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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sea-spray geoengineering in the HadGEM2-ES Earth-system model: radiative impact and climate response" *by* A. Jones and J. M. Haywood

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Response to Review by Dr. Hannele Korhonen

We would like to thank Dr. Korhonen for her thoughtful comments on our manuscript. Our responses are as follows:

1. & 2. Clearly the referencing of how we calculate CDNC was not good enough (a problem when trying to be concise), for which we apologise. CDNC is calculated in a manner similar to Jones et al. (2001) but since updated to take account of improvements in the aerosol scheme used in our model (new species, etc.). CDNC is calculated from the accumulation mode concentrations of sulphate, sea-salt and organic

carbon (from biogenic and combustion sources), with sulphate and the carbonaceous aerosols modelled prognostically and the sea-salt diagnostically. The aerosols are treated as an external mixture. DMS is oxidised to SO_2 and SO_3; the former may be transported some distance before it is oxidised to sulphate, while the latter forms sulphate aerosol immediately. As the aerosol scheme is single-moment, any increase in sulphate mass automatically implies an increase in sulphate particle number, so gas-to-aerosol nucleation from DMS/SO_2 is implicit in this approach. We concede that it is not ideal that we have a mixture of prognostic and diagnostic aerosols forming the background against which we investigate the impact of geoengineered aerosols, but this is the model we have, the one which has performed all our CMIP5 simulations, and the one we have to use. To address these issues in the manuscript we have included new text in the Model Description section to describe the calculation of CDNC in more detail (lines 61-71).

The suggestion to include a comparison of modelled CDNC with satellite retrievals is a good one and we have now introduced a new Section 4 for this comparison (lines 115-137 and a new figure 2). We have compared CDNC distributions (non-geoengineered) from the model with those from MODIS and from CERES/MODIS. An exact comparison was not possible as the satellite retrievals are for CDNC at liquid-cloud top, a diagnostic our model does not provide, so we compare against mean CDNC from $\sim$500-1500 m. There are differences between the two retrievals as well as between them and the model, with probably the biggest difference between the model and the retrievals being over ocean polewards of about 30N/S. However, as the most important areas for geoengineering are generally equatorwards of these latitudes (e.g. where most of the RFP is, as shown in Fig. 3), we do not believe this to be a serious problem. For ocean areas between 30N/S the mean model CDNC compares well with the retrievals (62 cm-3 compared with 59 cm-3 from MODIS and 60 cm-3 from CERES/MODIS). We agree that the background CDNC and the change relative to this are of great importance for indirect forcing, but we believe that, despite the simple parameterization approach, the CDNC values in the model are generally within observational uncertainties.

3. We have added a paragraph towards the end of the discussion to highlight the uncertainties surrounding indirect aerosol effects (lines 342-348). Anonymous Referee #2 also suggested that we tone down the conclusion regarding direct vs. indirect effects, which we have done (lines 351-352 and 353-356).

4. We have added text to indicate the larger RFP in G4 (lines 197-198).

5. We have changed the colours used for this figure as also requested by Anonymous Referee #2.

6. This issue was also raised by Anonymous Referee #2 (see response to their item 7) and we now discuss this in Section 7 (lines 295-300).

7. We have corrected the typographical errors noted (and hopefully not introduced any new ones!).

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 20717, 2012.