

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

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Response to the comments of Reviewer #3

We thank the reviewer for the constructive comments on the manuscript.

Major comments

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,

C1

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.

> Section 3.2.1 was shortened. The parts on the Waldhof station and on the geometric mean diameters were removed. The figures were moved into the Supplement. The part on model results at Westerland was shorted but kept because it highlights how the sea salt particle size distribution changes from the coast to the hinterland (at least in the model).

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.

> Instead of comparing the total model particulate sodium mass with sodium PM₁₀ measurements as done in the reviewed version, the evaluation was repeated with modeled sodium PM₁₀ concentrations. The modeled PM₁₀ concentrations were calculated on the base of the modal distributions (see new Appendix D). As a result, the overestimations in the SP13 and GO03 cases were considerably reduced.

> Unfortunately, no measurement data on ambient temperature and pressure were available via the EMEP database. Therefore, no evaluation of these parameters was possible.

C2

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.

> A section on the comparison with AERONET aerosol optical depth (AOD) measurements was added (Sect. 3.4). We decided against a comparison with satellite-derived AOD data because we are inexperienced in the usage of satellite products and pitfalls with respect to these. > Additionally, we see no added value in adding a comparison with satellite based AOD data because the deviation between the AODs of the model cases is considerably lower than the differences between model and measured AODs. One can expect similar results for spatially resolved AOD data.

Minor comments:

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

> corrected

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.

> removed

C3

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

> corrected

4) Page 3, Line 2: change to something like "This parameterization has not been used in a CTM setup in the study region."

> modified accordingly

5) Page 3, Lines 4-9: please expand the discussion of previous sea salt studies in this region

> A comparison of the findings in this study with the findings in some of the listed studies was added in the end of the Sect. 3.2.1 and in the end of the new Sect. 3.3. Presenting these studies' results in the Introduction would have considerably extended the Introduction section.

> Results of a recently in ACPD published study (Chen et al., 2016) were included.

6) Page 3, Figure 1: consider removing the blue and yellow colors denoting the ocean and land

> removed

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

C4

> Dust is not considered. A paragraph was added: “Dust was neither included in the boundary conditions nor in the emissions. The dust concentrations in Northwestern Europe are low compared to sea salt and anthropogenic particle concentrations (Cuevas et al., 2015). Moreover, in episodes with high dust loading, Sahara dust is commonly transported in higher atmospheric layers across Northwestern Europe (MACC II data, obtained from <http://macc.copernicus-atmosphere.eu>).”.

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

> modified accordingly

9) Page 9, Figure 4: please fix the label for the Reynolds number that is cut-off

> The label is fixed.

10) Page 9, Figure 5: please explain here or in the text why these three stations were selected as representative stations

> A sentence was added: “The station of Westerland is located directly at the North Sea coast, the station of Zingst is located at the Baltic Sea coast, and the station of Waldhof is located approximately 200 km inland. Hence, these stations’ measurements cover three different sea salt emission regimes.”

11) Page 30, Table 3: consider grouping the stations by coastal and inland sites

> grouped as requested (in all tables)

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