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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The sensitivity of Southern Ocean aerosols and cloud microphysics to sea spray and sulfate aerosol production in the HadGEM3-GA7.1 chemistry-climate model" *by* Laura E. Revell et al.

Anonymous Referee #1

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In this study, the authors examine how the aerosol optical depth (AOD), cloud condensation nuclei, and cloud droplet number concentrations calculated in the HadGEM3-GA7.1 chemistry-climate model vary over the Southern Ocean for different sea salt aerosol source functions and DMS oxidation schemes. They compare their results to MODIS and MISR observations of AOD as well as to observations of DMS and satellite-derived cloud droplet number concentrations. The authors find that the model overestimates AOD in winter and underestimates AOD in summer, assigning these differences to an overestimate of sea salt aerosol emission in winter and underestimate

of DMS-derived aerosol during summer. Based on a set of sensitivity studies they recommend a lower sea salt aerosol source function (by Hartery et al., in prep) and the implementation of new DMS chemistry schemes suggested by Chen et al. (2018).

This manuscript is a sensitivity study, as the title indicates. The manuscript is generally well written. The two main concerns I have are: 1) the new source function for sea salt aerosol emissions is based on a study in preparation, and 2) the recommendations of the manuscript are not well justified. I elaborate on these concerns below.

The authors contrast the Gong (2003) source function to one with a windspeed to the power of 2.8 from a study in preparation. Very little information is given from that study, other than it is based on "Analysis of aerosol measurements made on a 2018 Tangaroa research voyage on the Southern Ocean". What type of measurements were these? How was the analysis conducted? How does the new source function compare to observations of SSA mass concentrations available at coastal ground-sites and during other cruises? I have to be a little skeptical of applying the results of one single set of observations in a limited region and season to derive a source function applicable to the entire globe. Furthermore, I am not sure what the policy of ACPD is regarding citations of unpublished results (in prep. or submitted), but many journals do not allow such citations. I am not sure what the course of action is, given that this other study has not undergone peer review and that the current manuscript relies substantially on it.

The authors' main recommendation is "However, given that the chemistry schemes used in the CHEM2 and CHEM3 simulations also show the best agreement with Nd observations, we recommend a combination of the Hartery et al. SSA source function and either the CHEM2 or CHEM3 DMS chemistry schemes for future studies." (page 12). In their recommendation, the authors seem to weight more strongly the comparison to Nd than to AOD. Can they please justify this? Almost no information is provided about the Nd dataset used. The authors reference one paper (Grosvenor et al., 2018) in the figure caption, but that paper discusses multiple retrievals of Nd. What specific

Nd dataset is used, for what year? How accurate are these retrievals (I see not error bars in Figure 10)? How is that Nd dataset compared to the simulations (model sampling)? Also, Figure 10 shows that the Hartery et al. source function (SSF) leads to a decrease in Nd relative to most of the other simulations. I would thus expect that combining CHEM2 or CHEM3 to that SSA scheme would lead to Nd being close to the REF simulation. Hence the comparison between CHEM2-SSF and CHEM3-SSF to observed Nd would not necessarily lead to an improvement. Can the authors add these comparisons to Figure 10 and provide a better justification for their recommendation? As the manuscript stands I am unconvinced that one scheme is better than the other.

Minor comments.

Page 2. Lines 9-10 "...sulfate aerosol are formed from nucleation of sulfur-containing gases". Not all sulfur-containing gases lead to new particle formation. Another pathway is condensation on pre-existing particles.

Page 2. Lines 22-24. In addition to forming SO2, DMS can also form MSA.

Page 5. Lines 13-15 "...emissions of aerosols and their precursors were prescribed based on the year 2000". Is this only for anthropogenic emissions or all emissions? For example, are the SSA emissions based on winds for the year 2000 or do they vary with the actual winds calculated for the specific year?

Figure 1. The latitude range on panel d (60-40S) is different than for all other panels (65-35S), which makes the comparison difficult. Also, the labels on the colorbars do not fall on the discrete colors, making it difficult to relate a given color to a specific value in AOD or AOD difference. The sample problem exists with the colorbars for all the other figures. I suggest that the authors modify the colorbars to make them more readable.

Figure 4. Could the authors combine the 2 panels into one so that the two functions can be compared? Or at least, add the fit from panel a to panel b. Why were the bins

only done for the month of July? The texts mentions that windspeeds and hence SSA are high during all seasons except summer. Does the correlation between windspeed and AOD change for different months? Also, it wasn't clear from the text whether the AOD and windspeeds are averaged longitudinally before doing the binning, or whether individual gridboxes are considered.

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