

## ***Interactive comment on “Modeling wet deposition of inorganics over Northeast Asia with MRI-PM/c and the effects of super large sea salt droplets at near-the-coast stations” by M. Kajino et al.***

### **Anonymous Referee #1**

Received and published: 21 July 2012

#### Overview

This study evaluates predictions of a regional AQ model for wet deposition, precipitation, speciated particle concentrations, and ozone. Based on differences in model performance for sodium wet deposition and concentration at near-coast and far-from-coast sites, conclusions are drawn about the impact of “super large sea salt droplets” near the coast.

#### General Comments

1. The reference to super large sea salt droplets should be removed from the title. The manuscript does not present direct measurements of such droplets and the mod-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



eling does not simulate the droplets. While such droplets could impact coastal sites as indicated by the authors, this study does not present solid evidence to support the existence of the droplets and their characteristics (e.g., size). In the conclusion section, the authors indicate that the underestimation of sodium at near coast sites was “probably due to the contribution of short-lived super large sea salt droplets”. This type of statement is reasonable in helping to understand a model performance issue, but the lack of direct evidence of such effects is not strong enough to warrant the current title. Also, as mentioned below, the model configuration is not suitable for simulating near-coast processes, and so impacts of these processes should not be highlighted in the title.

2. There is not much detail on the approach to parameterizing sea salt emissions in the study. Was a special treatment of sea salt emissions from the coastal surf zone used? If so, how wide was the coastal surf zone assumed to be? Other modeling studies at finer resolution have found reasonable model performance for sodium predictions at coastal sites when a surf zone parameterization was used, but no mention of surf zone effects is given here. How was the impact of the coarse grid resolution (60 km) on sea salt emissions handled? For example, coastal grid cells would significantly overlap the ocean and the land. The average emissions in such a cell, which might contain a monitor, would be based on a combination of open-ocean emissions, coastal surf zone emissions, and land emissions. It is unclear how to interpret model performance results in such a situation. Why was such a coarse resolution used in this study where the focus of the evaluation is on a relatively small subset of the domain? A nested domain with finer resolution would provide a better resolution of the processes that are being evaluated.

3. The definition of the super large droplets is vague in the manuscript. The authors define “super large” as particles with diameter greater than 10 “or even 100” microns in parts of the manuscript. This definition is unclear. Common sea salt emission parameterizations represent particles with diameters up to 20 microns (e.g., see de Leeuw et

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

al., JGR, 2000, “Production of seas spray aerosol in the surf zone”) without considering the wave-crest tearing process mentioned in defining droplets as “super large” here.

4. More details are needed on the diameter cutoff for the PM and wet deposition measurements. The idea that large sea salt particles would not be sampled with the filter packs but would be sampled with the wet deposition sampler needs to be clarified with details on the sampling efficiency for the measurements.

General Comments: p.1342, line 14: The first reference to sodium on this line should be removed since it states that sodium was successfully modeled, whereas the second reference to sodium contradicts the first by indicating performance issues for sodium

p. 1342, line 26: Should “administrative” be “regulatory” or “air quality management” purposes?

p. 1345, line 2-10. The description of the configuration is confusing. For example, on lines 6-8 it is stated that WRF “or” JMA were used to simulate meteorology, and then the next sentence indicates that WRF was used to simulate meteorology. Please clearly state the configuration for each model run. Also, it is unclear in the upper-left box of Figure 1 which set of analysis data is being passed to WRF and which is being passed to MRI.

p. 1346, line 14: As mentioned above, please provide more details on the sea salt emission parameterization used including at least information related to the size range for particle emissions and how the surf zone was treated.

p. 1347, section 2.2: Please provide more details on the observations including at least information on the diameters of particles and droplets that are sampled. If there are known measurement accuracy issues for sodium (or other species) please note them.

p. 1350, Fig. 4: Why not show a full boxplot for the ozone evaluation?

p. 1351, line 9: Please add some measure of bias, such as normalized mean bias, to table 2.

p. 1351, line 26: The evaluation presented here does not support the statement that long-range transport was simulated correctly. The evaluation would need to consider model performance at various distances from the source region to understand how well transport was simulated. For example, if dust emissions were vastly underestimated in the model or vastly overestimated in the model, the evaluation results presented would take on different meaning. But we have no idea of model performance for dust near the source region.

p. 1352, lines 6-11: The relatively high bias at the far coast sites compared with the near coast sites suggest the model is no transporting sodium correctly. For instance, if the model underpredicted the deposition velocity of sea salt particles, the predictions would be increasingly biased high with distance from the coast as appears to be the case here.

p.1352, lines 21-24: This logic is unclear to me. It seems to suggest that there is no relationship between complex terrain and vertical air motion. Such a statement requires more justification

p. 1353, line 2: How do you know the size of the particles? There are no particle size measurements presented in the manuscript.

p. 1354, line 24: How do you know that overestimation of SO<sub>2</sub> is not due to overestimation of emissions?

p. 1357, line 5: In the definition of coarse particles, do you mean “greater than” 1 micron

p., 1359, line 26. Why is the impact of sea salt on pH so small? Does sea salt effectively contribute a balanced mixture of strong cations and anions?

p. 1360, line 13: The conclusion that transport was simulated accurately does not account for the increasing bias of sodium with distance from the coast.

p. 1363, line 4-5: The sentence “One must . . .” is unclear to me.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

p. 1361, line 19-21: The coarse spatial resolution probably has a much larger impact on performance than the 1 hour time resolution.

---

Interactive comment on Geosci. Model Dev. Discuss., 5, 1341, 2012.

**GMDD**

5, C438–C442, 2012

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C442

