Atmospheric
Chemistry
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Discussions

Interactive comment on "Response to marine cloud brightening in a multi-model ensemble" *by* Camilla W. Stjern et al.

Anonymous Referee #2

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The paper is a multi-model comparison to understand marine cloud brightening, including regional responses in cloud and precipitation fields, based on increasing the CDNC droplet number concentration over low clouds by 50% and the RCP4.5 emissions scenario. A radiative forcing (ERF) of -1.9 W m^{-2} is found in the ensemble, with the spread equal to -0.6 to -2.5 W m^{-2}.

Generally I thought the style of writing, and the quality of the figures is fine and would support publication in a modified form. The main issues I have with the paper in its current form are (1) it is unclear what has been done in parts of the manuscript; (2) as presented the results may be mis-interpreted unless more care / accuracy is taken in the wording.

The G4CDNC experiment is used. I am not familiar with this experiment myself, and I

had to do some searching in the text to find that it is a 50% increase in CDNC of low clouds. This should be brought to the fore, so that it is clear. It would be useful to know how much of an overestimate applying the CDNC increase to all low clouds over the ocean may be to those that could realistically be subjected to a geoengineering scheme. What area of the earth does this correspond to in each of the different models? This is an important parameter to know: generating the requisite spray for MCB is no trivial task in terms of energy and scale-of-operation requirements.

In the introduction there is reference to the finding of Alterskjaer et al., that seeding may lead to warming under certain conditions. Connolly et al. (2014, Phil Trans) discuss a similar finding in some detail. In short, the finding is that the Abdul Razzak et al parametrization does not work outside of the region they were originally tested and, in some ranges of parameter space, increasing the aerosol number concentration can lead to a spurious reduction in droplet number concentration. I note from table 1 that some of the models used in this study employ the same parameterisation, including NorESM. While CCN activation schemes are not a focus of this particular paper, some of the reasons for the effects talked about in the introduction may be due to CCN activation schemes, so it is an important point for other people working in this area.

I was not familiar with the Gregory regression method (section 3) until I did a search on the Internet. I think the original source should be cited. Would it be more complete to show / present the correlation coefficients associated with the analysis, to allow the reader to assess its suitability?

Fig 4a was not referred to directly in the text.

Although the models in the paper are important tools for climate assessment, the major issue with this kind of analysis is that the processes under investigation are not resolved at high enough grid resolution. There should be some caveat in the paper so that readers who are unfamiliar with the details are not mis-lead.

The statement: "Our results suggest that liquid cloud parameterizations ought to be of

appropriate complexity in order to attempt to model marine cloud brightening and the climate response." I did not see how this conclusion was arrived at. How do you know that liquid cloud parameterisations are complex enough?

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