

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-327

Anonymous Referee #1

Referee comment on "Assessing the potential efficacy of marine cloud brightening for cooling Earth using a simple heuristic model" by Robert Wood, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-327-RC1>, 2021

Review of Assessing the potential efficacy of marine cloud brightening for cooling Earth using a simple heuristic model by Robert Wood.

General Comments

This is a good paper on a very current topic, climate engineering. It presents a model to evaluate the multiple different ways in which marine cloud brightening could theoretically be deployed and also suggests ways for improving the simulation of cloud brightening in models. It is well presented and clearly written and I recommend publication once the following comments are addressed.

The paper is generally clear about which physical processes it includes (*e.g.* a detailed treatment of aerosol activation) and which it does not (*e.g.* adjustments of cloud LWP or amount), but one process which is not mentioned and could be of importance is the water injected by the sprayers. The proposals of Latham, Salter &c. are for the sprayers to inject a spray of sea-water, not of dry aerosol. Most of the water is assumed to evaporate as the spray is mixed through the depth of the MBL into the cloud above, which with a large number of sprayers has the potential to both moisten and cool the MBL with subsequent impacts on the cloud layer. A discussion of this process should be included and the impact of omitting it assessed.

Specific Comments

Lines 65-69 and 75-76. The forcing of -0.06 to -0.6 Wm^{-2} due to the current commercial fleet of around 60,000 vessels stands in sharp contrast to the forcing of around -4 Wm^{-2} due to only $\sim 10,000$ MCB sprayers (these latter results are introduced later in the manuscript). It would be nice if, somewhere later in the paper, this apparent contradiction could be discussed (presumably the issue is particle size).

Lines 110 vs. 118. The former says Rasch *et al.* used an N_d value of 1000 cm^{-3} but the latter says they increased N_d to 375 cm^{-3} .

Lines 181-182. It should be made clear that f_{spray} is an input parameter to the model, not something which has been determined from observations. It should also be emphasised that f_{spray} and f_{low} are single (global) parameters, because their description follows immediately after a discussion which talks about 10x10 degree gridboxes and it's easy for the reader to continue with that idea and imagine a geographic distribution of values of f_{spray} and f_{low} with different values in each gridbox.

Line 191-192. This needs a little rewriting to avoid the impression that the second sentence (beginning "Cloud condensate...") is still referring to the areas without low cloud which are referred to in the first sentence.

Lines 205-206. Does the phrase "Assuming the entire ocean area could be seeded" imply an f_{spray} value >1 ? Otherwise I can't see how an r_N value of 2.4 can give a forcing of -3.7 Wm^{-2} from Fig.1. Please make this clear if it is indeed the case (or if not, explain where the r_N value of 2.4 comes from).

Line 218. How sensitive are the results to the assumption of stationary sprayers? Most promotional material I've seen for such sprayers depicts them as decidedly non-stationary ships.

Lines 264-265 and Fig.2(c). Is the value of 0.41 TJ for the "Spreading" line the amount for the 0-3 day period shown in the figure or the amount estimated for the whole duration of the perturbation (perhaps as long as a week, given the form of the curve shown in the figure)?

Line 315 (and elsewhere). "PBL" is introduced (without definition) in the context "marine PBL" where it seems "MBL" (which has been defined) would do just fine. "PBL" also turns up in lines 348, 364, 438 and both tables. Either use "MBL" throughout or define PBL.

Line 321. As the source of the injected aerosols is presumably the local seawater it is unlikely to be pure sodium chloride.

Line 336 and Eq.(11). The greek letter tau has already been used (with the suffix "res") to denote a timescale, whereas here (with a different suffix) it's used for AOD. I suggest using different symbols for two such very different quantities.

Lines 366-367. The phrase "This is handled in the heuristic model as described in Sect. 2.5" really isn't sufficient: Section 2.5 has a lot in it so the process used needs to be spelled out in a bit more detail.

Lines 473-4. The reason for the "considerably shorter" residence time of natural sea spray particles should be given (larger particle sizes?).

Lines 596-7. For clarity, this sentence needs to end something like this: "...required to produce a significant radiative forcing via cloud modification rather than direct aerosol forcing." The point being that "significant radiative forcing" can in principle be produced via either mechanism depending on the size (D_s) of the particles injected.

Line 661. "suggests" would be more appropriate than "demonstrates" - all Fig.12 actually demonstrates is that the minimum value of f_{spray} required to achieve a $-dF$ of 4 Wm^{-2} is somewhere between 0.25 and 0.5.

Line 689. "insufficiently short" means "too long" which is precisely the opposite of what is intended.

Line 713: It would aid clarity if this was written out in full: "...tend to produce more brightening for clean cases and less for polluted cases".

Technical Corrections

Line 53: "handled" not "handed".

Line 166: " ϕ_{atm} " not " f_{atm} ".

Line 264: Remove the underlining of "8" in "(Eq.8)".

Line 436: An extra ")" is required after "(5)" to close the parenthesis opened on the previous line.

Line 498: "...of very adding small..." should be "...of adding very small...".

Line 705: "presents" not "presented".

Line 739: Shouldn't this line have a bullet point?

Figure 5(a): Why include the blue (negative) segment on the color-bar? It's not needed.