

Interactive comment on “Global distribution and climate forcing of marine organic aerosol – Part 1: Model improvements and evaluation” by N. Meskhidze et al.

Anonymous Referee #1

Received and published: 2 September 2011

This manuscript describes an attempt to include marine organic aerosols in a climate model and investigate their impact on climate forcing. Overall this represents an important effort and the investigators have clearly dedicated significant thought to the approach to modeling these aerosol. In particular, the effort to distinguish the impact when internally or externally emitted assumptions are applied is quite informative.

My major critique of the work is that the authors fail to address whether any of their results are statistically significant. It is common practice to run climate simulations for at least 10 years (rather than the 5 shown here) to estimate the impact of climate variability on any results, particularly any estimated differences between simulations.

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Thus, the results relevant to Figures 5, 6, and 7 need to be discussed in light of what differences can simply be expected from climate variability and whether any of the regional differences apparent in these figures are in fact “noise”. Five years of simulation is insufficient to judge this, thus the authors need to extend their simulations.

Some additional comments and suggestions are given below.

ADDITIONAL COMMENTS:

1. Abstract, lines 17-19, and page 18876, lines 15-20: This rosy interpretation of the model-measurement comparison is not supported by the analysis shown here (Figure 2), where WIOM is at least a factor of 2 off at both sites. The manuscript should clearly indicate that more work is required to improve the model simulation and/or compare it more extensively to observations.
2. Abstract, line 25: grammar “increases and decreases”
3. Page 18856, line 12: grammar, “important for the global”
4. Page 18856, line 15: grammar “and the lifetime”
5. Page 18856, line 23: grammar, “the resulting reflectivity”
6. Page 18857, lines 20-21: Do Ma et al., estimate a radiative forcing or a radiative effect of sea salt?
7. Section 2.1: The model description fails to describe how particle deposition is treated in the model.
8. Section 2.2.1/2.2.2: The source of the chlorophyll-a data or the “phytoplankton abundance” used is never described. Satellite products?
9. Page 18862, line 8: grammar “to be consistent”
10. Page 18863, lines 4-9: Could you give some numbers for the mean aerosol concentration differences over the ocean?

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11. Page 18864, lines 20-25: I assume the SOA is formed irreversibly? Please clarify.
12. Page 18870, lines 15-17: Perhaps it would be useful to justify here why simulated POA is compared to WIOM and SOA to WSOM and what uncertainties are associated with this assumption?
13. Page 18871: The discussion here of the challenges of comparing observations with a climate model, suggests that simulations with assimilated meteorology (which is possible with CAM) should have first been performed to validate simulated aerosol concentrations against a larger suite of observations before the climate model simulations described here. Assuming that the authors do not want to undertake this effort at this time, they should clearly indicate that comparisons in a CTM are required to evaluate the proposed aerosol schemes.
14. Page 18874, line 20: grammar, "such" out of place?
15. Page 18874, line 21: "enhance the CCN budget"
16. Page 18875, lines 4-5: presumably the relative differences in the polar regions are due to very low CCN concentrations?
17. Page 18877, lines 2-3: Could the authors elaborate on "the experimental evidence"? Are they primarily referring to their own work (Meskhidze and Nenes) or others?

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