**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Global distribution and
climate forcing of marine organic aerosol – Part 1:
Model improvements and evaluation" *by*
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Anonymous Referee #2

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This manuscript describes the inclusion of primary and secondary sources of marine organic aerosol into a climate model to investigate their impact on cloud condensation nuclei concentrations. Two recent parameterisations for marine primary organic aerosol (POA) emission have been implemented and compared, in addition to emissions of oceanic gas-phase organics that are thought to contribute to marine SOA production. The effects of these emissions on CCN are evaluated in the model, considering both externally and internally mixed inclusion of the emitted POA with sea salt. The paper represents an advance in this active research area, providing the first attempt at quantifying the CNN impact of marine OA based on recently-derived emission parameterisations. However, there are a number of issues that should be addressed

or clarified before the paper can be considered for publication in ACP.

The description of how the marine POA emission mechanisms affect the emitted aerosol size distribution should be made clearer (Sec 2.2.1). If I understand correctly, OM_ss provides a size-dependent organic mass fraction of sea spray. Do the sea spray parameterisations implemented in the model (Martensson 03, Monahan 86) account for sea salt only, or for total sea-spray? If the latter, then a fraction of mass emitted should be partitioned as POA according to OM_ss, thus reducing the mass of sea salt emitted (if this was previously the only assumed component of 'sea spray'). If the former, then POA should be added as extra mass, according to OM_ss, as a fraction of the total sea salt + POA mass (as has presumably been done here). The authors should clarify that the approach used is consistent with the sea-spray parameterisation used. For the internally mixed case where POA mass is added to the distribution, it is stated that addition of mass leads to ∼10% increase in mean modal diameter at OM_ss=0.5. It would be useful to know more details of the change across the 4 model size modes resulting from this increase in mass. Since changes in CCN are the focus, these details are crucial to the conclusions.

In the comparison of CAM5 with observations, it is stated that "Global climate models like CAM5 give an average realization of the atmospheric state..." (Page 18870). This is only the case if they are run for several years - model internal variability would be expected to lead to differences in a simulation from year to year. It is stated that the model has been run for 5 years in each scenario, but it is not explicitly stated whether 5-year average fields are shown and used to compare with observations. In addition, it is important to show the variability over the 5-year simulation, and how this compares with the magnitude of differences shown between the model scenarios (this should be shown on Figure 2c). The statement that "Long-term observations of marine organic aerosols available from these two sites smooth out day-to-day variations and therefore are suitable for judging the accuracy of CAM5..." is somewhat misleading in the context of these comparisons. The difficulty in comparing with CAM is not necessarily day-to-

day variability, but more interannual variability. Presumably the observations are still for specific years (although details are not provided), and therefore caution should be taken when comparing them with a free-running climate model. According to Figure 2C, the model appears to have difficulties in reproducing the shape and magnitude of both POA and SOA observations at the two sites. It is important to clarify whether this is a result of model internal variability (i.e. has only one year been shown?), or if this is the average of 5 year simulation, the variability around the mean should be shown. i.e. is this representative of the model skill on average, or does the model do much better for some years (due to e.g. variability in windspeed)? It would be useful to quantify the agreement between model and observations from the different simulations (e.g. model mean bias - see also specific comment below).

Other comments

- Page 18861, line 23: Remote-sensed chlorophyll data - please provide more information on the source of this (SeaWiFS?, MODIS?, years used).

- Page 18870, line 27/28: Underestimation of oceanic POA. It is not clear what potential problem is being highlighted by "averaging over a coarse model grid". Does this refer to problems in averaging fine-scale atmospheric features in the model, or does it refer to errors in emissions resulting from use of coarse grid-scale wind speed?

- Page 18871, lines 11/12. Please provide some reference/evidence to support the statement that "Amsterdam island can be considered to be representative for marine background".

- Page 18871, line 16: "significant improvement" of model OA. Please provide a more quantitative measure of the improvement (e.g. model mean bias compared with observations).

- Page 18871, lines 27/28: Possible underestimation of phytoplankton emissions of monoterpenes and isoprene. The authors have simply cited Luo and Yu, (2010), but a

little more detail on what the evidence is to support this possible underestimation would be beneficial.

Typographical errors

Page 18856, line 12: "important for *the* global CCN budget".

Page 18871, line 11: "over Amsterdam Island" not "over the Amsterdam island".

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 18853, 2011.