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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effect of primary organic sea spray emissions on cloud condensation nuclei concentrations" *by* D. M. Westervelt et al.

Anonymous Referee #2

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Westervelt and co-authors present a study of the effect that the primary oceanic organic aerosol emissions have on cloud condensation nuclei (CCN). The authors made model simulations that included aerosol microphysics and compared three (main) different scenarios: one with the ocean emitting only sea-salt as primary aerosols, and two more where the primary aerosol fluxes also include marine organics. Only in one of the latter two simulations the organics were allowed to act as sufractants. Two more sets of simulations studying the effect of the hydrophobic-to-hydrophilic conversion have been performed, and found no sensitivity on CCN concentrations. The main conclusion of the study was that the inclusion of organics in primary oceanic aerosol does not change CCN significantly, neither in the regional nor the global scales.

The work presented is of potential great significance and the reader does expect a lot

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while reading the manuscript. Yet, the few results presented are very briefly analyzed and even less validated. The approach in calculating the oceanic enrichment of sea-spray is not the most current one, since Vignati et al. (2009), already cited in the paper, updated the O'Dowd et al. (2008) parameterization. That should not affect the conclusions in any significant way, but the most resent parameterization must be used. Also, the validation of the model is not properly done and some generalizations are unjustified. I do not suggest publication to ACP in the present form, since the results presented are neither convincing nor properly analyzed.

The authors presented results in a couple of places selecting arbitrary examples. Although the cases selected are justified, they should present additional information for more cases and/or wider regions, for the reader to be able to understand how the conclusions drawn change, primarily in space. The results presented at the end of paragraph 2.3.1 are for 15% enrichment, while the parameterization used allows (and this is expected to happen as well) for enrichments higher than 50%. In these cases, the effects on CCN and kappa of the mixed particle are expected to be augmented. In addition, the authors arbitrarily selected a single grid point (45S, 0E) for the discussion in paragraph 3.4. I would strongly suggest extending this discussion in larger areas, or with additional representative grid points.

The aging was found to be insignificant, at least for the CCN activation. Still section 3.1 and table 3 should have some numbers from these simulations, at least the aging budget (middle of table 3). Further, having internally mixed aerosols should affect the solubility of the mixed sea-spray particles considerably. This is not evident in table 3, where the lifetimes are listed: sea salt lifetime hardly changes, while the lifetime of hydrophobic OC increases by about 15%. Is solubility of the mixed particle being affected in the same way as kappa?

One of the weakest parts of the manuscript is the comparison with measurements. Acknowledging that remote oceanic organic aerosol measurements are extremely sparse, one should note that they do exist. Out of the three stations mentioned in the paper,

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one of them (Azores) was not studied without any given reason. The second, Mace Head, is very difficult to compare with, since (to my understanding) the measurements were performed only when clean air masses were influencing the area. If the authors compared the model's monthly mean concentration with Mace Head's data, the comparison is not valid. For Amsterdam Island, they had a bad correlation without primary marine organics (underestimation), and they ended up with a bad correlation (overestimation). There is no improvement here, and no discussion why the model has improved, even with this overestimation. It was mentioned that the present study has a factor of 2 higher fluxes than previous studies. If one assumes for a minute that this factor of 2 is a real overestimation, the factor of 5 overestimation of Amsterdam Island will become something like factor of 2.5, which is still large. It was not mentioned though that Roelofs (2008) had almost a factor of 5 higher marine organic fluxes compared to this study.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5757, 2011.