

Interactive  
Comment

## ***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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The manuscript presents an ESM study of sea spray geoengineering with two scenarios: either maximizing the direct or the indirect effect. The major advance in this study is that it extends the Partanen et al. (2012) paper beyond RFP calculations and looks at actual climate effects finding clear differences in temperature and precipitation changes per unit RFP depending on the geoengineering strategy. It thus presents interesting new results well in the scope of ACP. The study is also well formulated and the results are presented clearly.

However, I have some criticism on the chosen model approach and also agree with the

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other referee that the authors may be overconfident in some of their conclusions. These two issues can, however, be overcome relatively easily, so once they and the minor comments listed below have been addressed, the manuscript should be accepted for publication in ACP.

1. The way the marine background aerosol is treated in the model should be described explicitly in the manuscript as it directly affects the interpretation of the results. I had to spend quite some time going through the earlier referenced work, which I doubt many readers will bother doing, and am still not 100% sure I understand how the marine aerosol and its interaction with clouds are handled.

Based on Bellouin et al. (2011) and Jones et al. (2001) the model scheme of the background marine aerosol is very simplistic: a diagnostic wind-speed-dependent sea salt number concentration which is reduced exponentially as a function of height, and DMS oxidation products affecting only the growth of particles and not making new CCN via gas-to-aerosol nucleation (+ potentially transported continental aerosol).

If this is how the background aerosol is dealt with, I find the chosen approach mixing a diagnostic background and a prognostic geoengineering aerosol a bit odd. In reality, natural emissions and geoengineering injections from a certain location would experience fairly similar transport and removal. It is therefore difficult to argue that including a prognostic treatment only for geoengineering aerosol would give very reliable indirect forcing/RFP predictions, which depend on the \*relative\* change in cloud drop concentration and are thus strongly influenced by the background CCN concentration.

Furthermore, gas-to-aerosol nucleation has recently been suggested as a major source of marine boundary layer (MBL) CCN. For example, Merikanto et al. (2009) found that according to their global model 55% of MBL CCN originate from nucleation. It is therefore likely that the ESM used in the current study is missing an important source for background cloud droplets.

Despite this, I think that the results of this study are still valuable and merit publication.

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However, the authors should explicitly acknowledge these limitations in the manuscript and explain how they may affect the results and conclusions. I also suggest a comparison of the modeled background CDNC fields to satellite-derived ones (for example, the the Quaas et al. retrieval should be easily accessible). A good match to satellite data would give justification to using a diagnostic background aerosol and also more confidence in the predicted effects of sea spray injections.

2. Is the cloud drop nucleation scheme used the one given in Jones et al. (2001)? I.e. does it assume only external mixtures, only sulphate and sea spray acting as CCN, and all Aitken mode particles acting as potential cloud droplets? If so, how would you expect this formulation to affect your results given that it is not a very realistic picture of what happens in the atmosphere?

3. Given the large uncertainties in aerosol-cloud interactions in large scale models (e.g. the measurement-based estimates of indirect forcing tend to be smaller than model-based ones) as well as typically poor model skill in predicting precipitation changes, I would tone down the conclusion on targeting the indirect effect being a better approach than targeting the direct effect.

4. First paragraph of subsection 5.1: You should mention already here that the RFP from the stratospheric simulation is larger and that the simulations are therefore not directly comparable.

5. The shading in figure 3 is impossible to see in a print-out.

6. Discussion: Via which mechanism do the D-mask injections cause a positive cloud forcing?

7. Typos:

- p. 20727, l. 11: 'an increases'
- p. 20728, l. 11: 'bewteen'

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- p. 20728, l. 24: 'the the'
- Twice in the reference list: 'Romakkaniemi, R.' should be 'Romakkaniemi, S.'

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

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