr, 2.8 TgC/yr, 5.8 TgC/yr. Nearly a factor of 3 difference! The authors mention different sea salt parameterization as the culprit. Could they elaborate? What are the assumed accumulation mode sea salt emissions used for each study? What parameterizations are used?

6) Figure 1. I am confused as to the difference between the central and left panels in Figure 1. They both used the Langmann et al. (2008) parameterization for chlorophyll, correct? Then I assume that the sea salt must be difference. It seems that the sea salt on the right is based on Jaegle et al. (2011). What sea salt is used for the central panel? What are the total OM sources for each panel? Which one is referred to as Langmann et al. in the text (8.2 TgC/yr)? Which one is referred to as GCL in Table 1?

7) Page 10985. Line 22. "Comparing model AOD to the MAN data, separately for the fine and coarse components. . ." It would be useful to the reader if the authors clarified this statement a bit. What is the model underestimate for fine AOD? Coarse AOD?

8) Page 10986 line 1-3. "anticorrelated with sea salt AOD simulated in the model". How does the difference correlate with sulfate AOD simulated by the model? How about total AOD? Simply looking at the figures it would seem that there is an anticorrelation with total AOD, indicating that the percent difference is largest at the lowest AOD where the retrievals are most uncertain (see main comment above).

9) Page 10986 line 15. "ICEALOT is the only campaign where GC is biased low". It

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appears that the model is also biased low during RHaMBLe (Table 1).

10) Page 10988 line 20-22. “Measurements of the sea salt component of sea spray (not detected by AMS) would be required to verify this relationship”. I know that measurements of submicron Na+ mass concentrations were conducted for ICEALOT and VOCALS using cascade impactors. I believe that Russell et al. (2010) discuss the correlation between Na and OM. It could well be that Na mass concentration measurements are also available for the other campaigns discussed by the authors, as it is a fairly standard measurement.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 10973, 2011.