Atmospheric Chemistry and Physics Discussions

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Comment on acp-2022-433

Anonymous Referee #1

Referee comment on "Supercooled liquid water clouds observed over Dome C, Antarctica: temperature sensitivity and cloud radiative forcing" by Philippe Ricaud et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-433-RC1, 2022

Summary

The authors used ground-based remote sensing observations collected during the summertime over the course of 4 years at a continental site in Antarctica to explore the change in net surface fluxes caused by the presence of supercooled liquid-phase clouds. The authors find a strong relation between condensate amount (i.e., liquid water path, LWP) to ambient temperature. By using a clear-sky reference (i.e., selected days that were cloud-free), the authors extract changes in the surface radiative fluxes as a function of LWP and predict an Antarctic-wide change in the surface net radiative budget.

The article is well written and contains interesting analysis. As listed below, there are several major concerns that make it difficult to support the authors’ conclusions. Since the article touches on a highly relevant topic, I recommend a resubmission of this paper after resolving all major concerns.

Major concerns

My main concern is the choice of clear-sky reference. Selecting clear days as reference has two major flaws: (1) the profiles of temperature, humidity, and aerosol properties may change drastically enough to introduce a bias when assessing clear-cloudy changes in surface radiation budget components; (2) this method deviates from traditional model-based assessments that simply perform radiative transfer calculations with and without cloud condensate on the same vertical profiles. A perfect clear-sky estimate would produce zero change in net surface radiative fluxes where clouds are absent. However, looking at Fig. 5 (top, e.g., 5-6 and 18-19 UTC) the change in non-zero. Similarly, relations shown in Fig. 7-9 (middle) show non-zero change in surface budget components when the LWP approaches zero. These examples hint at an underlying bias of the analysis. In their revision, the authors need to repeat their analysis using a typical broadband radiative transfer code (e.g., RRTMG) to verify the change in net radiative fluxes.

Looking at Figure 5 (top) the change in net radiative fluxes is strongly affected by spikes in upward and downward shortwave radiative fluxes. While the reason for these spikes may simply be rooted in three-dimensional radiative effects, it is unclear how these spikes affect the analysis. The authors should justify their current approach or else find a way to filter for homogenous conditions or smooth the measured shortwave fluxes.

The article contains numerous instances of vague or non-scientific language that confuse when reading. For example, the authors refer to "liquid water concentration" (l. 79) which should probably be "liquid water content". Instead of listing a measurement of "radiation" (l. 92), the authors need to be more specific (i.e., "radiative flux" or "irradiance"). Measuring "aerosol and clouds" (l. 112) leaves a lot of room for speculation and needs to be specified. "Temperature" is a key metric and it is unclear whether the authors used potential temperature of liquid-water potential temperature. The latter one is a conserved variable in a well-mixed, cloudy boundary layer and should be used. Using the former one (as maybe done here) would introduce a systematic error into the analysis that affect a key finding (i.e., a LWP-temperature relationship). The authors need to clarify which temperature was used and also over which altitudes the temperature was averaged (I'm guessing it is the cloud layer?).

Looking at Figure 1, there appears to be frozen precipitation below some clouds (e.g., 7-8 and 18:30 – 19:30 UTC) as indicated by relatively great backscatter and depolarization ration. It is unclear how (any) precipitation affects the HAMSTRAD liquid water content (or path) retrieval; the authors need to explain this. Furthermore, frozen precipitation hints at frozen particle inside clouds; the authors should discuss this issue and ideally refine their identification of supercooled clouds to ensure ice-free conditions.

Minor concerns

ll.. 53-54 While hydrometeors are larger, does the total water path change (or does it stay constant)? Please report.

ll. 55-60 This sentence is too long and lacks clarity. For example, what object is referred to in "two to three times lower…".

ll. 68-69 This sentence makes is sound like supercooled water emerges from heterogenous nucleation. Please rephrase.

l. 72 Please find an appropriate reference (perhaps within Storevlmo and Tan, 2015?).

l. 92 Please change “radiation” to “radiative flux”.

l. 112 Please specify “aerosol and clouds”. Which properties were retrieved?

l. 149 Please add “radiative” before “fluxes”.

ll. 191-192 This sentence needs rephrasing to improve readability.

ll. 266-268 I’m assuming liquid-water potential temperature was used (i.e., a conserved variable in a cloudy boundary layer). Also, could these warm events also be moist – in other words, are there perhaps warm-moist intrusions that explain this LWP-temperature relation?

ll. 300-302 This sentence contains too many verbs. Perhaps remove “is”.

ll. 382-385 Not sure what “This” refers to. Please rephrase.

ll. 444-445 The link doesn’t work. Please check.

Section 5.1 – The authors start a line of thinking that seems unfinished. I suggest the authors either flesh this out and compute whether a change in temperature truly explains the change in LWP or else shift the focus of this subsection to discuss the influence if warm-moist intrusions (as suggested above) or other meteorological drivers.

Section 5.5 – To make this rough estimate believable the authors should report whether LWPs in other Antarctic regions are comparable.

Figure 1 - Please improve the lower end of the color scale for depolarization ratio (i.e., near 5% that is used as threshold).

Figure 2 – The detailed images should be shown at higher quality to enable a visual assessment of the presence of ice-cloud features (e.g., halo). Also an image should be shown for the period 4-5 UTC that was brought up in the main text.

Figure 6 – It is unclear which temperature is shown (absolute, potential, or liquid-water potential temperature) and which layers were used for averaging (e.g., in-cloud layers only?).

Figure 6-9 – It is unclear why the authors decided to determine statistics (i.e., the bars) in horizontal direction. Isn't the question for example in Figure 7 "which change in net radiative budget corresponds to a certain LWP" which would call for a statistic per LWP (i.e., producing a vertical bar)?