

REVIEW REPLY: "Sensitivity to deliberate sea salt seeding of marine clouds – observations and model simulations"

Reply to H. Korhonen

The authors are thankful for a thorough review which has led to manuscript improvements both concerning scientific discussion and readability. We have carefully considered your comments as noted below.

1) The wording in the abstract was changed to make it clear that the emission strength and the area seeded were much greater than proposed in previous studies. **"We then carry out geo-engineering experiments with a uniform increase over ocean of $10^{-9} \text{ kg m}^{-2} \text{ s}^{-1}$ in emissions of sea salt particles with a dry modal radius of $0.13 \mu\text{m}$, an emission strength and area much greater than proposed in earlier studies."**

2) Reference included.

3) We appreciate your comment and rewrote the paragraph to correct for this: **"Similarly, Jones et al. (2009) conducted a study using different versions of the HadGEM2 model in which the CDNC was increased to 375 cm^{-3} in three regions of persistent marine stratocumulus. These studies found that cloud seeding could counteract or limit the warming of the global climate, (...)"**

4) The satellite products are now discussed in more detail in section 2.1:

"The satellite products include daily observations of the liquid cloud fraction, the cloud optical depth (COD) and the cloud droplet effective radius (CDR), and are from the Collection 5 processing stream. The collection number indicates what algorithms are used to process the satellite observations (<http://modis-atmos.gsfc.nasa.gov/>). The COD and CDR are retrieved using the absorption channel at $2.1 \mu\text{m}$ in combination with non-absorbing channels at 0.65 , 0.86 and $1.2 \mu\text{m}$ over land, ocean and snow surfaces respectively. The retrievals assume plane-parallel clouds and an overcast scene with cloud homogeneity within the 1-km observation pixel (Platnick et al., 2003; Bréon and Doutriaux-Boucher, 2005). Data on CDNC are taken from the Quaas et al. (2006) data set and are retrieved from the joint histogram of MODIS COD and MODIS CDR for liquid water clouds, and diagnosed assuming adiabatic clouds. The uncertainty in CDNC is largely tied to the uncertainty in retrievals of CDR (Quaas et al., 2006) and to the correctness of the assumption on adiabaticity. The product is more reliable for homogeneous, single-layer clouds than for more complex clouds, and the uncertainty is expected to be lower over ocean than over land surfaces."

5) Unfortunately, the in-cloud updraft velocity is not available output in our set of simulated data, but we agree that this quantity is important and will make it available in future studies. We have included a reference to a study that uses an earlier model version where the in-cloud updrafts over ocean lay mostly between 10 and 40 cm s^{-1} : **"The in-cloud updraft velocity averaged up to 2 kilometer height has been shown to be between 10 cm s^{-1} and 40 cm s^{-1} over ocean (Hoose et al., 2010, Fig. 6)."**

6) We agree that the performance of the NorESM model in the AeroCom project allows the reader to set our forcing estimates into context. The paragraph is rewritten:

“In the latest published quantification, the atmospheric component of NorESM (CAM-Oslo) has an indirect effect of -1.9 Wm^{-2} compared to an AeroCom mean of -1.6 Wm^{-2} (Quaas et al., 2009). Since then the model has been modified and the value is now around -1 Wm^{-2} (A. Kirkevåg, personal communication), as discussed in an upcoming paper.”

7) The second indirect effect is, in fact, partially simulated in the study. The text was rewritten in section 2.2 to make this clearer: **“This [offline simulations] also implies that aerosol effects on cloud cover and lifetime are not fully simulated in this study. The contribution to the second indirect effect associated with changes in cloud liquid water due to suppression of precipitation release is accounted for following Kristjánsson (2002).”**

8) The CMIP5 aerosol emissions are used. The text was changed to make this clear.

9) The second referee on this manuscript does not agree on the importance of the cloud fraction. We have rewritten the text in this paragraph to clarify that the cloud fraction dominates *where the solar zenith angle is small*, not everywhere:

“Comparing the cloud-weighted susceptibility to Figures 1(a) and 2(a) indicates that at low and mid latitudes the function (eq. 5) is dominated by the cloud fraction rather than by the susceptibility. One exception is the area of high cloud-weighted susceptibility over the Indian Ocean, which is influenced by a high susceptibility (Fig. 1(a)). Overall, the most susceptible areas (Fig. 1(a)), corresponding to regions of low CDNC, have small cloud fractions (Fig. 2(a)).”

We also included the following sentence at the end of the first paragraph in the abstract: **“At low and mid latitudes the signal is dominated by the cloud fraction.”**

10) You raise an interesting question. Clearly the cloud base height along with the vertical velocity is important in determining whether the added sea salt reaches the cloud base. However, this function is meant to be used as a first estimate and a tool for validating model performance of the cloud-aerosol interactions. We therefore wish to keep the function as simple as possible, allowing for it to be used on both satellite and simulated data. Neither cloud base nor vertical velocity is an available MODIS product. We have partly accounted for the cloud height by only considering warm clouds for satellite data and low level clouds for simulated data, knowing, however, that at low latitudes warm clouds can extend several kilometers above the surface. We believe it is correct to assume that the regional differences with the Sortino susceptibility are influenced by differences in the selection criteria used. In addition to using cloud base height, Sortino also gave weight to surface wind speed. Neither of these factors is included in our cloud-weighted susceptibility function.

11) We agree that the sentence was overly complicated and have split it up.

12) While the correlation between CDNC and cloud fraction is true for MODIS data, a closer look at the model data showed that this correlation is not found there. The relation found in the satellite data is now discussed in the follow manner:

“This relation could conceivably result from satellite retrievals of broken clouds with weak updrafts, low CDNC and small cloud fractions contrasting vigorous clouds with strong updrafts, high CDNC and large cloud fractions. Similarly, areas of low cloud fractions at the fringes of frontal

systems may have small updraft velocities and therefore low CDNC. The relation could also be spurious and based on a bias in satellite retrievals of low cloud fractions. If the cloud optical depth is underestimated, this will lead to an underestimation of the CDNC. An in depth discussion on this relation is beyond the scope of this paper.”

13) The modal radius is a number mean radius. We rewrote the paragraph to clarify this and to rephrase the comparison to the sea salt particle size suggested by Latham (2002): **“The experiments consisted of simulations in which the emissions of sea salt were increased uniformly over open ocean by $10^{-9} \text{ kg m}^{-2} \text{ s}^{-1}$. These sea salt emissions had a number mean dry modal radius of $0.13 \text{ }\mu\text{m}$ and a geometric standard deviation of 1.59, which corresponds to an effective radius of $0.18 \text{ }\mu\text{m}$. For comparison, Latham (2002) suggested using monodisperse emissions of sea salt particles with a radius of $0.13 \text{ }\mu\text{m}$. The total emission flux is equivalent to a global emission rate of about 350 tonnes of sea salt per second.”**

We also rewrote the paragraph where we compare our emission strength to that suggested in Latham et al. (2008): **“(…) even though we emit a sea salt mass about 70 times larger than what was suggested by Latham et al. (2008), (…)”**.

14) We agree that the correlation may not be very good and have removed the word “very” from the sentence.

15)

- i) We chose to replace this figure with two new ones; 6(b) now shows the percent change in SO_4 nucleation rate and 6(c) the percent change in SO_4 lifetime. Through showing that these properties change, we have also shown a change in the background aerosols. The sentence now reads:

“Our results show that increasing the number of sea salt particles in the atmosphere affects both the cloud supersaturation (Fig. 6(a)) and the background aerosols (Fig. 6(b) and Fig. 6(c)), (…)”

- ii) We have not quantified the respective effects of reduction of nucleation and the lifetime of SO_4 in this study, but this is something we plan to study in an upcoming paper. We have included two new plots showing percent change in SO_4 nucleation rate and percent change in sulfate lifetime. With sea salt emissions of this magnitude the nucleation of SO_4 is almost shut off in our simulations.

“The added sea salt particles greatly increase the total surface area of atmospheric aerosols, allowing more condensation to occur, reducing both the nucleation of new SO_4 particles (Fig. 6(b)) and the lifetime of SO_4 (Fig. 6(c)) as more is washed out with the sea salt. Please note that the values on the colorbar in Fig. 6(c) are not symmetric around zero. The respective effects on background aerosols have yet to be quantified, but Fig. 6(b) shows that sea salt injections of this magnitude lead to an almost complete shutoff of SO_4 nucleation in our simulations.”

- iii) Thank you for pointing out that there were references missing in the paragraph. We have now included references to the work of Ghan et al. (1998), Bower et al. (2006) and Korhonen et al. (2010).

16)

- i) The difference between the studies is what regime we are in. When you added a small number of aerosols and got a reduction in CDNC this was caused by the same mechanism that is described as coarse mode effects in Ghan et al. (1998) – you added sea salt activates, but has a small number concentration. The effect of the reduced activation of background aerosols due to suppressed supersaturation dominates. The decrease in CDNC in our study is caused by the added sea salt bringing the supersaturation below that necessary to activate the sea salt itself, in addition to the background aerosols. Increasing sea salt emissions further would enhance this behavior. When adding very small amounts of sea salt in a separate experiment we get a decrease similar to that found in your study. We are preparing a separate paper on these results.
- ii) We do not believe that positive forcing resulting from adding particles too small to activate is solely due to their influence on the supersaturation, but rather a combination of this and the effect they have on the background aerosols. We have rewritten the paragraph to point this out:

“Figure 6 and the associated discussion show that adding particles that are too small to become activated may lead to a decrease in the reflection of solar radiation through their effect on the supersaturation and the activation of background aerosols, (...)”

17) We have moved the discussion of the change in LWP and Figure 7(b) (now Figure 4(b)) to section 4.1 to make it clear that the discrepancies we see between the cloud-weighted susceptibility and the forcing in Figure 4(a) are likely to be caused by the second indirect effect. The signals of change in LWP are caused by the second indirect effect and are now discussed in the following way:

“Figure 4(b) shows the annually averaged change in cloud liquid water path (LWP), which is a result of the change in precipitation release following autoconversion dependence on cloud droplet size (Kristjánsson, 2002). This second aerosol indirect effect is not included in the cloud-weighted susceptibility function and leads to discrepancies between results of this function (Fig. 2(d)) and the change in radiative balance at the TOA (Fig. 4(a)).”

18) We agree. Figure removed.

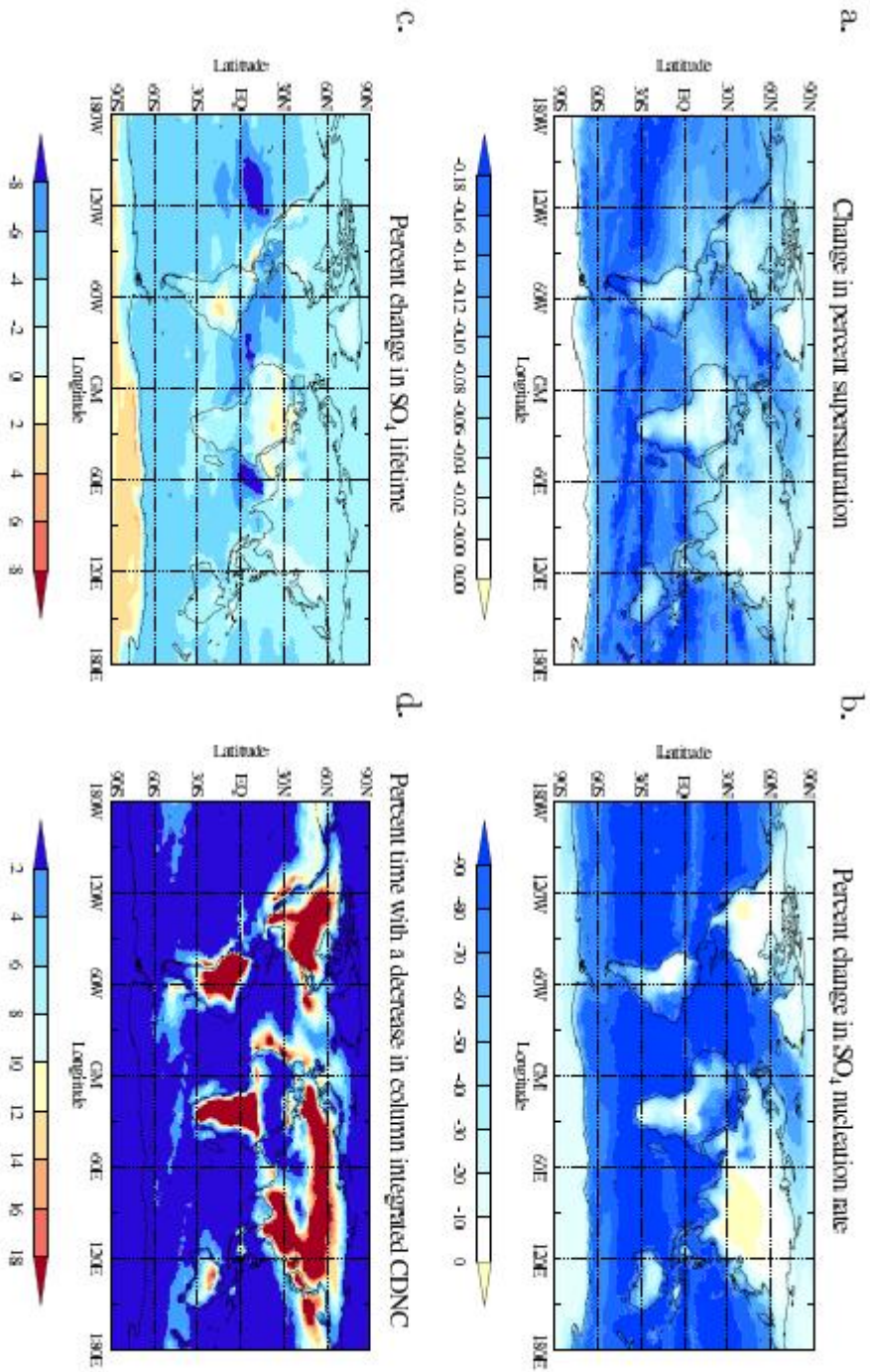


Fig. 6. NorESM: (a) Change in percent cloud supersaturation with respect to water at ~ 930 hPa, (b) percent change in SO_4 nucleation rate at ~ 930 hPa, (c) percent change in SO_4 lifetime, and (d) percent time with a decrease in column integrated CDNC. Annual averages.