

Response to Anonymous Referee #2 comments on “Updating sea spray aerosol emissions in the Community Multiscale Air Quality (CMAQ) model version 5.0.2”

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This paper describes an update to the SSA emission algorithms for the widely used open access CMAQ model and compares model simulations of atmospheric particle distributions to 3 observational datasets. The authors summarize existing models and use observations to evaluate various model approaches and identify a specific approach for updating the CMAQ model. There are a few points that the authors should consider before the paper should be published in GMD.

**We appreciate the reviewer’s comments and have responded in bold typeset to the individual comments below.**

1. The abstract mentions gas-particle partitioning of nitrate “potentially affecting the predicted nitrogen deposition in sensitive ecosystems”. This is an interesting point but it is not one that shows up much in the following text. It should either be discussed more in the manuscript or removed from the abstract.

**This phrase has been removed from the abstract in the updated manuscript.**

2. The authors note that global SSA emission estimates differ by 2 orders of magnitude but they give no indication of what drives these differences and where the CMAQ model falls within that range of estimates. Is the difference all due to open ocean emissions (which is not the subject of this paper) or do coastal emission play a role in the difference reported for global totals? A comparison with other model results for coastal U.S. (or coastal regions in general) would be useful.

**We agree with the reviewer that uncertainties in the global SSA emission estimates are not directly comparable to uncertainties in regional chemical transport models like CMAQ and have adjusted this statement to the following in the updated manuscript: “Model evaluations of SSA emissions have mainly focused on the global scale, but regional-scale evaluations are also important due to the localized impact of SSA on atmospheric chemistry near the coast.” Furthermore, the updated manuscript now includes the following reference to SSA emission updates in WRF/Chem: “Recent updates to the SSA emission parameterization in the Weather Research and Forecasting model coupled with chemistry (WRF/Chem) increased predicted submicron sodium mass concentrations over the northeast Atlantic Ocean by up to 20% (Archer-Nicholls et al., 2014).”**

3. In order to give some confidence that the model predictions should agree with the observations, some information on the accuracy of these measurements is needed. Do the two local datasets agree with the national dataset? There are considerable artifacts associated with analysis of filter samples, such as volatilization of some chemical species that should be mentioned. How do the known observational uncertainties impact the use of these observations to evaluate model performance?

**The updated manuscript now includes the following sentences about BRACE/CSN comparison: “The PM<sub>1.8</sub> sodium concentrations at the BRACE sites were lower than PM<sub>2.5</sub> sodium measured at a nearby CSN site (located at 28.05N, 82.378056W) averaging 0.34 μg m<sup>-3</sup> during the same period but well correlated (correlation coefficients ranging from 0.65 to 0.90) for the 5-6 days of coincident measurements. This CSN site is part of the CONUS model evaluation described in Sect. 3.3.” The following sentence about measurement uncertainties has also been added: “Although we use the filter-based measurements from the IMPROVE and CSN networks and BRACE campaign for direct model evaluation, we acknowledge that they have uncertainties related to instrument sensitivity and volatility (White et al., 2008).”**

4. Throughout the paper, comparison of model and observed is simply indicated as an under (or over) estimate without showing if there is a significant difference or even if it is a relatively small or large difference. It would be useful to provide something beyond just under or over estimate.

**Throughout the updated manuscript, we’ve added additional statistical measures such as normalized mean bias to give context and significance to the reporting of over- or underestimates.**

5. The focus of the paper is on an updated emission model but there are no flux measurements to evaluate these emissions. The authors should make it clear that they are evaluating an emission model, not with emissions, but with ambient concentrations that are controlled by emissions, deposition, transport, and chemistry. The manuscript should provide some background on how well we know each of these other processes and show how that impacts this model evaluation. For example, are the uncertainties in deposition of the same order as the uncertainties in emissions? Could using a different deposition approach change the results and lead you to choose a different emissions approach for the updated model?

**The updated manuscript now includes the following: “A potential limitation of this study is the reliance on ambient surface concentrations in the evaluation of modeled SSA emissions. Although all model processes other than SSA emissions are left constant for the CMAQ simulations listed above, the selection of deposition, transport, and chemistry parameterizations within the model can affect the predicted concentrations. Nolte et al. (2015) found that constraining the aerosol mode widths and enabling gravitational settling for all model layers in CMAQ affected the predicted coarse mode sodium at the BRACE sites. Although changes in the model chemistry would likely have a minor impact on the Na<sup>+</sup> evaluations, future diagnostic evaluations that account for deposition and transport uncertainties are advised.”**

page 3923, line 10: “domian” should be “domain”

**This has been corrected in the updated manuscript.**