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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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 #1

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The manuscript by Spada et al. discusses the implementation of sea salt aerosol in a global high resolution model and compares the simulated results at two spatial resolutions against surface measurements and coarse mode aerosol optical depth retrievals. Four different sets of sea salt emissions are being tested, two of which are combinations of different parameterizations. The paper is clearly written, and both the explanation of the model setup and discussion of results are clear. I recommend publication to ACP, after addressing the minor comments listed below.

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Interactive Discussion

Discussion Paper

I find the choice of the parameterizations used not very representative of the range of uncertainty. G03 is an improvement of M86, especially with regard to the fine particles which is unrealistically high in M86. Since this is a new model development and not an improvement of a previous (old) parameterization, I find no reason including the M86 calculations in this study. This is also found in the paper (end of section 5.14): the two parameterizations show the same skill, since the only part where they drastically differ is excluded from this study, due to the 0.1um cutoff. Having said that, I believe that the M86/SM93 should be replaced by G03/SM93, and M86/SM93/MA03 with G03/SM96/MA03, unless the authors have a good reason to believe that M86 is better than G03. In addition, I would like to see the Lewis and Schwartz (2004) parameterization as a part of this study, if possible. I also do not understand why the authors decided to exclude the Jaegle et al. (2011) parameterization; one of the major findings of the paper is that the SST parameterization of MA03 worsens the model's skill, thus the inclusion of an alternative SST parameterization appears justified in this exercise. The authors claim that they didn't include this parameterization on purpose (page 11622, lines 18-20), but I do not understand why.

Although there is a fair amount of model-data comparisons, the model's performance needs to be put into perspective of previous modeling studies of the same kind. As an example, the authors could compare their model's performance against Stier et al. (2005), Vignati et al., (2010), Jaegle et al. (2011), Tsigaridis et al. (2013), but there are many more. The scattered comparisons with the AeroCom models, although valid and with value, is too vague and incomplete. Both the data from Table 6 and the comparison with measurements should be compared against other studies in the literature.

The regional zoom over New Zealand (section 5.2 and later) is weak. The authors take almost for granted that their simulation is better, without neither sufficiently describing their regional model and the parameterizations used, nor properly validating the results. In addition, the initialization of zero aerosols at the region boundaries is not a good approach. If based on the mean sea salt lifetime a distance of 400km was

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selected, this means that 1/e fraction of aerosols are missing at the measurements locations. This is not a negligible amount, and by itself is able to explain the simulated concentration decreases. Given the last paragraph of the section which points out that the problem might be mostly the emission parameterizations and not the resolution, I suggest completely removing the zoom discussion (and, thus, the appendix). The paper is strong enough without it, no need to add a largely speculative discussion in it.

Specific comments

Page 11599, line 3: Based on the zoom results, even 0.1x0.1 should be considered as coarse? Which resolution is coarse, based on the authors' interpretation?

Page 11602, lines 6-10: It was mentioned earlier that the direct effect is not yet included in the model, since it is under development. How are the aerosol optical properties taken into account then?

Page 11603, line 25: The Lacis and Hansen (1974) radiation comes from GISS, not GFDL. Maybe the authors mean that they used the GFDL version of the radiation transfer calculations developed at GISS?

Section 2.2 is not needed, the reader can refer to Perez et al. (2011) for the details on dust implementation.

Page 11605, lines 6-8: This sentence makes no sense, since there is no indirect effect calculation included.

Page 11606, lines 6-10: Most (all?) parameterizations are not valid in this size range anyway.

Page 11607, line 6: This cutoff means that coarse particles, where most of the mass is and will dominate in surface concentration comparisons, has no SST dependence? If yes, this should be made clear in the discussion that follows, especially when the SST effect is mentioned.

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Section 3.4: what refractive indices were used? Are the model results used clear-sky or all-sky?

Section 4: Why the Maritime Aerosol Network (MAN) data were not used? They are not continuous measurements, but they cover a big part of the worlds oceans. Since other cruise data were used in the manuscript, I do not understand the exclusion of MAN.

Page 11613, last line: Since the year 2006 was simulated, the information for pre-2000 is not needed.

Page 11614, lines 6-9 are repeated, and can be deleted.

Same page, line 13: “all” should be “any of”

Section 5.1.2 appears to be circular comparison. Since the model uses reanalysis winds, the comparison with satellite retrievals is like comparing the reanalysis, not the model. In addition, isn’t the case that NCEP uses QuickScat in the reanalysis?

Page 11615, line 6: important peaks to what respect?

Page 11616, line 12: overestimate by how much?

Page 11618, lines 9-13: A more detailed discussion is lacking here.

Page 11619, line 13: I do not see a significant influence on the applied emission scheme. The spread appears to be about the same with the measurement error bars, which show the interannual variability.

Same page, line 20: How is the fit weighted?

Page 11620, line 10: Other than the interannual variability range, the absolute uncertainty of the coarse mode AOD product should be mentioned here.

The x-axis of figure 1 is dry or wet size?

The color scale in Figure 4 needs to be modified, it is important for the zero values to be clearly visible.

Figure 5: Please comment on the Arabian peninsula local maximum.

For both figures 5 and 6, all color bars can be deleted, except the ones appearing at the last row.

Figures 9 and 12 have the regression lines forced to pass through zero. This is not a good idea, since it potentially strongly affects the slope of the line. In addition, it implicitly assumes that when measurements have zero, the model also is zero, which is not necessarily the case.

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