

The authors thank the referee for the useful comments, which are all addressed below. Our comments are below along with the referee comments which are given in italics.

The reviewer's main concern is that we rely on the assumption that an increase in CDN will lead to an increase in cloud albedo. We take the reviewer's point that this is a simplification and we have reworded several sections of the text to clarify that we only predict the change in CDN and thus must be cautious about drawing conclusions about cloud albedo.

We have changed the title to: *A multi-model assessment of the impact of sea spray geoengineering on cloud droplet number*

We have re-worded claims about the efficiency of geo-engineering simply in terms of the change in CDN.

We have added the following section to the introduction: "The cloud response to a change in aerosol loading is highly complex and depends on the meteorological conditions, for example \cite{wang11} used a cloud resolving model to investigate the response of the cloud to sea-spray geoengineering and found geoengineering to be inefficient when the cloud is strongly precipitating or heavily polluted but efficient under clean and non/weakly precipitating conditions. Other studies have examined the response of clouds to the injection of aerosol from ship plumes, these question the extent to which an increase in aerosol will result in an increase in cloud albedo as in addition the cloud liquid water, cloud depth and cloud lifetime may also be affected. For example, \cite{segrin07} found that the cloud liquid water path decreased in 60% of clouds in response to an increase in aerosol loading. \cite{coakley02} found that in ship tracks liquid water was typically reduced by 15 -- 20%. \cite{christensen11} found that the response to a ship plume was different in closed and open cells within the stratocumulus cloud deck, with open cell clouds increasing in height in response to the increased aerosol. The albedo response to geoengineering is also affected by cloud lifetime, which may increase as a result of the reduced droplet size which reduces the drizzle rate \cite[e.g.]{}{christensen09}."

In Methods we have added the caveat: \textbf{This offline approach does not treat the entrainment of air at cloud top. By using an activation parameterisation we cannot capture any changes to the macrophysical properties of the cloud that occur as a result of the injected aerosol, thus we cannot calculate the resulting change in cloud albedo or forcing as a result of the geoengineering.}

In the conclusion we have added: This study only examines the response of the number of activated aerosol to the injected particles, in order to assess the climate impact one needs to also consider the change in albedo that arises from the geoengineering. This is dependent on the cloud liquid water response, the initial cloud albedo and the simulated cloud cover. Further work examining the model dependence of these properties is also required for a robust assessment of the efficiency of sea spray geoengineering.

The spatial and temporal resolution should be specified for each model.

Done

Further information regarding the limitation of using these models using an offline approach should be provided? For example, one of the limitations is discussed on line 6 of page 7138 where it is stated that collision/coalescence is not treated. Does this also extend to cloud top entrainment as well?

The offline approach cannot treat entrainment; we have added this to the text. We have also re-worded claims about changes in forcing to refer only to changes in cloud droplet number.

We agree that these physical processes are important determining the cloud lifetime and will affect the response of the cloud to geo-engineering but the treatment of collision / coalescence and entrainment in global models remains uncertain and highly simplified parameterisations are often applied. We believe the examination of the

response of the macrophysical cloud properties is better done using cloud resolving models, but this would not allow the global overview that we provide in this paper

These processes are key to the maintenance of boundary layer clouds and how the albedo responds to elevated concentrations of cloud water if you would see the same response in Figure 8 if drizzle were included. A stronger updraft will promote higher supersaturations and hence, CDN, but rainfall acts to oppose that response through depleting CCN and CDN. There is seemingly no bound on the CDN increase through injection when the updraft speed is greater than 0.4 m/s and, at these updrafts speeds rainfall is likely to occur. This limitation casts some doubt on the accuracy of these results as they likely serve as an upper bound on the CDN concentration response to geoengineering. Due to these limitations, the responses reported in this study likely represent ?instantaneous? microphysical changes of the clouds and miss important cloud feedbacks that can have negative adjustments to the CDN and cloud albedo responses on timescales of hours to days [Wood, 2007]. It is thus, recommended to discuss the breadth of the aerosol indirect effect responses and how effective this approach/model is at capturing them.

We already state that due to the neglect of collision / coalescence the response is an upper limit, but we have added the referee's suggestion that this is most important at higher updrafts where rain is more likely to occur.

How is the updraft speed calculated? Is it an average over all cloudy grids at the cloud base height that is taken to be 940 hpa? Also, is it the same at every grid location in the model (i.e., the mean of $w = 0$ with a standard deviation of 0.25 m/s)? Is this an appropriate assumption given the average range of updraft speeds across the planet? Please clarify.

We already state that: "In these simulation a Gaussian PDF of updrafts is assumed to occur **in every marine model gridbox** (sigma= 0.25, mean = 0.0): CDN is calculated for multiple (10) updrafts within this PDF then a mean CDN is calculated from a probability weighted mean of these values"

We have added the words in bold to clarify. We use this approach as the calculation of updraft velocity in a global model is very uncertain and by using this approach we know that the same updraft is used in every model and in every model gridbox. As we only calculate CDN in marine regions there is no need to differentiate between marine and continental regions.

Minor Corrections:

2) The notation of units is non-standard. A space should be included between units. For instance, "m s⁻¹" should be used, not "ms⁻¹"

Corrected

3) Page 7126 line 1, we are talking about low-level clouds so I would add the word boundary layer after marine.

Done

4) Pg. 7126 lines 4-6, this sentence in the abstract is a little vague. Can the authors clarify what the previous modeling study was (e.g., a reference), what a modest increase in CDN is (e.g., a % increase), and what a plausible emission scenario looks like (e.g., an injection rate)? I believe Salter et al., 2008, lays this out nicely.

Changed to:

A previous global model study \citep{korhonen10} found that only modest increases ($< 20\%$) and sometimes even decreases in cloud drop number (CDN) concentrations would result from emission scenarios calculated using a windspeed dependent geoengineering flux parameterisation.

5) pg. 7126 line 12, "0.1 ms⁻¹"? (Presumably the authors meant meters per second rather than inverse milliseconds.)

Changed

6) pg 7126 line 23, I'm not sure what a cloudy grid box means in this context, can it be changed to something like, 50% of the region covered by clouds??

Changed to: in only about 50% of grid boxes which have > 50% cloud cover

7) Pg. 7126, lines 22 to 25 need to clarify and explain how a stronger updraft allows for a higher CDN to be achieved. You might recast it to say something like this "However, at stronger updraft speeds, higher values of CDN are achievable due to the elevated in-cloud supersaturation. Achieving a value of 375 cm⁻³ in regions dominated by stratocumulus clouds with relatively weak updrafts cannot be attained regardless of the number of injected particles, thereby limiting the efficacy of sea spray geoengineering. "

Changed to: But at stronger updraft speeds (0.2 ms⁻¹), higher values of CDN are achievable due to the elevated in-cloud supersaturations. A value of 375 cm⁻³ in regions dominated by stratocumulus clouds with relatively weak updrafts cannot be attained regardless of the number of injected particles, thereby limiting the efficacy of sea spray geoengineering.

8) Pg. 7127, line 10, an increase in the planetary/cloud albedo would only arise if the macrophysical properties of clouds remained constant as CDN increased.

Changed to: These particles would then increase the number concentration of cloud droplets in marine stratus clouds, and thus, if a constant cloud liquid water path is assumed, increase the planetary albedo.

9) Pg. 7127, line 21, remove "of" and replace it with "that can range from"

Changed to: from

10) Pg. 7127, line 24. The word online/offline is used numerous times throughout this manuscript. For the reader who is not familiar with a modeling study and their association to that word is through the world wide web, it might be useful to explain what is meant by a calculation being performed offline.

Added clarification to the methods section: offline (i.e. as a post-processing step rather than during the model simulation)

11) Pg. 7127, lines 25 - 28, Please clarify the impacts (climate I presume?) and emission rates (which ones? sea spray geoengineering? Or through natural wind-driven processes) that are being discussed.

Changed to: The Korhonen¹⁰ study was the first to consider geoengineering from an online windspeed dependent emission rate through to the change in CDN

12) Pg. 7128, line 1. Clarify what is competing for the moisture, is it the cloud drops (active), haze drops (inactive), or aerosols?

Added term: activated aerosol

13) Pg. 7128, line 3. At the end of the sentence I might add, to have a cooling influence on climate.

We don't think this is required.

14) Pg. 7128, lines 4 - 7. Numerous commas are needed and the verb tense needs to be consistent throughout this sentence. Also, what is a host model?

Changed and removed the word "host".

15) Pg. 7128, line 10. Please explain what is meant by the word scenarios? Does this refer to a geoengineering scenario, or something about the cloud parameterization scenarios that limited their ability to understand the response?

Changed to:

While the \cite{korhonen10} and \cite{partanen12} studies are useful assessments of the efficiency of sea spray geoengineering they are based on a particular scenario, for example a fixed injection size and updraught are assumed and although a range of injection numbers is considered the dependence of the increase in CDN on the number of particles injected is not explicitly examined.

16) Pg. 7128, line 19. Missing a word between CDN and be.

Added: would not

17) Pg. 7129, line 1. Add "into cloud droplets" after the word activate and before depends. On point (ii), it is primarily the concentration of the background aerosol serving as cloud condensation nuclei that control the CDN concentration, not simply, the background aerosol.

Added: into cloud droplets

Added: able to serve as cloud condensation nuclei

18) Pg. 7129, line 10. This finding is inconsistent with that of Hobbs et al., [2000] where the concentration of CDN was found to principally depend on the size of the emitted particles. When oceangoing vessels burned diesel fuel or low-grade marine oil, the bulk of the produced particles in the exhaust were in the accumulation mode (0.03 to 0.05 μm) and, ship tracks were generally produced using this fuel type. Particles produced from gas and steam-turbine engines were commonly too small to serve as CCN and thus, did not increase CDN or produce ship tracks.

Added: This is in contrast to the work of \cite{hobbs00} who used in situ measurements to analyse the cloud response to ship emissions and found that the response of the cloud was affected by the size of the particles on emission.

19) Pg. 7130, line 7. The authors state that this assessment provides an upper bound on the CDN increase and potential radiative cooling. This is probably a correct assumption because the model/approach does not account for negative adjustments to the CDN through cloud macrophysical changes (changes in liquid water amount, cloud depth, drizzle, ect..). Therefore, I would simply state (for macrophysically identical clouds) after the words in CDN and before the word which.

Done

19) Pg. 7131, line 25. Please explain what the optimum conditions are. I assume the authors mean, the optimum conditions for enhancing CDN concentrations?

Added: for sea spray geo--engineering

20) Pg. 7132, line 6. Do the authors mean, an annual mean increase in CDN concentration?

Added: in aerosol number

21) Pg. 7133, line 3. *This sentence might sound better if reworded: ? . . . we found that at the updraught speeds considered, the number. . . ?*

Unchanged, except added a comma

22) Pg. 7133, line 10. *Replace the word on with of?*

Unchanged but added that the increase is 3 cm^{-3}

23) Pg. 7134, line 4. *Is VOCALS an acronym? If so, spell it out the first time it is used, which I believe is here. Also, there are an assortment of other acronyms (mostly model names) that should be spelled out throughout the manuscript as well.*

Changed

24) Pg. 7134, line 17. *Change an to a and, add units to the standard deviation, I'm assuming it is in micro meters. On that note, the units are missing in a variety of locations throughout the manuscript.*

Standard deviation is unitless, missing units have been added elsewhere in the text.

We are not clear why this information will help the clarity of the paper, so we have not made any changes.

25) Pg. 7134, line 17. *The change in cloud albedo is proportional to the percentage change in CDN under constant changes in liquid water content.*

Changed to: the change in cloud albedo is proportional to the percentage (not absolute) change in CDN, when cloud liquid water is constant

26) Pg. 7135, line 24. *Can the word conditions be explained in the following phrase? but the exact value depends on the conditions?? Do the conditions refer to the updraft speed or to the background aerosol concentration? Please clarify.*

Changed to: depends on the both the pre-existing aerosol loading and the updraught

27) Pg. 7138, line 25. *Higher concentrations of CDN were probably observed in closer proximity to the coast due to the industrial complex of copper smelters along the Chilean coast whose combined sulfur emissions total 1.5 TgS yr^{-1} . This is comparable to the entire sulfur emissions from large industrialized nations such as Mexico and Germany.*

Changed to: measured CDN concentrations of $> 200 \text{ cm}^{-3}$ close to the western coast of South America (probably due to the high sulfur emissions from Copper smelting in this area)

28) *In Figure 6, every region exhibits the same behavior, thus I think the paper could be cleaned up a bit by only including one of them. Is it possible to also include the average CDN concentration under the no injection ($d000$) scenario for comparison?*

We agree with the reviewer and we have changed the figure to show just one region and combined Figures 6 and 7 (see below).

29) Pg. 7141, line 10, It is stated that increasing the injection diameter above 160 nm reduces CDN. The Figure indicates otherwise. I would replace the word reduce with has a smaller influence on enhancing the concentration of CDN?.

Changed to: leads to a smaller increase in CDN

30) Again, I would reduce Figure 7 to include the results from one region. Better yet, Figs 6 and 7 could be combined using the results from only one region and the similar results from the remaining regions could be discussed in the text? Removing these Figures would increase readability and enable easier navigation of the paper. This is a recommendation and, the decision to remove the content is entirely up to the authors.

Done.

31) Pg. 7142, line 7. I'm not sure which system is being referred to at the start of this line, presumably the authors meant the climate system?

Changed to: pre-existing marine aerosol

32) Pg7144 ? 7145. The authors discuss the role of the dilution of ship plumes in stratocumulus. Hudson et al., (2000) and Ferek et al., (1998) provide evidence of this effect i.e., whereby higher concentrations of CDN (and lower supersaturations) are generally observed closer to the ships and tend to decrease down the lengths of ship tracks. To my knowledge, there is currently no evidence for a reduction in the CDN concentration by the aerosol plumes from ships.

We already state after this finding that: High resolution modelling or field experiments would be required to assess the magnitude of this finding in more detail.