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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling and evaluation of the global sea-salt aerosol distribution: sensitivity to emission schemes and resolution effects at coastal/orographic sites" *by* M. Spada et al.

Anonymous Referee #2

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This manuscript by Spada et al. (2013) presents a computational study that intercompares four marine aerosol production functions and two hybrid functions within a moderate resolution global transport model (NMMB/BSC-CTM) and compares the results to observations of bulk sea-salt mass (based on the U. Miami/Prospero seasalt dataset) and coarse particle aerosol optical depth (AOD) for the year 2006. The manuscript could use some attention in several areas, as outlined below, that border on major revision. But, if the concerns are appropriately addressed, this paper merits publication in ACP.

The authors very early invoke uncertainty of the marine aerosol source term as a motivation for this study. Unfortunately, a sensitivity study such as this does little to address uncertainty without a critical analysis of the validity and impact of all terms in the continuity equation. What are the sources of uncertainty? Based on results in other papers (e.g. Long et al., 2011), the uncertainty can be very large for an emission function; and based on those results, it would seem that the variability seen in this study cannot be considered statistically significant.

For example, based on Fig. 8, it seems all functions vary with a factor of 2-4 of each other in terms of sea-salt mass. Given that your U10 comparison to QuikSCAT had a bias that covers this range, can it be said that any of the parameterizations performed any differently than another compared to observations? What about the uncertainty associated with the functions themselves, or the whitecap scaling parameter? I think when considered together (e.g. error bounds), there is little that can be gained from the information as it is presented in this paper since it would all overlap significantly.

A potential remedy to this is to compare these results to other similar studies of marine aerosol emission functions – as the first reviewer points out as well. Further, as the first reviewer also points out, there are many form which to choose, and many of them use the emission functions used in this study as well. This would at least reveal a sense of the variability associated, for example, with loss terms. Textor et al. (2007), which the authors cite, is a reasonable comparison, but I stress that much has been done since AEROCOM. Expanding Table 6 to include the information from Textor et al., or Pierce and Adams (2006; Table 2) would put your results in good context.

Also, since the authors provide some testing and discussion of the impact of SST on the emissions (based on Martensson et al., 2003), it would be very useful to the community to provide some thorough quantitative analysis of this and how/why it compares to Jaegle' et al (2011). Further, I'm curious why the Jaegle' et al. (2011) SST correction wasn't tested in this as well.

Much of the analysis and discussion of the marine aerosol mass and radiative forcing results and comparison to observations is based on properties of the 'coarse' fraction of the aerosol population (aerosol mass and AOD). Is there no means of comparing the number concentrations to observations? This would put the size resolved sub-micron particle results in some sort of analytical context, which is not done. If not, then it would be reasonable to simply model a few size bins above 1 micron. The rest is overkill, and should be removed from the paper. This includes the discussion of the sensitivity to the number of size bins.

The examination and discussion of modeled vs. observed AOD is very important, and it the most important results presented in this paper. Discrepancy between modeled and observed AOD based on marine aerosol is still not understood. It could use a more thorough statistical analysis, which if done, will alone merit publication of this paper. Further, this discrepancy is one of the major driving factors in the Jaegle et al. (2011) SST analysis, which further backs up the point that this paper should receive far more attention in your work here.

I also agree with reviewer 1 that the New Zealand 'zoom' portion of this study be removed. The analysis is relatively weak and it is unlikely that the higher-resolution would impact marine aerosol populations based on the limited analysis performed against observations of mass and AOD.

Specific comments:

P11607 – L 6: Just as review 1 points out, by using only the SST dependence below 1.4 microns, you are removing any potential variability in the particle range controlling mass, which is the only region where you perform comparative analysis. This has to be corrected and clearly discussed or the whole SST test, results, and discussion should be removed.

Section 5.1.2 This is a useful experiment, but the analysis is weak. Please present some proper statistical analysis of the results. From this, a sense of the model vs.

observed U10 uncertainty can be applied to the aerosol emission uncertainty. Was the bias reduction based on resolution refinement statistically significant?

P11615 – L19-20: Aren't the trade winds actually very humid? Don't they drive tropical cyclones?

P11615 – L25: This analysis should be performed more explicitly and quantitatively. Simply referring to shaded contour maps isn't much use to the reader.

P11616 – L11-14: Why are the peaks overestimated by your results?

P11617 – L23-28+ : A strong correlation is cited but there is no quantitative analysis of this correlation presented. Provide a more robust statistical analysis.

P11618 – L9-11: Do the 10m and 18m wind-speeds compare well enough? A basic calculation using a sea-surface roughness parameter of 0.0002m yields nearly a 1 m/s difference between wind at 10m and 18m. I may be wrong though. It may be worthwhile converting the 18m winds to 10m winds to be sure.

Figures (general): The use of shaded contour maps makes it difficult to get more than a qualitative sense of the regional differences between the functions. Suggest expanding this analysis.

Figs 5,6,7,8,10 – Increase font size. It was difficult to read unless zoomed far into the PDF. Unreadable on paper.

Can spume droplets be mixed high enough to be observed in the U Miami instrumentation? I know that some sites are offset from the surf-zone sufficiently that spume droplets may not reach the sampler.

There are many many typographical and grammatical errors. It became overwhelming to catalog them all for this review. Please carefully review the paper prior to resubmission.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 11597, 2013.