Atmos. Chem. Phys. Discuss., 10, C10881–C10887, 2010
www.atmos-chem-phys-discuss.net/10/C10881/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling sea-salt aerosol in a coupled climate and sectional microphysical model: mass, optical depth and number concentration" *by* T. Fan and O. B. Toon

Anonymous Referee #2

Received and published: 9 December 2010

This manuscript describes the validation of an integrated sea spray emission function (achieved by combining three previous functions) against measurement data sets. It also briefly discusses the role of removal processes on the atmospheric sea spray aerosol number and CCN concentration, and finds that these properties do not necessarily peak when the emissions are the highest.

The main strength of the manuscript is that it presents comparison of several aerosol properties (number, mass, optical depth, size distribution) which increases the confidence in the chosen source function. My main criticisms of the manuscript concern omitting all non-sea spray aerosol components from the simulations, the representa-

tiveness of some of the data sets and some technical aspects of the model simulations (see major comments). However, its thoroughness should make it an interesting read for many global aerosol modelers, and therefore I recommend it to be published should the authors present the limitations and uncertainties of the study more clearly as well as address the specific comments below.

Major comments:

1. None of the atmospheric aerosol components exist in total isolation, and thus it is very challenging to draw comprehensive conclusions based on a model simulating only sea spray. Both primary organic material co-emitted with sea spray and sulphate from condensation and aqueous phase reactions can affect the climate properties and removal processes of sea spray. The implications of this on the conclusions of this study should be discussed.

2. For many aerosol properties, the authors compare the model only against a single data set. I understand that more suitable data may not be available; however, the uncertainties arising from this should be discussed.

3. Mårtensson et al. (2003) showed that the shape of the source function depends on the sea water temperature. Since the Clarke source function does not include this dependence (measured at 25C) and since Mårtensson and Clarke have previously been shown to agree at 25C, why not use the Mårtensson flux as a base of the new integrated flux? Since many of the observations used for comparison were not measured in tropical conditions, the effect of the temperature dependence on the model-measurement comparison should be discussed.

4. There are some technical details that I'm not totally convinced about: a) The Caffrey and CMS source functions have been multiplied with equation 1, but the Gong hasn't. Apparently this is because the Gong function is already low in the larger size range. However, now the Gong function is not anymore comparable to the other two. In my opinion, the Gong simulations should be rerun. At the very least the effect of treating

the source functions differently should be thoroughly discussed. b) How representative of the oceans are the shape and scale parameters for dust storms (page 24510)? How will the uncertainties in them affect the sea spray fluxes? c) Instead of normalizing the measured and modeled values in many of the plots (e.g. 14), the authors should simply show two scales on the y-axis. This way it would be possible to compare also the differences in the magnitude, not only in the shapes of the curves. It is also totally unacceptable to normalize only some part of the curve as was done in Figure 14 (or does p 24523, line 8-9 mean that all particle sizes where normalized but actual value used for normalization did not take into account the smallest particles?).

Specific comments:

5. SSA is a commonly used abbreviation for 'single scattering albedo', and thus using it for 'sea spray aerosol' is confusing especially when the optical properties are discussed. I recommend changing to SS.

6. It would make it easier for other groups to adopt CMS if the source function equations were presented explicitly in this manuscript.

7. In the introduction, make sure to indicate which of the previous findings are purely from models and not confirmed by observations.

8. page 24505, lines 2-4: -> differ *by up to* a factor of 2. Also, there are similar magnitudes of difference also around 200 and 500-600 nm.

9. Page 24506, lines 6-10: Contradicting information about the upper limit of Clarke applied (0.6 or 1 um?). Last paragraph: For some flux functions the largest particles dominate the areas also at low wind speed. Last line: "SSA area is usually related. . ." is vague. Clarify in which cases the relation holds.

10. page 24508, lines: 9-11: is the model run truly offline or in a nudged mode? line 18: observation -> observations; lines 21-22: I don't follow what 'as they are —' refers to

11. Section 2.3: The dry deposition scheme is fairly standard and thus this section can be significantly shortened.

12. Section 2.4: The particle wet size is highly dependent on the RH in the range 98%-99.9%. Thus setting an upper RH limit of 98% is not a good assumption. I also can't follow the 'theoretical base' part of why this is done. If Gerber's formula cannot be used at high RHs, then they should not be shown in Figure 4.

13. p. 24514, lines 8-13: Do I understand correctly that the user can prescribe 0-100% of in-cloud scavenging or what does the solubility factor refer to? If the former, then the first sentence about default value is slightly confusing. Please reformulate.

14. Section 3: State the time period of the model simulations as well as the measurements. It is mentioned in several places that not exactly the same years are compared (which is understandable in the case of global models) but more detail is needed. When same years are not compared, do the observations represent multiannual monthly means, etc.?

15. Section 3.1., end of first paragraph: For consistency, it would be better to treat the data sets in the same way (i.e. do the elimination based on weekly data, if this is the coarsest time resolution). Is similar elimination of data done for model results? If not, why? Given the different local wind speeds and removal in the model and in the real atmosphere (due to poor spatial resolution, different years, difficulty to model wet removal in the first place) and the relatively small differences between the source functions, is it really possible to make conclusions about the superiority of one function over the other?

16. p 24515, lines 21: do equally -> do almost equally; lines 22-23: this is true for CMS parameterization slope but not correlation (best correlation with solubility factor 0.3). Not at all true for Gong parameterization; Line 25: very well -> reasonably well

17. p 24516, lines 3-4: this is somewhat contradictory to p 24507, lines 4-5 where it is

said that it is not know if spume is crudely presented.

18. p 24518: What size range does ‘coarse mode optical depth’ correspond to? If typical definition of coarse mode (i.e. diameter larger than 1 um or 2.5 um), then only a very small fraction of SS in terms of total optical depth or number is investigated. I’m not convinced the presented optical depth comparison is then very useful for model validation. Last sentence of section 3.2.1: is the whole range (0.1-1um) included in the optical depth calculations presented.

19. Reformulate Table 3: Since two totally different data sets for different locations are used, it might be clearer to present mass and optical depth in two different tables. At the very least, the table caption and heading must be clarified to highlight this fact. The second line of heading (‘Model=S_M SP’) is very confusing and uninformative. Give the sites/regions/networks compared to in the table caption. Is the model optical depth from only one model grid cell (Mace Head)? This is something that should be clarified also concerning comparison to other data sets.

20. The Mulcahy data set is one whose representativeness is not certain (one site, very limited data set due to strict requirements). Therefore I’m not sure it makes sense to compare to model ‘North Atlantic’ and ‘Southern Ocean’. Also refer to table 4 for definition of these areas.

21. Table 2 shows 7 (not 5 ocean regions)

22. Any scaling or normalization done for the figures must be explicitly stated in the figure captions, as it is unlikely that all readers go through the text in detail.

23. How do the values in Figure 12 compare if the scaling is not done? In my opinion it would be more honest to show the unscaled values, the similar wind speed dependence should be evident also in this case.

24. page 24522: How representative is the Norris measurement?

25. Figure 14: If the discrepancy in the volumetric size distribution is likely due to

inversion of measurements, it might be better to omit 14 a) and just discuss it in the text. Many readers only scan through the abstract and figures, and as is Figure 14 a) gives the impression that the model doesn’t perform very well. Again: normalization must be mentioned in figure caption

26. Figure 15: In all fairness it should be stated in the text that overall the Gong function performs the best against the available data set. p. 24525, lines: 6-7: given that there are about an order of magnitude difference at some sizes, it cannot be said that they match ‘very well’. I agree that the absolute concentration is (always) very small in these size ranges, but that doesn’t make the match very good.

27. p. 24525, last paragraph: Since the majority of aerosol number from CMS comes from the part described with the Clarke parametrisation, it is a little pointless to compare to the measurements behind the Clarke parameterization. At least state this explicitly. Clarke diameter space does not match the indicated radius space (0.01-0.8 vs. 0.01-0.4); overall, this sentence is not necessary.

28. Figure 18 and last paragraph of 3.4.2 are common knowledge and can be removed.

29. How do the calculated CCN concentrations compare with previously published model values (e.g. Korhonen et al. (2008, JGR) present simulations without DMS for the Southern hemisphere remote oceans)? It should be straightforward to recalculated CCN at corresponding supersaturations.

Technical comments:

30. Throughout the manuscript, replace Marttenson or Martenson with Mårtenson (i.e. one t, the second letter is an å not an a)

31. page 24504, line 13: they -> there

32. In figures 2, 14, 15 and 17, use simple lines without the dots/triangles etc. which just make the plot busier and more difficult to read. Use a wider range of line colors in plots 8, 9, 11, 13, 14, 15, 16, 17 and 18 to make them easier to read.

33. I find the ticks on the log scales difficult to read. Do they correspond to 2, 4, 6, and 8? Please indicate this in the figures or use the more conventional format in which all the 8 ticks (2-9) are shown.

34. Figures 16 and 18: the y-axis label is confusing. Simply ‘%’ would be better since the capture gives the details.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 24499, 2010.