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Discussions

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Comment on acp-2022-140

Anonymous Referee #2

Referee comment on "Impacts of active satellite sensors' low-level cloud detection limitations on cloud radiative forcing in the Arctic" by Yinghui Liu, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-140-RC2, 2022

This study examined the potential biases in the joint CloudSat/CALIPSO cloud mask and radiative fluxes retrievals. Due to the difficulties in distinguishing the cold surface from clouds by passive sensors in the polar regions, the joint CloudSat/CALIPSO observations are critical to study polar clouds and their radiative effects. Although the limitations of the CloudSat CPR and the CALIPSO cloud observations and retrievals are well known, this is the first study to systematically quantify the potential biases in the CloudSat/CALIPSO cloud cover and radiative forcings due to such limitations.

This study uses the ground-based MMCR retrievals during SHEBA as input to the QuickBeam radar simulator to simulate cloud profiles from the perspective of CloudSat CPR and the CALIPSO CALIOP. The effects of ground clutter of the CPR signals are carefully approximated. Adding the approximate ground clutter together with the simulated CALIOP integrated signal attenuation, the author identifies the missing clouds in the simulated profiles using a simplified version of the joint CloudSat and CALIPSO cloud mask. Indepth and detailed analysis of the contribution to the missing clouds and the cloud radiative forcing (CRF) from clouds of different phase are presented. The author finds monthly CRF uncertainties up to 2.7 W/m2 at the surface and 4.0 W/m2 at TOA and the uncertainties are up to 30 W/m2 in individual cases.

The findings of this research provide a more solid basis for the studies of the Arctic cloud and cloud radiative effects. This manuscript is well suited for publication at the *Atmospheric Chemistry and Physics*. At this point, it still requires careful revision before publication, especially for Section 3.2, mostly with clarification and improvements to presentation. Detailed comments are listed below.

General comments:

Maybe consider moving redundant information to the supplement materials, especially the long tables and Figures from previous publications. I do appreciate the author having all the data in the table for easy and quick reference but having them in the manuscript sometimes disrupts the flow of the presentation.

Specific comments:

P2, Line 23: “CALIPSO” is capitalized here. Please use consistent abbreviations throughout the manuscript.

P2, Line 23: “high-level clouds”, this sentence is ambiguous because the signal attenuation issues can happen for clouds at any level.

P3, Line 30: It should be Shupe et al. 2006.

P4, Line 6-7: Is there an estimate of how many profiles are excluded? Maybe adding a qualitative estimate of how such selection will affect the total CRF in later sections as well? Including such assessments in the manuscript will help the readers to interpret the results of this study more accurately.

P4, Line 16: It is mentioned on P7 that solid hexagonal column is used for ice particles in radiative transfer code. Is that consistent with the Quickbeam setting here? By default, Quickbeam uses ice spheres. If solid column is used here, are other parameters adjusted as well, such as for area/mass ratio?

P4, Line 18: Haynes et al. 2007 for the BAMS paper about Quickbeam.

P5, Line 12-13: Does this statement refer to subtracting the estimated mean clutter from the received power of the lowest four bins? Marchand et al. (2008) mentioned it as preliminary. Was this approach shown to be effective in later studies or used in later version of the CloudSat cloud mask processing?

P8, Line 8-11: These two sets are referred to frequently later in the manuscript. Maybe it is useful to have an abbreviation/code name for each of them? The efforts to describe them accurately make sentences long, convoluted, and confusing. Similarly, it might be worthwhile to find a more succinct way to address cases when clouds of a specific phase are missed by the combined CloudSat/CALIPSO mask.

P9, Line 8-11: Maybe specify the sign of the difference here as well? I can tell from the captions of Fig. 10 and 16 but it will be easier for the readers to follow to clarify.

Fig. 6 and 9: Maybe use thicker lines?

P16, Line 10: The time period of negative CRF from SHEBA is much shorter than the results by Kay and L'Ecuyer (2013). The SHEBA CRF is likely biased because most of its albedo observations were taken mostly locally from ice. It is true that they used a 200 m line to sample more surface types, but it was not likely going to represent the extent of open water over a larger area. The length of negative CRF may vary with latitude and the time span of surface melt/open water. So, the SHEBA CRF does show the typical seasonal cycle of the CRF in the Arctic Ocean but might not be an accurate representation of the Arctic Ocean in summer.

Fig. 10 and 16: Maybe change to two panels, one for CRF for all profiles, and one for the difference of CC from all? The differences are difficult to tell in a single plot.

P18, Line 6: How about adding the last sentence of caption in Fig. 11 here too? It helps to remind the reader that CRF differences within 2 W/m2 are not included in the following analysis.

P18, line 8-9: This sentence is confusing, please rephrase.

P19, Line 14: Does this mean the low-level clouds around 1 km is still opaque enough that the effective emitting temperature are close to when the clouds below 1 km are included?

P19, Line 15: Figure 11, S6?

P19, Line 15-17: this sentence is long and confusing. Please rephrase.

Fig. 13 15, and 18: These scatter plots are pretty noisy. Because the optical depth is related to the fraction of reduction of incoming radiative fluxes and not with the absolute values of the fluxes, normalizing the CRF differences with the solar zenith angle for SW and Ts^4 for LW may produce a cleaner fit.

P20, Line 1: I think the temperature of clouds below 1 km should be compared to the effective emitting temperature of the clouds above 1 km instead of to the surface temperature. The LW CRF being small in winter suggests the clouds above 1 km emit at a temperature cloud to the temperature within 1 km. Fig. S1 shows a liquid layer above but close to 1 km, which is consistent with LW CRF being small. The question is how representative this case is in winter.

P22, Line 9-10: This statement is not exactly accurate. The clouds are brighter than the surface in the summer mainly because the surface has more open water, more ponds, and less snow-covered ice rather than clouds being brighter.

P22, Line 12-13: Is it possible that the LW effective emitting temperature at TOA is much higher in the atmosphere that the BL inversion is not relevant at tall?

P23, Line 9-10: Maybe this is another sign that the LW emitting temperature is above 1 km?

P24, Line 7: "... determine the CRFs" to "... determine the CRF changes".

P25, Line 11-12: Add comma between "Cloud especially ..." and add comma between "… near the surface help brighten …".

P25, Line 12: Remove the second "larger".

P25, Line 13: Remove “more”.

P25, Line 14: Remove “larger”.

Fig. 20, 21: use thicker lines and larger fonts.

P26, Line 22: Is there a reason for Quickbeam to perform better in ice clouds? Comparing Fig. S1 and Fig 19, the large reflectivity differences are in a slightly shallower layer than the retrieved liquid layer in Fig. S1. Maybe the larger differences in reflectivity of liquid clouds are related to the uncertainties in the height of phase transition? Using the refractive index of water for ice clouds will increase reflectivity significantly. If you set the places with large liquid reflectivity differences to ice phase for the case in Fig. S1, would it get rid of the large reflectivity differences? If so, it might point to phase retrieval error.