

Authors response to: Interactive comment on “The efficacy of aerosol-cloud-radiative perturbations from near-surface emissions in deep open-cell stratocumulus” by Anna Possner et al.

Anonymous Referee #1

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Possner et al. present a modelling study in which the effects of idealised ship emissions on clouds and radiation are examined. Tools and analysis are adequate, and the diligent analysis by Possner et al. allowed for very important insights. A particularly important finding is that a substantial increase in all-sky albedo even relatively far away from the aerosol emission source may be expected, and that there is no simple way of identifying these changes given the large natural variability of the cloud properties. With a known location of the emission source, in turn, detection and attribution seems feasible.

The study is very relevant to the readership of Atmos. Chem. Phys. and should be published after some minor revisions.

We thank the reviewer for the assessment and have addressed all issues raised in the revised manuscript. Please find our response to each of the individual comments below.

The most important one is that the authors need to clarify better than they did which quantities are reported as all-sky, and which as cloudy-sky averages. Some further mistakes or unclear aspects that I list below also should be corrected.

Domain means have been clarified with respect to when they were taken over all sky, cloudy sky or clear sky. To this end the captions of Table 1 and Table 2 were revised and minor edits were undertaken in the main text of the manuscript.

Specific comments

p1119 – the forcing is for the anthropogenic perturbation of the aerosol

This was corrected in manuscript.

p215 – invert the order of the Durkee references

Done.

p2115 – Toll rather did conclude that the LWP change was small

The increase in LWP was quantified in main text for accuracy. According to Fig.2 of Toll et al. (2017), the increase in the precipitating regime was between 16 – 24%.

p2119 – correct “Agency” reference

Done.

p2131 – unit missing (cm⁻³)

Added.

p2132 – reference to Platnick and Twomey is missing

Added.

Table 1 – interquartile range: of the temporal variability of the domain-mean quantities? should be specified. Are LWP and Nd in-cloud or all-sky in both, observations and model results?

The caption has been revised for clarity. Domain-mean and in-cloud Nd are given in the table (correspondingly for observations and simulations). The interquartile range covers the spatial and temporal variability of the cloud field. This is now clarified in the revised caption.

p3114 – The quantities in Table 1 are not numerous, and don't include boundary layer properties

The sentence has been rephrased.

p4120 – a reference for the Quikscat data source would be good

Added.

p5110 – for completeness, the symbols τ , h , and z could be explained, too. ρ_w presumably is a constant?

A definition of all variables has been added.

p616 – how do the authors come to the conclusion that these are “realistic”?

The cell sizes of open cells observed during VOCALS-REx were around 30-40km (p3L15), which is consistent with Fig.1d. Rephrased “realistic” with “observed”.

p716 – if the authors want to make this point, they should consider listing the observed range in Table 1.

A reference to Fig. S2, where the range of observed R_{cb} is shown, has been added.

Fig. 2 – how is cloud top defined?

Reference to definition of cloud top given in revised caption of Table 1 has been added.

Table 2 – the caption should report the definition of “ship-seeded” / “ship-unseeded” as well as “detrained” and “wall”

The definitions have been added to the caption. Entries are rephrased as “seeded” and “unseeded” also.

p8114 – the plume with $N_a > 1000 \text{ cm}^{-3}$ seems to me much narrower than 60 km. I would guess, only a few km

The 60 km refers to the length and not the width of the plume. This has now been clarified in the revised manuscript.

p914 – were \rightarrow was

Done.

p10 last line – reduction compared to what? to the ctrl simulation?

The decrease in N_{d_top} is with respect to the cell-wall cores. This has been clarified in the revised manuscript.

p1217-9 – I have difficulties following this argument. What is meant by “domain-average A_{cld} ”? Is it what is labeled “albedo” in Table 2? It is hard to understand that the increase in in-cloud albedo by 5

It was meant to be “all-sky” albedo. The revised caption of Table 2 together with the renaming of “albedo” as A_{all} and A_{cld} should clarify this discussion in the revised manuscript.

The calculation is that:

$$CF_{filament_ship} * A_{cld_filament_ctrl} + CF_{wall_ship} * A_{cld_wall_ctrl} = 0.9 * \frac{CF_{filament_ship} * A_{cld_filament_ship} + CF_{wall_ship} * A_{cld_wall_ship}}{CF_{filament_ship} + CF_{wall_ship}}$$

where e.g. “ $CF_{filament_ship}$ ” corresponds to the cloud fraction entry of Table 2 for the cloud filaments of the ship simulation.

p12117 – this decrease in in-cloud LWP is not documented in the Tables or figures. Or is the area covered by detrained + wall the entire cloudy area?

Yes, the table entries under filament and wall are cloud-mean only, and one can see the decrease from detrained ctrl to detrained seeded of the ship simulation. This is now clarified in the revised caption of Table 2.

p12120 – given the relatively small mean increases in N_d , and the fact that albedo is much less sensitive to N_d changes than to L changes, is this plausible? Or is the distribution of the relative changes of both quantities relevant?

The increase in in-cloud A_{cld} of the filaments was attributable to changes in N_d and, therefore, the Twomey effect. In-cloud mean LWP within the filaments is found to decrease and, therefore, cannot contribute to the brightening. However, it should be noted that 90% of the increase in domain-mean A_{cld} does not come from brightened clouds, but is attributed to the increase in cloud fraction assuming an equally bright cloud distribution. This should now be clearer in the revised manuscript given the more careful definition of all-sky and in-cloud mean quantities.

p1415 – it probably rather is “satellite-based estimates in CRE changes”

Done.

p14116 – maybe specify “even in the absence”?

We decided to keep the sentence as is.

p1611 – superfluous comma

Removed.

p16126 – the

Done.

p16127 - “annual mean insolation”

Changed.

p16131 – extent

Changed.

Supplement

Fig. 2a – this seems to me an awkward definition of a CDF. I am used to CDFs that increase monotonically and asymptotically approach 1 (as in Fig. 5, in fact).

This was a misnomer of the Figure. Indeed, the probability density function (PDF) is shown. This has been corrected.

Fig. 3a – it would be good to indicate the ship position here, as well as the past ship track

We have played around with super-imposing the aerosol perturbation in a transparent layer over the cloud fields and decided not to do it for clarity of the figures. The information is instead displayed separately in Fig 3 of the main manuscript (Fig 3a - aerosol plume and Fig 3b - Nd_top field) separately.

Fig. 6 – this figure seems to be wrong. I believe the authors intended to show Nd_top vs. Acd histograms in all rows.

Thank you for pointing this out. It has been corrected in the revised supplement.