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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Wind speed dependent size-resolved parameterization for the organic enrichment of sea spray" *by* B. Gantt et al.

Anonymous Referee #1

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General

This study addresses the atmospherically important production of organic matter from oceans. It takes into account to study locations in Point Reyes (Pacific) and Mace Head (Atlantic) with different measurement records and quatities to derive an empirical relationship between a) the organic matter in sea salt aerosols (OM_{ss}) and the surface wind speed b) between the particle diameter at 80% relative humidity and the OM content. The authors explain clearly the formation of an organic film layer at the oceans surface that becomes disturbed by the surface wind causing Langmuir circulation pattern at elevated wind speed that forcing the film to break up. In order to develop an equation applicable for the global simulation the authors distinguish three different sections, i.e.

(i) calm wind speed less than 3 m $^{-1}$ (slick possible), (ii) intermediate surface wind speed up to 10 m s^{-1} (non-slick microlayer) and (iii) higher surface winds with only little enrichment. They derived a curve fitted algorithm including the surface wind speed and the chlorophyll-a concentration. The first is to cause the sea spray to develop and the second is being used as a marker for biogenic activity that can be monitored from space. Gantt et al. also derived a size dependent OM contribution as a function of particle diameter with a similar method. This summarizes in the globally estimated primary organic aerosol (POA) emission from sea surface of 2.8 to 5.6 Tg C/year.
In general this study is clear an should be accepted after minor changes addressed in the specific comments below.

Specific details

- Data reduction (filtration) due to meteorology at Point Reyes was quite substantial: Only 10% was left!
- The correlation of surface wind speed with OM_{ss} depends essentially on the height of measurement. This becomes really obvious in Figure 2: The Mace Head data for wind speed taken 10 m a.s.l. display a much worse correlation than the Point Reyes data taken at 4 m a.s.l. The authors tried to cope with that by applying scale laws. Are there any short term measurements supporting that, because that assumption is very critical on the general results!
- A second critical point to make is the use of primarily shoreline measurements and their application to the ocean in general. This seems partially supported by the rare ship measurements. But that is not always the case. Sciare et al. (2009, J. Geophys. Res.) found a different behaviour of chlorophyll-a and OM than anticipated by this study. But one could use Amsterdam island as a much better value for the open ocean than Point Reyes or Mace Head, which are excellent

for shorelines without any doubt. Amsterdam island isn't perfect to. But this only questions the applicability of specific site measurements to a broader range.

- The chemical composition is approximated sometimes in a challenging way. Surface fluxes are processes in th eorder of seconds to minute time scale. The average chemical composition was gained for Mace Head in a resolution between 50 to 100 h (2-4 d). A correlation should be made only tentatively or investigated in a shorter timescale. I am aware that this addresses sensitivities of instrumentation etc. But in any case both measurements (wind and chemical composition) should be in closer time steps in order to allow a robust transfer of results. Interesting in this case might be the intercomparison of Point Reyes (24 h) and Mace Head data (2-4 d). This aleady might give an indication. This is partially obvious from Figure 2.
- This shows up on p. 10532 were I do not understand the paragraph "To be consistent with the Mace Head data, it was assumed that 70% of the OC measured at Point Reyes is insoluble. This WIOC/OC fraction was similar to that observed at Mace Head (Cavalli et al., 2004; O'Dowd et al., 2004) and Amsterdam Island (37.80 S, 77.57 E) (Sciare et al. 2009) during the summer when OC concentrations were highest." Was this a matter of purpose? Please reformulate to prevent that the number taken for Point Reyes is just a tuning to fit the expectations. This is surely not the purpose.
- Rinaldi et al., 2009: This study indicates a good match between open ocean and shore lines at medium particle sizes but higher production rate at shore lines for smallest and large particles. Thus, at the open ocean aerosols are less! Especially the large aerosol production rates will influence the global emission number given. What is the uncertainty?
- p. 10538: The density of organic compounds can vary significantly between 800 and 1900 kg/m^3. How does this alter your results because you assumed a value

of 1000 kg/m^3 (water)?

- Emitted as sea spray the OM compounds and aerosols should be in a liquid solution nearly their entire atmospheric lifetime except in extremely rare conditions (rel. humidity < 30%). Thus no activation is required anymore. How does that affect radiation effects? How do the distributions of OM and sea salt match with the occurrence of clouds and their properties or the properties of oceanic aerosols?
- Give numbers when intercomparing to previous studies (Spracklen et al., 2008; Vigniati et al., 2010). This would allow a classification of your new results to any reader. Indicate strengths of the new results and weaknesses. Mark regions of mismatch and potential regions for required measurements in the future.
- If possible make a list of uncertainties for the global estimate and split this up into the uncertainties of individual assumptions made.

Final

If especially the last two points will be addressed, the present study will be cited frequently and would serve as a

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