

Interactive comment on “Sensitivity of modeled atmospheric nitrogen species to variations in sea salt emissions in the North and Baltic Sea regions” by D. Neumann et al.

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Response to the comments of Reviewer 2

We thank the reviewer for the constructive comments on the manuscript. Please find the revised version of the manuscript (acp-2015-758-manuscript-rev01.pdf), the revised version of the manuscript with marked changes (acp-2015-758-manuscript-rev01_diff.pdf), and the revised supplement (acp-2015-758-supplement_text_rev01.docx) in the attached zip file.

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1) There are, however, few points that need improvement, especially, the title, which doesn't seem to represent the main outcome of the paper.

> We removed the section on corrected sulphate, added a section on nitrogen deposition, and changed the title from “*Sensitivity of modeled atmospheric nitrogen species to variations in sea salt emissions in the North and Baltic Sea regions*” to “*Sensitivity of modeled atmospheric nitrogen species and nitrogen deposition to variations in sea salt emissions in the North and Baltic Sea regions*” - following also the recommendations of reviewer #1. Please see our answer to question (3) of reviewer #1 for our reasoning.

2) My major concern is with the capping of surf zone, although, there are some explanations why it is needed, but physical meaning is not presented. At least, I don't see it. OK, concentration increases without capping, but authors had not shown that it is bad or inconsistent with the measurements. There is no explanation for using the specific 0.47% capping either. Why this value was selected, from which measurements/considerations? How it is applicable to other regions? Removing the capping might change the conclusion of surf zone having no effect. Certainly, more arguments based on data are needed here.

> Considering the Dutch, German, and western Danish coast (see new Fig. S2), the capping is applied only in a low number of coastal grid cells. The impact on atmospheric particulate sodium concentrations is negligible at all considered EMEP stations except at the Danish station Ulborg (DK0031R) as indicated by Fig. S3. Therefore, the capping is not relevant for this study's analysis. However, there are coastal regions in the model domain, in particular the Norwegian Atlantic coast, where the surf zone size is considerably overestimated.

> Surf zone emissions are complex to estimate because they depend on the length of the coast line but also on the wind direction with respect to the coast line, on

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coastal features (sandy beaches and steep cliffs), and on the offshore area (long flat bathymetry or islands). Additionally, there are large river estuaries and fjords that have a long coast line. However, their coast line exhibits lower emissions because it is protected against wind and long waves. The idea behind the applied capping was to choose a coast line length that could be completely unprotected from the wind. Figure S1 shows a simple geometric coast line that was chosen for this purpose leading to the 0.47% of surf zone per grid cell (24 km grid). We cannot validate this approach and we do not consider this approach as the correct one - but we see the necessity to cap the surf zone size at some threshold. However, we consider the calculated capping surf zone size to be in the correct order of magnitude. From the authors' knowledge there are no other approaches available for calculating the surf zone size that do avoid the described problem with protected coast lines.

> We cannot make any statement on the applicability of our approach to other regions.

3) Page 29711, line 13: spume drops are torn by wind and splash drops are created by breaking waves, I'm sure author knows that, but it should also be consistent in the paper (switch places in the text).

> We switched it.

4) Page 29714, line 15: I would disagree, NaNO₃ would give Na, but won't be a 'pure' sea salt, rather processed or aged sea salt.

> We wanted to express that the sodium mass does not change in the aging processes of sea salt. Therefore, sodium concentrations are the best tracer for atmospheric sea salt.

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> We changed "*Sodium cations (Na⁺) represent pure sea salt. They are considered for evaluating sea salt particle predictions.*" to "*Sea salt emissions are the major source of atmospheric sodium cations (Na⁺). Na⁺ does not evaporate from sea salt particles in contrast to Cl⁻ and it does not condense onto particles in contrast to HCl and H₂SO₄. Therefore, Na⁺ is a good tracer for sea salt particles and is considered for evaluating sea salt particle predictions.*"

5) Also, line 17-18,: SO₄ resulted from DMS can dominate total SO₄ in some regions (Antarctic or North Atlantic Ocean), be more specific and present references.

> We removed the results on SO₄²⁻ and all related parts from the manuscript due to recommendations of reviewer #1. Please see our answer to question (3) of reviewer #1 for our reasoning.

6) Page 29716, Lines 15-16: It is not clear, why surf zone emissions lead to a reduction in the modeled concentrations, I would expect opposite?

> Yes, it is the opposite. During our internal revision we accidentally switched the meaning. We changed "*Surf zone emissions lead to a reduction in the modeled concentrations most of the time.*" to "*Deactivating surf zone emissions leads to a reduction in the modeled concentrations most of the time.*"

7) Page 29716, line 20 and figure 5: It is not clear what orange line, is it orange stars?

> Yes, it should be orange stars. We changed "*The orange line represents a simulation without salinity-dependent scaling of sea salt emissions.*" to "*The orange asterisks*"

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represent a simulation without salinity-dependent scaling of sea salt emissions denoted as the full case.”

8) Tables 3, 4, 5 and Figures 6, 7, 8: it is not an addition of xSO₄/sNH₄/ etc., but for xSO₄/sNH₄. .

> We switched “with the addition of” to “showing” in the captions. The caption of Fig. 6 was further modified in order to avoid the doubled use of “showing”.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/15/C12201/2016/acpd-15-C12201-2016-supplement.zip>

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 29705, 2015.