

Interactive comment on “A comparison of sea salt emission parameterizations in Northwestern Europe using a chemistry transport model setup” by Daniel Neumann et al.

Anonymous Referee #3

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General Summary:

This article by Neumann and coauthors describes a modeling study in which several sea salt emission parameterizations are evaluated for Northwestern Europe during winter and summer months. While the study does a comprehensive comparison of the different parameterizations, evaluation of the parameterizations with observational data is rather limited. As this region has recently been shown by the same authors to have an important interaction between sea salt and nitrogen deposition, evaluating and improving the sea salt emissions in the model is important. I'd recommend publication in ACP after the following comments listed below have been addressed.

Major comments:

C1

1) I consider the Sect 3.2.2 the weakest part of the article due to the limited observational dataset of particle size distributions. Without size resolved observations besides coincident PM_{2.5} and PM₁₀ sodium concentrations at one site, little evaluation of the particle size distribution predicted by these emission parameterizations is possible. I'd suggest limiting discussion in Section 3.2.2 to the Melpitz site.

2) I am concerned about comparison of model predictions and PM₁₀ observations throughout the article. Depending on the coarse mode GMD and standard deviation, it seems possible that a significant fraction of the mass could be in particles greater than 10 micrometers in diameter. In addition to describing the environmental conditions of the PM₁₀ measurements (i.e. ambient vs standard temperature and pressure), I'd appreciate the authors analyzing the fraction of the predicted coarse mode that may not have been measured at the various sites. If significant, the authors should consider providing additional statistics for the model-predicted PM₁₀ sodium.

3) In light of the limited observational dataset beyond sodium PM₁₀ concentrations, I'd suggest the authors explore additional datasets that can be used for model evaluation such as satellite-derived aerosol optical depth. Given the large mass and number emission differences between the parameterizations, it may be possible to evaluate whether the associated AOD changes bring the model into better agreement with the satellite-derived values particularly over the Atlantic Ocean.

Minor comments:

1) Page 1, Line 15: should be "coastal"

2) Page 1, Lines 16-18: I don't think the accuracy of fine and coarse mode predictions can be adequately determined by the available dataset and I'd suggest removing or changing the sentence to discuss the comparison between parameterizations.

3) Page 2, Line 25: should be NO_x and SO₂ where the x and 2 are subscripts

4) Page 3, Line 2: change to something like "This parameterization has not been used

C2

in a CTM setup in the study region."

- 5) Page 3, Lines 4-9: please expand the discussion of previous sea salt studies in this region
- 6) Page 3, Figure 1: consider removing the blue and yellow colors denoting the ocean and land
- 7) Page 3, Sect. 2.1: please state whether dust emissions were active in the model and whether they have an impact on sodium concentrations
- 8) Page 7, Figure 3: consider using the same x- and y-axes as Figure 2 and give the wind speed used to determine the size distribution
- 9) Page 9, Figure 4: please fix the label for the Reynolds number that is cut-off
- 10) Page 9, Figure 5: please explain here or in the text why these three stations were selected as representative stations
- 11) Page 30, Table 3: consider grouping the stations by coastal and inland sites

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