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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Enhancement of marine cloud albedo via controlled sea spray injections: a global model study of the influence of emission rates, microphysics and transport" *by* H. Korhonen et al.

Anonymous Referee #2

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This study uses a state of the art global-microphysical model to explore the expected changes in cloud drop number concentration from one particular set of assumptions for how the proposed particle distribution would be implemented. The authors have used a state-of-the-art microphysics model to employ a specific scenario of increased Mbl aerosol; their results do not reveal any underlying new behavior but they do have interesting new implications for the feasibility of geo engineering. What seems disappointing is (1) the authors' failure to use the model to explore the sensitivity of their results to various uncertainties, which would clearly do a far better job of increasing our

understanding of the system. For similar reasons it is disappointing that (2) the authors have failed to quantitatively compare the differences in their results to the published results of Rasch and Jones, so as to say definitively the degree to which their conclusion of the reduced effectiveness is largely predicated on their assumption of how much salt would be distributed where, based on their implementation of an undated "personal communication" of S. Salter. While these two deficiencies could be easily remedied by additional model runs and interpretation that would make this manuscript highly suitable for a very interesting paper in ACP, in its present form it seems to be a better fit for a more geoengineering-focused journal where the projected cooling effect is of greater interest than the scientific understanding of the atmosphere.

Problems to be addressed: (1) Sensitivity studies of both the particle distribution scheme and the other key assumptions should be carried out to explore the physical constraints on why the proposed cloud albedo modification is unlikely to work. Specifically, the authors should consider 1. Running different salt distribution scenarios, including those previously published by Latham et al. 2006 and 2002. It strikes me that this is likely to be particularly fruitful because the spray distribution scheme being considered, which is highest when/where natural spray is highest, is particularly ill-suited to target the most susceptible clouds. (See note below.) 2. Running different precip schemes (or at least one and one off) would also seem imperative to identify whether any of the schemes is effective at lofting and maintaining aerosol. 3. The suppression of supersaturation is noted, but I think this merits further discussion, as this is scientifically the most interesting part of the paper. How frequently does this occur? How important is it? Does it yield thermodynamically consistent clouds or is LWP increased? If so, how?

(2) Comparison to published literature should be quantified explicitly: 1. On the last point [(1)-3.], it seems this is not entirely new, and there is possibly observational evidence to use as constraints from the group of the handling editor (e.g. Russell et al., J. Geophys. Res., 1999) showed reduced Sc for measured conditions and fixed updrafts.

How are the reductions predicted in this model compared to what was found there? 2. There is a limited discussion of and comparison to Bower et al; it seems more is merited since opposing conclusions were drawn. Is the specific difference in the scaling to the global distribution, or is the CDNC prediction different? I feel very strongly that a quantitative comparison to the existing literature is essential to place this work in context. 3. For global modeling results, we need a more specific quantitative comparison to Rasch and Jones models reported in Latham et al. (PhilTransRoySoc, 2008); this work is cited but they should be added to graphs and quantitative regional differences should be explored and explained.

(3) The discussion of the choice of sea spray choice should be more comprehensive, and the degree to which this choice is arbitrary should be made more explicit. The "5xGEO" does not seem to represent the spirit of the Latham proposal, as the particle generation is not to be done by increasing wind speed. In fact, the Rasch et al. work shows that a better approach is to target the areas with low wind speed, providing a disproportionately larger effect in those areas. If the main difference of this model is that the authors use the Salter design

(4) "Personal communications" should be explicitly approved by the person to whom they are attributed, when they cannot be avoided altogether. Can the authors verify that S. Salter has approved this interpretation of information he provided?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 735, 2010.