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Interactive comment on "A multi-model assessment of the efficacy of sea spray geoengineering" *by* K. J. Pringle et al.

Anonymous Referee #2

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Review of "A multi-model assessment of the efficacy of sea spray geoengineering" by K. J. Pringle, K. S. Carslaw, T. Fan, G.W. Mann, A. Hill, P. Stier, K. Zhang, and H. Tost.

This study explores the sensitivity of the activated cloud droplet concentration to different concentrations of artificially-produced sea-salt particles under different assumptions about the updraft strength and distribution and for different background aerosol size distributions produced by three global models. As far as this reviewer can tell, the global models are used only for producing the background aerosol fields (which are not interactive in the sense that activation outcomes do not feed back onto aerosol fields) and only their activation schemes are tested here. Impacts upon cloud optical properties and radiation are not examined, nor is the model used to predict the updraft speeds used in the activation. It seems more a test of the model activation parameter-

ization under a range of background aerosol scenarios than a true test of the efficacy of geoengineering. I think the reader needs to be made aware of what the manuscript is attempting to do from the start, because the current title does not indicate that it is only the efficacy of the activation process that is tested here. There is no attempt to examine the feedbacks of changes in cloud droplet concentration on cloud condensate or cover. It is fine to do this, but a test of the efficacy of this type of geoengineering would need to include much more. As process studies show, such feedbacks are likely to be critical in determining the overall efficacy of sea spray geoengineering.

That said, I think the manuscript does contain some interesting findings and I think will be acceptable for publication subject to some revision.

Major points:

1. The authors need to rename their title to make it clear what has been done and to avoid misleading those thinking that the study provides global model estimates of geoengineering efficacy (i.e. cloud albedo increases driven by deliberate salt particle emissions). I might suggest something like:

"An assessment of the efficacy of increasing cloud droplet concentration by sea spray geoengineering for different updraft speed assumptions and for background aerosol states derived from three global models"

2. This study replicates the essence of previous parcel model studies (it should be noted that the models used here are essentially using activation schemes derived from the parcel-type approach). The most important finding in this study that differentiates it from previous studies is that the cloud droplet concentration enhancement with sea-spray geoengineering is found not to be very sensitive to the assumed width of the injected sea-spray aerosols.

3. The main substantive criticism I have of the paper is that the assumed updraft speeds are somewhat low compared with most real stratocumulus clouds over the

oceans. In citing only recent papers ignores the rich history of documenting and understanding the turbulent structure of Sc determined from observations from the 1980s onward. These include Nicholls (1984), Nicholls and Leighton (1986), Nicholls (1989), all QJRMS, Hignett (1991, JAS), deRoode and Duynkerke 1995 (QJRMS), 1997 (JAS), Wood (2005, JAS) etc. and a multitude of modeling papers from the 1990s. The review paper of Wood (2012, MWR) discusses this history in detail. Typical updraft speeds in most marine low clouds are larger than those in Lu et al. (2007) that are used to justify the authors' choice. The boundary layers in Lu et al. are very near the Californian coast and are atypically shallow compared with boundary layer depths over the broader subtropical oceans where the maxima in marine stratocumulus occur. The observed convective velocity scale in the ASTEX Lagrangian case are consistently above 0.6 m/s throughout (deRoode and Duynkerke, 1997, JAS, Table 2). See also Bretherton et. al. (2010, ACP) for some measurements of the vertical velocity pdf in deeper MBLs more typical of the open ocean stratocumulus regions. These results showed vertical wind standard deviations in the range 0.3-0.8 m s-1.

Thus, I am concerned that the conclusion about not being able to achieve 375 cm-3 everywhere might be problematic, since with 0.2 m/s it can be achieved everywhere. I would like to see some more discussion of this. It would be much more robust a statement if the model actually predicted the updraft speed and its geographical variability.

4. The authors need to state what is new here that isn't in existing papers (e.g. those focusing upon geoengineering itself: e.g., Bower et al. 2006). Also, there are a number of papers that have explored the competition effects of introducing coarse mode aerosols (papers by Ghan and others come to mind) that surely have provided insights for this study.

5. Why is the geometric standard deviation of injected particles assumed to be 1.1? Is this just a guess, or is there some physical basis for this number? Are the results sensitive to this choice?

6. What exactly is a marine "background". Does such a thing exist? It suggests that there's some kind of steady state in the marine boundary layer, but all the studies and observations I have seen suggest quite the opposite: an aerosol that is highly variable in time and space and can sometimes be almost completely wiped out by precipitation.

7. Statements such as "geoengineering becomes more efficient" are misleading. All you know is that the increase of CDN becomes more efficient. This is only one part of the geoengineering problem.

8. The model is just used to give the fields of the unperturbed aerosol size distribution. Doesn't the model need a vertical velocity to produce such fields, since they depend on physical processes such as activation, precipitation scavenging, cloud processing etc.? What updraft is assumed for these and is it consistent with what is used in this study?

9. Does the use of monthly mean aerosols fields average out a lot of potentially important variability? This can be checked by comparing with the results performed on instantaneous model aerosol fields.

10. Are the authors comfortable with the fact that the ECHAM-HAM has zero accumulation mode aerosol over most of the global ocean and no apparent sign of continental sources showing up over the ocean?

11. It would be great if the authors showed the MODIS observations for comparison. Unless one is familiar with the Bennartz study, it is difficult to assess the relative model skill at getting the correct mean cloud droplet concentrations.

12. Besides needing increases in CDN, one also needs existing clouds with intermediate albedo (50% is ideal) to effectively brighten clouds. The authors might wish to comment on this. Do the models produce realistic cloud fields?

OTHER COMMENTS

1. At least one of the authors of the Korhonen et al. study is an author on this paper. Yet

the assessment of the causes of the differences between the two studies is in places vague and speculative (P7140, line 23-25). The utility of models is the ability to test such hypotheses.

2. All the results in Fig. 6 look almost identical for the four regions. Why then not just focus on one region and look in more depth?

3. P7144, Lines 1-4: I don't understand this statement. The updraft speed is not produced by the model, it is assumed. I agree that more attention needs to be paid to the frequency distribution of updrafts in the real world and in attempting to predict this in large scale numerical models. There are ways to do this.

4. P7145, L10: Do the authors mean the opposite? Why inversely proportional?

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