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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Sensitivity to deliberate sea salt seeding of marine clouds – observations and model simulations" *by* K. Alterskjær et al.

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This manuscript presents a simple susceptibility function that can be used to identify regions in which marine clouds may be susceptible to artificial brightening via sea spray geoengineering. A global model simulation with an explicit description of sea spray injections and aging shows that this susceptibility function is able to capture the main geographical features of the radiative forcing from modeled sea spray geoengineering. As such, the manuscript addresses a timely scientific topic well in the scope of ACP. The methodology used is appropriate. The results are clearly presented and support the drawn conclusions.

Therefore, I recommend the manuscript to be accepted after the following comments and questions have been addressed.

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1. While I agree that there are huge uncertainties in the potential magnitude and geographical extent of artificial sea spray injections, the abstract in its current form reads quite provocative (geoengineering can more than cancel warming from doubled CO2). To put the results in the right context from the start, the abstract should explicitly state that the simulated geoengineering emissions are higher than proposed in previous studies and that they are applied globally over oceans.

2. p. 29529, line 6: The original reference is Latham (1990)

3. p. 29529, line 20-21: Jones et al. (2009) conclude that their set-up of sea spray geoengineering could counteract 35% of the current greenhouse gas radiative forcing; not that cloud seeding could counteract the warming of the global climate.

4. The text should briefly discuss the uncertainties related to the satellite retrievals of COD, liquid cloud fraction and especially CDNC. These are relevant for the reliability of this study.

5. Recent (yet unpublished) studies have highlighted the role of simulated updrafts in the efficiency of cloud brightening. Thus it would be useful for future comparisons with other studies to indicate the simulated updraft range in the text.

6. Describe briefly how the aerosol indirect effect in the model compares within the AeroCom project.

7. Clarify in the text that since an offline model is used, the second indirect effect is not simulated in this study.

8. The text says that "CMIP 5 aerosol fields" are used. Does this mean CMIP aerosol emissions or climatologies? I would assume the former since you discuss nucleation and the loss processes of particles, but please specify.

9. I think the result that the cloud seeding efficiency seems to be determined more by the cloud fraction than the susceptibility is an important one and deserves a mention in the abstract.

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10. E.g. unpublished ECHAM-HAM simulations indicate that the cloud seeding efficiency in different regions depends quite strongly on the height of the cloud base. Figure 9 indicates that the effect of the sea spray injections is also in your simulations mostly limited to the lowest 1 km or so. Do you therefore think that the susceptibility function could be made even more accurate by including a measure of the cloud base height? Could neglecting this parameter be a reason for the regional differences with the Sortino susceptibility (they included the base height with quite a high weighting factor)?

11. p 29537, line 4-7: The latter part of the sentence is slightly difficult to understand at first reading. Please reformulate.

12. What is the physical reason for correlation between CDNC and cloud fraction indicated at the end of section 3.2?

13. Section 4, second paragraph: Is the modal diameter a number mean or a mass mean diameter? Indicate the standard deviation of the mode of sea spray injections (presumably 1.59). It is slightly misleading to state the size was suggested by Latham (2002) since he did not suggest a log-normal distribution with this width. When you write later that you emit 70 times more sea salt than suggested by Latham (2008), do you mean by mass? Since you use a lognormal mode and the Latham calculations were for a monodisperse aerosol, and it is mainly the total number (not the total mass) that determines the CDNC enhancement, this again could be slightly misleading.

14. Section 4.1: I agree that the cloud-weighted susceptibilities correlate well with sea salt emission impacts. However, I would argue that the correlation is not *very* good based on the inconsistencies in the relative strengths in several regions. (You could of course calculate a correlation coefficient between data in Figs 2d and 4 to show if I'm wrong).

15. p. 29541, second & third paragraph: On line 16 you write "pre-existing aerosol concentration"; specify that you show sulphate mass concentration (not aerosol num-

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ber which would be more relevant for CCN). Have you quantified the respective effects of reduction of nucleation and the lifetime of SO4 on larger particles? I'm asking because in our earlier study (Korhonen et al., 2010), the effect of sea spray injections on nucleation was negligible, although both SO2 and H2SO4 were greatly reduced in the boundary layer. Furthermore, the supersaturation reduction has been previously found in several field studies not directly related to geoengineering (add references to the text), as well as our geoengineering study.

16. p. 29542, first & second full paragraph: In Korhonen et al. (2010) we found a reduction in CDNC with the smaller injection flux (GEO) but not with five times higher flux (since the large number of injected particles compensated for the pre-existing particles that did not activate due to supersaturation suppression). Your study, however, shows a reduction in CDNC with very high aerosol number. Can you speculate the reason for this difference between the two studies (monodisperse versus log-normal injections?)? Furthermore, is the reason why "adding particles that are too small to become activated may lead to a decrease in the reflection of solar radiation" solely that they act as a water sink as they swell?

17. What is the reason for the large changes in LWP around the equator?

18. Figure 8 is not discussed in the text (merely mentioned) and can thus be omitted.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 29527, 2011.