Response to referee comment #2

In this paper, the authors compare simulations using the ICON model to observationsfrom the ACLOUD and PASCAL campaigns. They find that the ICON simulations predict a more strongly positive cloud radiative effect (CRE) than that derived from the ACLOUD observations. They then determine that an important contribution to this difference is the small number of cloud condensation nuclei (CCN) activated in the ICON model, which subsequently results in low cloud liquid water contents. They improve the model results by accounting for the effects of subgrid-scale turbulence on cloud droplet activation and by scaling their assumed CCN profile. I feel that the study merits publication, provided that the following comments are addressed.

We thank the reviewer for the constructive comments that helped to improve the manuscript.

General comments

1. The authors briefly mention cloud ice in a few places in the paper, but they largely restrict their analysis to liquid cloud water. Some definitive or quantified statements about the contributions of ice clouds to the radiation balance or hydrometeor concentrations, both in ICON and in the observations, would be welcome. Could differences in the amount of frozen cloud make a significant contribution to differences in the surface radiation balance or the cloud radiative effect between the model and the observations?

From the observational side, it is difficult the quantify contribution of ice clouds to the radiation balance or hydrometeor concentrations as the amount of ice in the clouds during ACLOUD and especially during the period of our sensitivity study was relatively low and often times below the detection threshold of the in-situ probes. Looking at the ICON model, we have performed a sensitivity analysis in which we turned off any radiative effect of cloud ice. If one compares the radiative variables like surface CRE (see Figure 1 and Figure 2) and F_{net} at the surface (not shown), the differences between our basic set up of ICON and the one without an effect of cloud ice on the radiative field is small and on the order of 1 $\mathrm{W\,m^{-2}}$. This is due to the already low cloud ice fraction in the model, which also causes the radiative effect of cloud ice to be low. Due to the limitations of the observational dataset in terms of cloud ice, it is hard to constrain the model from the observational side. Therefore, any estimation of the impact of cloud ice on the radiative balance has to be interpreted with some caution. We added this information to the revised manuscript.

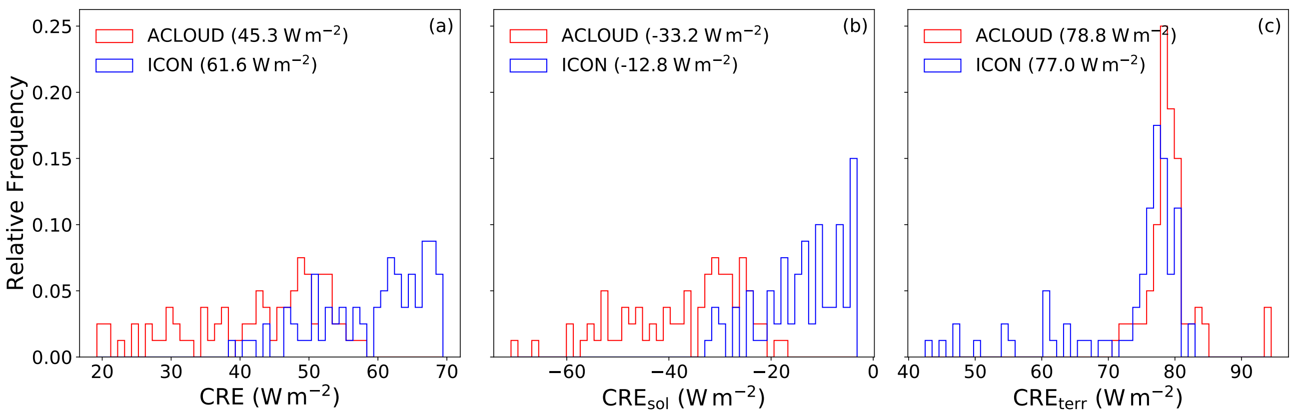

Figure 1: As Fig. 4 in the revised manuscript but for the period from 2 June to 5 June.

2. Sect. 3.2, p9: The authors mention here that the CRE is calculated from the observations through the methods of Stapf et al. (2019a). Given that there are potential inconsistencies in the calculated CRE between the model and the observations, just a little more detail on the radiative transfer simulations of Stapf et al. (2019a) seems prudent here.

In the revised manuscript, more information on the radiative transfer simulations are given.

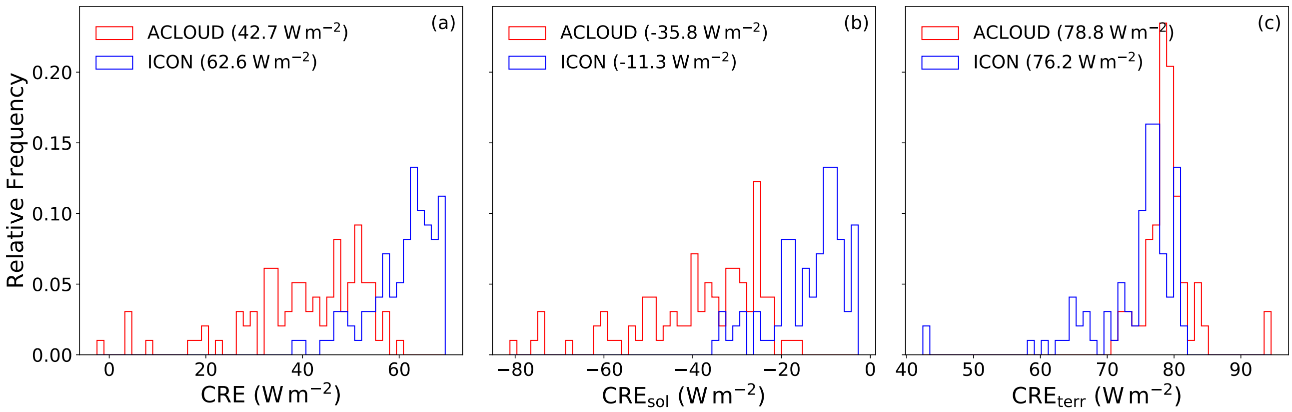

Figure 2: As Fig. 4 in the revised manuscript but for the period from 2 June to 5 June and without effect of cloud ice on radiation.

The authors mention that "While the prescribed functional dependence of the sea ice albedo has been derived for cloudless and cloudy conditions, the surface albedo that is used to derive the CRE from the observations is for cloudy-sky only. This can lead to inconsistencies between the modeled and observed CRE (Stapf et al., 2019a)." However, If I understand correctly, the radiative transfer simulations of Stapf et al. (2019a) account for cloud surface-albedo interactions. Given that the surface albedo is prescribed in the ICON simulations, these cloud-surface-albedo interactions will not be accounted for in the ICON simulations. Therefore, wouldn't it be a more consistent comparison if the cloud-surface-albedo interactions were also neglected in CRE calculations based on the observed data? Can the authors comment on this?

The radiative transfer simulations to derive the CRE from the observation are different from the ones in Stapf et al. (2019a) as in our study, the albedo from all-sky conditions was used. All-sky albedo was also used to derive the functional dependency used that we implemented into ICON for the purpose of this study. We now explicitly state that all-sky albedo was used and removed a misleading citation to Stapf et al. (2019a) to avoid confusion.

Specific comments and technical corrections

p2, line 38: optical → optically
Changed.

p2, lines 44-47: Please improve the clarity of this sentence.
Following the advise by reviewer #1 to reduce the LES vs kilometer-scale simulation, this sentences has been removed in the revised manuscirpt.

p3, line 74: sea ice covered → sea-ice-covered
Changed.

p3, line 83: unmatched parenthesis: "given (for"
Parenthesis added.

p3, line 84: refer → refer the reader to
Changed.

p5, line 108: "general feature of ICON." Perhaps the authors mean "generally representative of ICON"?

Changed.

p5, line 120: "caused by the way how our simulations" Please either choose "the way that" or "how".
Changed to "how".

p8, line 176 "sea ice covered surface". This should be either "the sea-ice-covered surface" or "sea-ice-covered surfaces".
Changed to "sea-ice-covered surfaces".

p8, line 189: "Figure 3 a" → "Figure 3a"
We refer to Figure 3a in the following sentence, and this sentence was intended to generally introduce this figure.

p9, line 200: Please insert a comma after "without clouds"
Comma inserted.

p9, line 202: "measurements of atmospheric/surface observations". Perhaps the authors mean "atmospheric or surface measurements" or "atmospheric or surface observations"?
Here, we refer to observatoins of the atmosphere (i.e. dropsonds) and of surface properties (i.e. albedo). We reformulated this sentence to be more concise.

p9, line 211: Please either choose "The way that" or "How".
Changed to "The way that".

p9, line 212: "allows to narrow down, which effect" → either "allows us to narrow down which effect" or "allows one to narrow down which effect".
Changed to "allows us to narrow down which effect".

p9, line 212: "If clouds would be" → "If clouds were"
Changed.

p9, line 215: "fraction" → "ratio"
Changed.

p10, line 226: "which allows to" → "which allows us to"
Changed.

p11, line 260: "extend" → "extent"
Changed.

p13, line 301: large → larger
Changed.

p13, line 302: stems → stem
Changed.

p14, lines 327-328:The overestimation of small hydrometeors mentioned here seems to be in contradiction to the statements of p12, lines 278-280.
Here, we refer to the overestimation of small hydrometeors in Schemann and Ebell (2020). Due to the much finer resolution of their ICON simulations, the activation of CCN into cloud droplets can be sufficiently resolved and any bias is only to the unsuited background CCN profile for an Arctic domain. Neverthless, we revised this sentence to make that clearer that we refer to the simulations in Schemann and Ebell (2020).

p16, lines 393-394: Since the last simulation discussed was not the default set-up but instead was the one using the revised CCN activation scheme, most readers would assume that the authors are comparing the simulation with the CCN scaled by 0.4 to the revised CCN activation simulation. The authors need to make it clear that they are comparing this simulation to the default set-up.
It has been clarified in the revised manuscript that we scaled the revised CCN activation simulation and not the default set-up.

p17, lines 411 and 414: Do the authors mean Figure 9f instead of 9e?
Yes indeed, Figure 9f is the one we refer to. This has be changed accordingly.

p20, eq. B3: If I divide eq. B2 with $k = 3$ by eq. B2 with $k = 2$, I find the trailing factor to be $A^{-1/\mu}$, not $\lambda^{-1/\mu}$. Is the error in eq. B2 or eq. B3?
Thanks for thoroughly going through the equations. Indeed, there is a typo in B2 as there has to be a λ in the denominator instead of A. This has been corrected in the revised manuscript.

Figure 1 caption: "inner domain has a" → "inner domain (red) has a"
Changed as proposed.

Figure 5 and p11, lines 258-261: There is significant overlap in the points on this plot, which makes it difficult to tell, for example, how large a fraction of the data have observed cloud depth < 0.4 and modelled cloud depth < 0.2. This also means that it is difficult to judge the degree of underestimation of the cloud depths. I don't have a perfect solution for this issue, but the authors may wish to consider making the data points partially transparent, or substitution of the scatter plot with a histogram (with different subplots for the different observation days, if the authors wish). I am open to other solutions, or arguments from the authors in favour of the current plot. In any case, the median values of the modelled and observed cloud depths should be provided to help the reader quantify the degree of underprediction. The means and standard deviations may also be helpful.
We revised this figure and now display the bias in the form of a histogram. The mean and the standard deviation of the depicted histogram are given in the revised manuscript.

Figure 6, Figure 7, and Figure 8: The red lines for panels b and c are very similar in the three figures, but not quite identical. Note for instance that the peak in frequency of hydrometeor number concentration is > 100 in Figure 6 and < 100 in Figure 7 and Figure 8. Rather than state in the captions that the lines are identical, the authors instead should very briefly remind the reader why the lines differ slightly. Also, it seems that the red and blue lines are reversed in panel a in all three figures.
The lines in Fig. 6-8: Panel a) are indeed swapped, which has been corrected in the revised manuscript.

Figure 9: It would be prudent to remind the reader in the caption that the red lines differ slightly due to the sampling that is applied.
We added a remark in the caption of Figs. 6-9 that the red lines differ due to the sampling strategy employed.

Further revision

In line 186 of the submitted manuscript, the threshold for a surface to be classified as sea ice covered should be 0.7, not 0.5. This has been corrected in the revised manuscript.

References

Schemann, V. and Ebell, K.: Simulation of mixed-phase clouds with the ICON large-eddy model in the complex Arctic environment around Ny-Ålesund, Atmospheric Chemistry and Physics, 20, 475–485, https://doi.org/10.5194/acp-20-475-2020, 2020.