Review of the revised version of "Supercooled Liquid Water Clouds observed and analysed at the top of the Planetary Boundary Layer above Dome C, Antarctica" by Ricaud et al. (acp-2019-607)

Main comment:

I am very satisfied with the answers and improvements made by the authors to the manuscript. The two case studies are clearly put into context and the authors assess how representative they are. The discussion is definitely a plus and improves the paper. To me the essential aspects are addressed and the paper can be published almost as is, but I list a few points below that should be considered in my opinion.

- Why not showing figures of the modeling experiments in the main text? This is important and interesting to show that the typical case can be simulated by changing microphysical parameters (the partition scheme), while this is not enough for the perturbed case. (please see also my comment of Lines 742-765).
- Please mitigate a bit the SLW cloud vs Mixed-Phase Cloud discussion (please see my comment of the section 7.1)

Line by line comments:

Title – Considering the substantial effort made by authors to include some modeling experiments by changing the ice/liquid partition scheme, I would tend to suggest this title:

"Supercooled Liquid Water Clouds observed, analysed and modeled at the top of the Planetary Boundary Layer above Dome C, Antarctica"

This is also a paper about modeling, particularly in its revised version.

Abstract

L32 "…exhibited SLW clouds": please add "for at least one hour"

1.Introduction

L76: "…sea ice production of ice-condensation nuclei". This is more appropriate to say INPs for Ice Nucleating Particles. However, here, this is also sea ice as a source of CCN and not only INP, which is discussed in the papers. Sea ice could bring CCN in the form of sea salt. Moreover, you are citing Legrand et al. 2016's paper, which I think is about sea salt measurements coming from sea ice. Sea salt is a good CCN, not a good INP, at these temperatures.

4.Typical diurnal case of the PBL

L 322: you defined LT but are using LST here. Please define.

L353-361: you could also say that ARPEGE is missing the precipitating ice as well (comparing Figure 2 and 3) between 0 and 12UTC.

L452-465: Two almost identical paragraphs here. I guess you want to keep the second one only.

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7. DIscussion

7.1 SLW Clouds vs Mixed-Phase Clouds

I see your point. However, when we look at Figures 2a and 9a there is a clear sign – to me – of ice (the streaks) coming from the height where the SLW layer is. It is fine to say that the SLW layer is virtually only liquid but it is difficult to think that no process is taking place that depletes liquid and turns in into ice, hence giving the ice seen below the SLW layer. I think it is difficult to rule out the fact that the SLW layer is not a part from a – microphysically speaking – mixed-phase process. Where would the precipitation come from then? Small (undetected) crystals can be falling out of the SLW layer then grow while they fall so that the lidar detects them, eventually.

Put it more simply I can see why saying that the "ice component, even if present, is irrelevant from a radiative point of view" (Line 658-659). I think it is not from a microphysical point of view. The authors recognize themselves that there might by small crystals not seen by the lidar ("Some S signal is nevertheless present…" L644-645). How do you know the SLW layer is not slowly disappearing also because it is slowly converted into ice, which precipitates (see e.g the end of the day Figure 9a)? In which case the ice microphysics would also be important since it guides the termination of the SLW layer, hence impacting (also!) the radiative budget (indirectly).

Unless the authors have something against this argument, I would like to see something about this in the discussion to mitigate the "SLW cloud". To me you are investigating the SLW layer of an overall mixed-phase process (or say mixed-phase cloud).

More generally it is always difficult to say what is and what is not a mixed-phase cloud and definitions differ especially when observing with different instruments (space lidar vs. ground-based one, for instance).

I think here, it is not entirely fair to the complexity of mixed-phase microphysical processes to treat these SLW layers as “pure” SLW clouds. It just rules out the role ice might be playing in driving their lifetime (hence indirectly impacting the radiative budget).

7.3 SLW Clouds in ARPEGE-SH

L722:
-Then it would be interesting – if easily doable ? – to know how frequent SLW layer with a lifetime >12h (and not >1h) are, so that the first case’s representativity (in terms of SLW layer lifetime) could also be assessed. What does Figure 17 with a 12h- and not 1h-criterium give? This would back up more the “may have a strong impact on the calculation of the radiation budget” (Line 723).

-There is not any section 3.1, only a section 3, now.

L733: “… since the cloud water is not a model control variable in the 4DVar scheme, it cannot be analysed”

I am not sure what is meant here by just “analysed”: do you mean the analysis step of the data assimilation process? or just the fact of analysing an output? I suppose it is the former and then I would advise to say: “updated by the analysis step of the 4DVar data assimilation process”.

L742-765: I don’t see why you would not show the figures with the modeling experiments here (Fig. Supp11 and Supp12), in the main text. You can very well leave the Figure showing the partition scheme in the Supp. Material, however. This is very interesting to see that you manage to find a partition scheme, which improves a lot the modeling for the first case at least, but also is not enough to solve the problem (2nd case). In the second case (Figure Supp 12), it clearly seems that the sudden increase of water vapour (advection?) is not reproduced in the model and should be the reason of the only slight improvement in the SLW modeling brought by the new partition scheme.

8.Conclusions

L779 : recall here “for at least one hour”.