Minor revision

of the manuscript ACP-2022-395: “Investigating the radiative effect of Arctic cirrus measured in situ during the winter 2015/2016” by Marsing, Meerkötter et al.

Dear referees, dear editor,

First of all we thank you very much for re-evaluating our changed manuscript, based on your original comments. We are pleased that the revised manuscript now delivers the quality and comprehensiveness that you expect for publication in ACP. In the following we address the remaining comments.

Yours sincerely,

Andreas Marsing, on behalf of the authors

Comments by anonymous referee #1

The authors successfully address all my comments in the revision submission. I don’t need to see the manuscript again. Below are a few minor suggestions left for the authors to decide if they feel it is good to take. The suggestions may help further polish the manuscript.

Minor suggestions:

Lines 402-404. Yes, the horizontal solid and dashed gray lines in Fig. 9 are above their counterparts in Fig. 8. In other words, the net upwelling longwave irradiance is smaller at the surface than that at the top of the atmosphere. However, the slopes of the colorful curves in Figs. 8 and 9 are not affected by the offsets (or y-intercepts) of the curves. Are the reduced slopes of the colorful curves in Fig. 9 probably due to the higher proportion of the diffusive downwelling shortwave irradiance at the surface than that at the top of the atmosphere? Is the decrease of the diffusive downwelling shortwave irradiance with increasing sza less pronounced than the decrease of the direct downwelling shortwave irradiance with increasing sza?

In other words, with decreasing sza, for example for sza < 65°, differences in SW contribution to the net irradiance (F_{net}), when comparing the TOA and BOA cases, become increasingly smaller as a result of strong forward scattering of SW radiation at the ice crystals. Considering the extreme case where the ice cloud would represent a "pane of glass" at small sza values, curves for $F_{net,\ TOA}$ and $F_{net,\ BOA}$ would coincide at e.g. sza = 0°. Thus, the connecting line of F_{net} values at sza = 90° and at sza = 0° would result in a smaller slope in the BOA case.

Accordingly, we added a sentence at the end of line 404: “At the other end of the x-axis at small sza values, the curves of $F_{net,\ BOA}$ (Fig. 9) and $F_{net,\ TOA}$ (Fig. 8) tend to converge for the same albedo values, a consequence of increasing forward scattering of incoming SW radiation at the ice crystals. Generally, with decreasing sza an increasing shortwave component … “

Lines 424-425. Does the relative importance of surface albedo vs. cloud albedo play a role in the cloud radiative forcing shown in Fig. 10? Is the sign of cloud shortwave radiative forcing predominately determined by whether the cloud albedo is greater or smaller than the surface albedo at a given sza?

Yes, your statement is correct and illustrative. In line 427 we therefore added: “In general it can be said that the sign of $RF_{SW,\ TOA}$ is predominantly determined by whether the ice cloud albedo is greater or smaller than the surface albedo. Furthermore, ice cloud layers emit …”

Section 5.2.4 and Lines 540-542. In the authors' response to my major comment in the last review, they show that the longwave heating/cooling rate profile is affected by the temperature profile. It appears that a small lapse rate within the cloud layer results in longwave radiative warming at the cloud base and longwave radiative cooling at the cloud top, whereas a large lapse rate within the cloud layer results in longwave radiative warming at the upper portion of the cloud layer and longwave radiative cooling at the lower portion of the cloud layer. For the selected case on January 25, 2016, the lapse rate between 6 km and the tropopause is near the superadiabatic lapse rate. Not sure if such a steep upper tropospheric temperature gradient is common over the Arctic, but this is interesting to me. One of focuses of this study is the influence of ice cloud radiative effect on the thermal stratification. As introduced in numerous previous studies such as those I listed in the last review, cloud base absorption and top emission result in longwave radiative heating at cloud base and cooling at cloud top in general. Hence, the cloud longwave radiative effect generally tends to decrease the static stability of the cloud layer. However, in this study, the authors show that if the lapse rate is near the superadiabatic lapse rate within the cloud layer, longwave radiation heats the upper cloud layer and cools the lower cloud layer and hence tends to increase the static stability. Would it be helpful if such a contrast is emphasized?

This is a very interesting observation, thank you for pointing this out! Indeed we observe rater large lapse rates ($\gtrsim 8\ K\ km^{-1}$) within the cloud layer in many of our profiles. As stated this might not be representative, but seems worth mentioning. Although this is not a purely cloud-induced effect, it is amplified by the presence of the cloud. There we added in line 547: "The strong tropospheric lapse rate in some of our observed cloud profiles induces a stabilizing radiative effect of the cloud in most of the tropospheric heating rate profile inside the cloud layer, with an overall higher (LW) heating rate at the cloud top (warming) than at the cloud bottom (less warming or cooling)."

Comments by Yi Huang (referee #3)

The authors well addressed my comments, especially those on IWC uncertainty and radiative transfer modelling. I appreciate their efforts and am glad to see that the quality of the paper noticeably increased and to find some new/revised results quite interesting, such as the dependence on profile resolution. I think the paper is largely ready to publish as it is. I'd appreciate it if they could address the following further comments - these should be considered minor (non-obligatory) suggestions:

1) separate fig 12 to LW and SW components, to identify which component accounts (more) for the variation;
   Although an interesting suggestion, we would like to not separate LW and SW components in this section and leave their treatment to future work, which should then include a more detailed discussion of how the artificial cloud layers are set up.
2) add a column in fig 11 to show the heating rate itself (besides the difference), so that the difference can be appreciated in a context;
   We agree that showing only the heating rate differences might be unsatisfactory and added another column with the absolute heating rates.
3) The 5x perturbation experiment of fig 13 looks very interesting in that the effect is shown to not simply scale with IWC. It's however a quite lonely result concerning this dependency (total ice amount). Why not include irradiance results as well and examine it over more values of IWP (like fig 8/9 for SZA dependency)? I am particularly interested in whether the sign change of cloud forcing as reported by Tan et al. (2016, doi:10.1002/2016GL071144, their fig 3j, explained in fig S4) can also happen in the Arctic cirrus case.
   Studying the sensitivity of the radiative effect on (artificially) varying IWP certainly sounds interesting to us. The 5x perturbation was included to estimate the range that might appear if the measurements were severely affected by undersampling of ice particles (as explained).

However, we wanted (and were advised by reviewers) to keep the study as close as possible to the observations, and no overdo with sensitivity results. The few cases also do not fully represent the range of optical thicknesses of Arctic cirrus. Still, a hint on changing signs of cloud radiative forcing can be found in Fig. 10, comparing panels (a) vs. (c) or (b) vs. (d). In our view, a more detailed study on the IWP/optical thickness effect should be based on further observations.

Comment by anonymous referee #4

The authors have satisfactorily replied to all of my review comments/questions and have revised the manuscript accordingly. I have no further comments, other than to consider revising the scaling of tick marks on the upper x-axis in Fig. 6. Major tick marks are in increments of .0025 g/m3 for IWC, but minor tick marks are in increments of .000625 g/m3 (5 tick marks would give increments of .0005 g/m3).

Yes, scaling of tick marks on the upper x-axis in Fig. 6 should be revised. This has been done.

Own changes

We adjusted the colouring or added symbols to the data in Figures 7, 8, 9, 10, 11 and 12 to better respect the needs of people with colour blindness handicap.

Regardless of the reviewers' comments, we would finally like to make a small change to Figure 7. In Figure 7 the previous red curve should be omitted since by mistake it is still based on the two-stream and not on DISORT calculations. Furthermore, data from the red curve is not used in the radiative transfer calculations and it is not discussed in the text, neither is it planned anymore. The new Figure 7 shows the data which are actually used in our model simulations. This implies a small change also in the corresponding figure caption.