Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-641-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Employing airborne radiation and cloud microphysics observations to improve cloud representation in ICON at kilometer-scale resolution in the Arctic" *by* Jan Kretzschmar et al.

Anonymous Referee #2

Received and published: 31 July 2020

In this paper, the authors compare simulations using the ICON model to observations from 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:

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?

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.

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?

Specific comments and technical corrections:

p2, line 38: optical -> optically

p2, lines 44-47: Please improve the clarity of this sentence.

p3, line 74: sea ice covered -> sea-ice-covered

p3, line 83: unmatched parenthesis: “given (for”

p3, line 84: refer -> refer the reader to

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

p5, line 120: “caused by the way how our simulations” Please either choose “the way that” or “how”.

p8, line 176 “sea ice covered surface”. This should be either “the sea-ice-covered surface” or “sea-ice-covered surfaces”.

p8, line 189: “Figure 3 a” -> “Figure 3a”

p9, line 200: Please insert a comma after “without clouds”

p9, line 202: “measurements of atmospheric/surface observations” Perhaps the authors mean “atmospheric or surface measurements” or “atmospheric or surface observations”?

p9, line 211: Please either choose “The way that” or “How”.

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”.

p9, line 212: “If clouds would be” -> “If clouds were”

p9, line 215: “fraction” -> “ratio”

p10, line 226: “which allows to” -> “which allows us to”

p11, line 260: “extend” -> “extent”

p13, line 301: large -> larger

p13, line 302: stems -> stem

p14, lines 327-328:The overestimation of small hydrometeors mentioned here seems to be in contradiction to the statements of p12, lines 278-280.

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.

p17, lines 411 and 414: Do the authors mean Figure 9f instead of 9e?

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?

Figure 1 caption: “inner domain has a” -> “inner domain (red) has a”

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.

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.

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.

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