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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global distribution and climate forcing of marine organic aerosol – Part 2: Effects on cloud properties and radiative forcing" *by* B. Gantt et al.

Anonymous Referee #1

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The manuscript presents a modeling study of the effect of marine OM on cloud properties and the estimated aerosol indirect forcing. It is one of the first studies to do so, as most previous papers have not looked beyond the effect on CCN, and is as such well worth publishing. My main criticism is that based on the noisy-looking results (e.g. Fig 2) and the analysis of statistical significance (Fig. S1) it seems that the modeled time period may be too short to draw any real conclusions on the impact of marine OM on clouds. Furthermore, the authors do not put their results into a wider context and the methodology should be described in a bit more detail. These flaws can, however, be relatively easily fixed, and therefore I recommend publication after the following comments have been addressed.

1. As I already said above, the noisiness of the results (Figs 1c and 2) indicates that the model run may not be sufficiently long. This is further supported by Figure S1 which shows the areas where the modeled changes are statistically significant (using a rather high threshold p value of 0.1!): the model grids showing significant change are very scattered (the main exception being the Southern Ocean) and also frequently show up over the continents thousands of kilometers from the nearest oceans. In order to draw solid conclusions based on these simulations one should be able to explain why statistically significant changes take place in the areas they do, and not in others, but clearly this is impossible based on the current results. This leads me to think that much of the statistically significant change is in fact due to model noise. Specifically, the current results (Fig. S1) do not really support the statement that clouds in the North Pacific and Atlantic Oceans are sensitive to OM any more than e.g. parts of the Indian Ocean.

I strongly recommend that the authors extend their key simulations for at least another five years (this probably isn't necessary for all the sensitivity simulations). While this will require a lot of computer time, I do think it necessary in order to draw robust conclusions. Furthermore, reanalyzing the extended simulation data set should be relatively straightforward and not take up too much time.

2. The introduction and methods sections are very short and lack in detail. I understand that the authors do not wish to repeat long sections of Part 1 of the manuscript. However, both parts should be able to stand alone in the sense that the readers must be able to assess the validity of the basic methodology and the significance of the results without referring to any other paper for information. Because of this, the introduction section should be expanded to summarize the main findings of previous studies on this topic and to highlight what is the added value of this particular study. Furthermore, a short review of the CAM5 model version used as well as the simulations performed in Part 1 (and used here in Part2) should be given.

3. The authors mention briefly that CDNC and other cloud properties show similar

seasonality to Chl-a at some high-latitude areas. Given that the seasonal variation of Chl-a tends to be very large in these latitudes, it would be very interesting to see an additional figure on the seasonal changes in either CDNC or radiative forcing.

4. Overall, the introduction of the specific simulations in Section 2 could be made a bit easier to follow (e.g. give simulation name in text when specific sensitivity tests are discussed). The same goes with Table 1 which is now split into two parts (1a and 1b). In reality, many readers scan the tables and figures before deciding whether to read the text at all, and in such cases the current legends are confusing (not explaining why the simulations in the Part 1 paper are presented here). Either combine tables 1a and 1b or modify their legends (e.g. 1a: "— simulations run for the Part 1 paper and further analyzed here in the Part 2 paper" and 1b: "— additional CAM5 simulations —"). Indicate in Table 1a also the default activation parameterization against which FN parameterization is tested.

5. Section 3.1.1: While it is clear based on Table 2 that LWP is not kept fixed in these simulations, reading through section 3.1.1 one might be left with this impression (see first sentences of this section). Consider reformulating.

6. Section 3.2.1, comparison to Meskhidze and Nenes (2006): It is speculated here that the difference in magnitude of SWCF is likely due to model grid, selected period and averaging. Given that the only differences between the simulations compared in Fig 2 (Default and G11) are marine POA, SOA and MS-, I would expect DMS to play a role as well.

7. Section 3.2.1, p. 5, "Ghan et al (2011) showed —": Give the same accuracy for all the values, ie. the difference of -1.60 and -1.76 should be 0.16, not 0.2 (or alternatively -1.6, -1.8 and 0.2).

8. The experiments using Vignati et al. (2010) parameterization are not discussed in the text and can thus be omitted.

ACPD
12, C2453–C2456, 2012

9. Give the names of the simulations compared in Fig 2 legend.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7453, 2012.

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12, C2453–C2456, 2012