

Interactive comment on “OCEANFILMS sea-spray organic aerosol emissions – Part 1: implementation and impacts on clouds” by Susannah M. Burrows et al.

Anonymous Referee #2

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The manuscript titled “OCEANFILMS sea-spray organic aerosol emissions – Part 1: Implementation and impacts on clouds” by Burrows et al. describes the implementation of the OCEANFILMS parameterization in an earth system model, discusses the results from a set of sensitivity simulations with varying assumptions about the emission state of marine organic aerosol, and examines the impact of such aerosol particles on cloud properties. Two different aspects of the emission state are examined: whether the emitted organic aerosol is internally or externally mixed, and whether it replaces or adds to the amount of sea salt emitted. Internally mixed organic aerosol that adds to the sea salt emission is concluded to agree best with observed seasonal cycles, and particles emitted under such assumptions is found to induce a significant increase in

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the in-cloud cloud droplet number concentration, particularly over the southern ocean.

The manuscript is a valuable contribution to the field, as OCEANFILMS is the one of the only physically based parameterizations for organic sea spray aerosol emission, and advancement in its implementation and development is beneficial for subsequent studies. The study also addresses an important open question concerning marine organic aerosol, namely the state in which these particles are emitted, and shows interesting results. Together with its thorough consideration of previous studies on the subject, the manuscript is thus recommended for publication after addressing some comments as suggested below.

Minor comments:

1. Section 3.1.1: is it already conclusive from experimental observations that marine organic matter are internally mixed with sea salt? If so, what's the argument for investigating this aspect in this study?
2. There are quite a lot of nice results, but the interpretations are sometimes lacking. For instance for table 4, the text in the manuscript did not provide any interpretation or connections with the rest of the study beyond stating the values already listed in the table. Figure 8 is another example where perhaps more can be said.
3. Page 13, line 12: any speculations for the different findings in the Southern Ocean and the Arctic? In particular, the correspondence between CCN and IWP changes is not as clear in the Arctic as over the Southern Ocean. Is it still valid to claim that an increase in CCN concentration results in an increase in IWP in the Arctic?
4. Second paragraph in section 4.3.1: does cloud areal fraction include the temporal component? If not, does that change? (cloud lifetime, occurrence frequency)

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5. To what degree can the results found in this study be generalized to other models?
6. Related to the previous question, but particularly relevant for conclusions drawn with regards to the impact on clouds: how does E3SM compare to other models and observations in terms of its global aerosol distribution?

Technical comments:

1. The use of acronyms can be more consistent (they are often introduced but not consistently used thereafter). e.g. MOA, Nd, experiment short names (especially in subsequent figures)
2. Figure panel titles can be more useful.
3. One of the main strengths of OCEANFILMS is its physically-based approach. It requires therefore fields from an ocean biogeochemistry model. Despite this, the current study which is advertised as an implementation of OCEANFILMS into an ESM, only utilizes atmosphere-only simulations. This is a reasonable choice for the purpose of performing sensitivity studies, but it raises the question whether OCEANFILMS can indeed be run with current state-of-the-art ESMs or if a more sophisticated ocean biogeochemistry model would be needed. Perhaps it would be good to mention this in the manuscript, also providing information about which ocean biogeochemistry model was used in the current setup (page 5, line 21).
4. Section 3.1: there are 4 sensitivity cases, but only the two mixing state options are explained.
5. Section 4.3: the title states that this section focuses on the INT_ADD simulation, but the content includes discussions of results shown in Figure 3 for the various sensitivity simulations. In general the structure of the results section is not

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very consistent, especially with Figure 3 already showing interesting results concerning the impact of marine aerosol on clouds that are worth discussing. The structure of the sections are however divided such that the impact on clouds is only meant to be discussed for the INT_ADD case. Maybe some restructuring of the results section could be beneficial. Or change the contents of the sections to be consistent with the titles.

6. Page 2, line 10-11: "to the total of and..."?
7. Page 3, line 11: "or" → "of"?
8. Page 7, line 12: MOA density reference?
9. Page 8, line 3: it has been suggested "that" ...
10. Figure 7: significance test in shading?
11. Figure 8: shading in top panels missing?
12. Bibliography errors: spurious "{" brackets (page 24 lines 7, 11, 29), author list on line 18 (which is in review from 2016?), title of companion paper also confusing (page 22, line 30: also Part 1?)

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