Review 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)

Summary:

The paper investigates the water budget (cloud, water vapour) in relation to the thermal structure of the boundary layer at Concordia Station, Antarctica. It describes two distinct cases studies from the summer 2018-2019 campaign and highlight the impact of the misrepresentation of supercooled liquid cloud in the ARPEGE model on the surface radiative budget. This study shows that the warmer and wetter episode with cloud leads to radiative biases larger by a few tens of W m-2 than for a more typical configuration of the PBL with colder and drier conditions at “night”, when biases of 20-30 W m-2 are already measured. The authors show that this is mainly due to the longwave part of the spectrum, and conclude on the possibly large impact of the misrepresentation of SLW layers on Antarctica’s surface energy budget.

Relevance of the paper and overall comment:

The paper presents very interesting observations of the Antarctic boundary layer, combining cloud, water vapour, and thermodynamic measurements. It clearly demonstrates large biases related to supercooled liquid water (SLW) misrepresentation using a model configuration of ARPEGE (ARPEGE-SH) zoomed in over Dome C. Interestingly, it distinguishes between two PBL regimes, showing that wetter and warmer conditions lead to even larger radiative biases, still related to a misrepresentation of the SLW. To me this dataset allows to address an important question of the link between the modelling of cloud properties (and not just the overall cloud cover) and the surface energy biases measured in Antarctica, which still remain to be 1) understood 2) corrected in NWP models. Moreover, most of the in-situ studies have mostly concentrated on coastal Antarctica so far, and in-situ observations of SLW on the continent are rarely analysed. This study is well in the scope of ACP. I am in favour of its publication in the journal provided improvements are brought to the presentation and discussion of the results. (Minor revision).

Main comments:

My three main points are:

1) Say how the two case studies are representative of the whole summer campaign and please better introduce first the two PBL conditions at once (see my comment on L119 – section 2) and give the synoptic scale context for both cases.

2) Provide with a discussion section, which is currently lacking and/or spread over section 3,4,5, in order to provide the reader with a more synthetical views of what is presented in 3 and 4, before concluding in 5.

For instance, you do not discuss the possible bias due to the water vapour (as GHG) vs. the one due to SLW. Given the data-to-model comparison you show, could we say that both are acting as factors biasing the modelled radiations, rather than pointing at SLW only?

Please discuss the fact that the model shows even larger biases in the perturbed (warmer and wetter) case. Is this because the wetter environment allows for a thicker SLW layer to form (hence larger LWP values) and/or also because larger water vapour biases are measured that day? A discussion should compare both case studies.

Can the vertical resolution of the model be responsible for the poor modelling of SLW through failure of simulating enough supersaturation and the right PBL structure? (one would expect higher vertical resolution to allow to better simulate temperature and supersaturation, for instance).

Are there any comments to make regarding instrumental/observations biases from the LIDAR and/or the HAMSTRAD instrument that could somehow affect the conclusions of the study? For instance, the fact that HAMSTRAD is measuring non-null LWP will LIDAR is not seeing any SLW layer in the first case study is interesting. Can the radiometer-derived LWP be biased somehow for instance when large particles of ice precipitate below the SLW layer? (Is this answered in a previous paper e.g. Ricaud et al. 2010b)

Also, have the authors tried to change some settings in ARPEGE in order to allow for more SLW simulation to happen? Is the model not representing SLW because it converts all the vapour into ice or is this rather that it does not even capture the water vapour right? Or both? What can be discussed regarding that matter by comparing both case studies' simulations?

Would you say the radiation biases spotted for the two case studies are representative of the biases for the entire campaign?

I was also wondering whether you were seeing any aerosol with the lidar that could impact the SW radiations, and that would not be considered in the model?

3) I recommend to reorganise a little bit the paper by:

- introducing a Method subsection in 2. where you present both types of PBL cases and justify why these two are of particular interest (e.g. representative of most of the campaign?) and give the larger – synoptic – scale context (see comment about L119)

- moving both radiation subsections together in separate (sub)section after the descriptions of both case studies in terms of cloud, temperature, water vapour etc. (see my comment of Line 350 - section 3.4.). In doing so, the main and final aspects of this study (the effect of SLW on radiation budget) would come at once, at the end of the results section. Both Figures 9 and 16 are very interesting and showing pictures of the cloud cover at the same time is a very good idea.

- adding a discussion section (cf point 2. above)

Line by line comments:

Title – I would rather say Supercooled liquid “layers” (not “clouds”) as the examples shown here appear more to be mixed-phase clouds (SLW layer + ice in/below the layer). (see e.g. my comment to L392-393)

Abstract

L39 –I would start the sentence with “The second case study takes place on...”

L42 and L44 – Since you already said at L31 what you define by a “typical” PBL you do not need these quotation marks here, I think.

L46-48 – I am not convinced by this sentence which is very general compared to the text above and suggests that SLW is absent from all NWPs model over Antarctica, which might to some extent be true, but still this is not shown in the present paper. The verb “indicate” is also not very clear. I would suggest to rewrite this sentence by simply stating that the correct modelling of SLW layers appears crucial to achieve the correct representation of the surface energy budget of the polar atmosphere on the continent.

1.Introduction

L58 – There are other papers to cite here:

- the Bromwich et al. 2012 cited above

- Listowski, C., Delanoë, J., Kirchgaessner, A., Lachlan-Cope, T., and King, J.: Antarctic clouds, supercooled liquid water and mixed phase, investigated with DARDAR: geographical and seasonal variations, Atmos. Chem. Phys., 19, 6771–6808, https://doi.org/10.5194/acp-19-6771-2019, 2019.

L59 – “(<30%)”: This is what Adhikari et al. say in their abstract but please note in winter, when the cloud cover increases over the Plateau, it is more than 30% over at least half of the Plateau (in all of the studies cited above). However, it is indeed less than 30% almost year-round in the area where Dome C sits. You may then want to rephrase a little bit the sentence here.

L63: “…near the coast (Listowski et al. 2019)” Their paper demonstrate this using satellite observations.

L65-66 – The whole Antarctic region, not only the continent I think.

L61 – “Some measurements exist”: Yes, and papers that are lacking from the current bibliography investigated the microphysical properties and provided new constraints to modelling. They should be cited here. These are papers dealing with the analysis of airborne measurements:

Grosvenor, D. P., Choularton, T. W., Lachlan-Cope, T., Gallagher, M. W., Crosier, J., Bower, K. N., Ladkin, R. S., and Dorsey, J. R.: In-situ aircraft observations of ice concentrations within clouds over the Antarctic Peninsula and Larsen Ice Shelf, Atmos. Chem. Phys., 12, 11275–11294, https://doi.org/10.5194/acp-12-11275- 2012, 2012.

Lachlan-Cope, T., Listowski, C., and O’Shea, S.: The mi- crophysics of clouds over the Antarctic Peninsula – Part 1: Observations, Atmos. Chem. Phys., 16, 15605–15617, https://doi.org/10.5194/acp-16-15605-2016, 2016.

O’Shea, S. J., Choularton, T. W., Flynn, M., Bower, K. N., Gallagher, M., Crosier, J., Williams, P., Crawford, I., Flem- ing, Z. L., Listowski, C., Kirchgaessner, A., Ladkin, R. S., and Lachlan-Cope, T.: In situ measurements of cloud mi- crophysics and aerosol over coastal Antarctica during the MAC campaign, Atmos. Chem. Phys., 17, 13049–13070, https://doi.org/10.5194/acp-17-13049-2017, 2017.

Also note that Grazioli et al. (2017) observed microphysical properties and shapes of precipitating crystals at DDU (aggregates, rimed particles etc):

Grazioli, J., Madeleine, J.-B., Gallée, H., Forbes, R. M., Gen- thon, C., Krinner, G., and Berne, A.: Katabatic winds diminish precipitation contribution to the Antarctic ice mass balance, P. Natl. Acad. Sci. USA, 114, 1858–10863, https://doi.org/10.1073/pnas.1707633114, 2017b.

L74 – It is also or rather King et al. 2015 that should be cited here, where the authors show the large radiative biases in three high-resolution models and hypothesize the link with the lack of simulated SLW by showing the little liquid amounts formed by these models.

King, J. C., Gadian, A., Kirchgaessner, A., Kuipers Munneke, P., Lachlan-Cope, T. A., Orr, A., Reijmer, C., Broeke, M. R., van Wessem, J. M., and Weeks, M.: Validation of the summertime surface energy budget of Larsen C Ice Shelf (Antarctica) as represented in three high-resolution atmo- spheric models, J. Geophys. Res.-Atmos., 120, 1335–1347, https://doi.org/10.1002/2014JD022604, 2015.

A recent study that should appear in the introduction used the above-mentioned aircraft measurements to specifically show the link between poor/better SLW modelling and poor/better radiation modelling at the surface:

Listowski, C. and Lachlan-Cope, T.: The microphysics of clouds over the Antarctic Peninsula – Part 2: modelling aspects within Polar WRF, Atmos. Chem. Phys., 17, 10195–10221, https://doi.org/10.5194/acp-17-10195-2017, 2017.

These studies above, that deal more with a coastal environment stress even more the importance of the findings of the present paper, which address the continental environment, which has been less investigated so far with respect to links between SLW modelling and radiation biases.

You should also say here that Lawson and Gettelman (2014) conducted a study of SLW observations on the Plateau at South Pole with a MPL. However, if they did look at the radiation changes at the surface by changing some model parameters to simulate more SLW in their model, they did not analyse simultaneous radiation measurement I think (please double-check). You are doing this and this is a big plus of your study compared to theirs in terms of ground-truth radiation budget investigation (and you could emphasise this in your introduction).

L75 – Rather say for instance: "…impacting the radiative budget of the Antarctic and beyond" or something like this, since the Antarctic (including the SO) is the region mainly investigated by Lawson and Gettelman (2014) in their modelling experiments.

L84 – Is there any document or reference describing this project, which could be cited here? Or is the present paper aimed at being the first reference of this project?

L115 – "The method employed and data sets used in our study are…" (see below comment on L119)

L117 – As suggested in my main comment, there should be a discussion section gathering information from 3 and 4 and coming before section 5 (conclusion).

2.Datasets

L119 – I recommend section 2 explaining not only the dataset used but also the method, i.e. the fact of choosing two scenarios of PBL regimes, and state how representative these two scenarios are for instance (perhaps citing previous work like Ricaud et al. 2012, etc.), with a presentation of both larger synoptic scale contexts. The authors could think of one figure demonstrating the clear difference between the two cases by overplotting temperature e.g. at two different altitides (surface and 500m?) and IWV for both cases. This would better introduce the results for the two case studies. Ideally this would have been event better to compare both atmospheric conditions with the corresponding average+/-std of the whole summer campaign (I am thinking of the representativeness of the two case studies.)

L137 – Does this wet bias takes into account any possible dry bias of the sondes?

L138 – Do you mean studied or validated?

L178 – Please say here which version of the CALIOP product you are using.

L188-189 – what is the vertical resolution of the model configuration, at least in the PBL? 7.5 km stands for the horizontal resolution.

L190 – Since you show cloud fraction in some figures, please recall here how this cloud fraction is defined? What do the values shown in Figure 2a exactly mean?

3.Typical diurnal cycle of the PBL

L198 – Here you could build on the Method already given in section 2 as recommended in my comment of Line 119, instead of just saying a "typical" PBL cycle, which is not necessary transparent to a reader non familiar with Antarctica.

L203 – "LIDAR cloud backscatter" is redundant with saying that it "indicates that clouds,.." are present. Just say: LIDAR backscatter, and use beta. No need for beta_c here, I think, as long as you refer to “high values” of beta (clearly defined as >100*beta_mol)).

L211-212 – I recommend rather saying a “SLW layer” because what we would call “cloud” here would rather appear to be the combination of the SLW layer at the top and ice below (and probably in) the SLW layer (hence a mixed-phase cloud). Please change here and everywhere in the text, where relevant.

L214 – See my comment of Figure 2 (end of this review) for the choice of colour, which is not the best here I think.

L218 – I would say: the cloud is mainly confined

L225-227 – The modelled SLW shows very low mmr: 5 10-9 kg/kg = 5 10-6 g/kg ~ 4 10-6 g/m3. Compare this to typical values to Antarctic/Arctic stratus (on coastal areas) of about 0.1 g/m3. (cf. O’Shea et al. 2017, Lachlan-Cope et al. 2016 / Young et al. 2016).

First two were cited above, the third one (as an example) :

Young, G., Jones, H. M., Choularton, T. W., Crosier, J., Bower, K. N., Gallagher, M. W., Davies, R. S., Renfrew, I. A., Elvidge, A. D., Darbyshire, E., Marenco, F., Brown, P. R. A., Ricketts, H. M. A., Connolly, P. J., Lloyd, G., Williams, P. I., Allan, J. D., Taylor, J. W., Liu, D., and Flynn, M. J.: Observed microphysical changes in Arctic mixed-phase clouds when transitioning from sea ice to open ocean, Atmos. Chem. Phys., 16, 13945–13967, https://doi.org/10.5194/acp-16-13945-2016, 2016.

When saying “ presence of SLW cloud almost all day long in ARPEGE-SH compared to SLW clouds from 08:00 to 22:00 UTC in the observations” you are not comparing very similar things. The “all day long” SLW layer in the model has very low concentrations, that would – if real at all – be missed by the lidar, especially if it forms above the precipitating ice detected during the first half of the day by the instrument. These very low values should probably be mentioned before comparing things.

Since you speak of the SLW in this paragraph you should also point to the LWP comparison between HAMSTRAD and ARPEGE (Fig. 3b) to give a more quantitative estimate of the difference between both. The sentences of the next paragraph (L228-241) speaking of LWP should be moved to here I think and the next paragraph would only focus on ice and water vapour.

About Fig3b – Interestingly the lidar detects some SLW at 9UTC while HAMSTRAD LWP increases only very slightly but was already non-null before. Why is that? Could it be that at earlier times the lidar is not seeing SLW because of the obscuration by the ice below it (Fig1a)? HAMSTRAD sees non-null LWP values. Are these real? If they are, then you could say that the model is right in continuously simulating SLW (although very small amounts) after all, since HAMSTRAD does continuously detect non-null LWP (as opposed to the LIDAR).

L240-241 – I am not sure why this is recalled here since this was already said before (see my previous comment to line 137).

L242-247 – I would rather say that CALIOP complements (and not validate) the ground observation since it observes from the layer top downwards, and the ground-based LIDAR from the bottom upwards. Also note that both will not have the same field of view at all so that features seen by the ground based lidar could be missed by CALIOP. The ground lidar will be more prone to detect finer structures and ice below the SLW (since CALIOP signal will get extinguished by the SLW layer). Besides, in the VFM of CALIOP note that SLW is spotted by the space lidar but no ice is detected, while the LIDAR does detect some (Fig. 1a shows that there is ice below the SLW layer). This is most probably due to CALIOP signal getting extinguished because of the presence of SLW. This should probably be commented on in the paper at some point, to help the reader understand the observations.

In Fig4a – Note that the feature detected by CALIOP seems almost to lie on the surface (I am not sure what the green colour is in Fig4a – see my comment about Figure 4 regarding this matter). What is the measured height of this feature compared to the surface? How can we say this is not an artefact? Can SLW missdetections happen very close to the surface within the CALIOP product that is used here?

L251 – How close? How does this distance compare to the typical dimension of the SLW layer in the other direction (the 280km of horizontal extent you mention later). We are not necessarily observing the same layer here, after all.

L261 – same remark about the distance

L264 – I would suggest a title saying "vertical profiles of temperature and water vapour" since you already speak of water vapour in the previous section. The title, for now, suggests that water vapour was not mentioned before.

L291-293 – This explanation about how the PBL is derived should come in section 2 in the subsection dealing with ARPEGE. Actually, the PBL height is already superimposed in all Figures 1a-c and 2a-c without explaining how it was derived.

L295 – Shouldn't you cite King et al. 2006 here?

L300-302 – I am not sure to understand this. What is exactly meant by "elsewhere in the surrounding environment"? Plus, on the figures, the SLW layer after 12UTC seems to remain in a colder (not "warmer") environment (Fig5a and 5b). Then, the model suggests a dryer (not wetter) environment (Fig 5c) and the observation a wetter environment (Fig 5d). I might be missing something here.

L307 – Please define residual mixed layer.

L315-316 – The SLW layer is just below, and not coinciding with, the local max. of dtheta/dz. As it is a bit difficult to see this local maximum on the Figure 6, can you give its height and value in the text?

L317 – I would plot the RS for the potential temp. gradient starting at 100m above the surface, to avoid the unnecessary features/artefacts at the bottom of the red curves.

L323-324 – Since ARPEGE cannot reproduce the fine vertical structure of the theta gradient I would say this as such, instead of saying "broadly consistent", because it seems that this fine structure may in the end be one reason for the wrong simulation of SLW. To me, just saying “broadly consistent” suggests that this is ok and we don’t need to further pay attention to this.

L327 – You speak about colocation. Can you add on Figure 7 a horizontal line giving the height of the SLW layer as obtained from the LIDAR? This would help locate this layer vs. the altitude of the dtheta/dz maximum.

Also I would rather speak of a colocation of the positive dtheta/dz “with the SLW layer”, not “with the height of the SLW layer”.

However, here, you speak about the colocation of the positive dtheta/dz with the layer while, before, you were rather speaking of the colocation of the maximum of the dtheta/dz with the layer. Do you mean to say both or just one of both? Please remain consistent within this subsection.

L332-333 – Can you recall for the reader the definitions of these zones by e.g. describing a bit more your Figure 8, rather than just referring to previous papers?

L336-348 – In this paragraph you speak about observations over the entire YOPP campaign while it was only about a specific case study so far. This is confusing. Please remove this paragraph. It rather belongs to the discussion section, like the one I am recommending to add in my main comment at the beginning of this review.

L350 - Section 3.4 – I recommend presenting the second case study of SLW layer here. After, you could have a subsection dedicating to surface radiations for both case studies at the same time. It is better not to separate both SLW/PBL case studies so much so that the reader can compare them easily. Surface radiation considerations can very well be moved to a common part, later in the paper and it would be better to show Figures 9 and 16 at the same time, so that – again – both cases can be paralleled.

L353: Figure9 (top)

L365-368. “As the SLW… over Antarctica”. This sentence would better go in the discussion section that I am recommending to add. Focus on the case study here. Also, you don’t necessarily know whether what CALIOP sees is exactly the cloud you see from the ground. Plus, note that it is 280 km along the satellite track and you don’t know about the cloud cover in the perpendicular direction (unless the second orbit you are not showing gives info about cloud cover size along a different direction?).

L370: you only mention the increase in downward LW radiation. In theory you should also detect a decrease in SW because small droplets are very efficient in reflecting sunlight. And, actually, you do see this in your plot. Around 12UTC you see a reduction in down/upward surface SW, because of the SLW layer reflecting sunlight, hence reducing the upward SW (reflection by the icy or snowy surface) as well. Please refer to this as this satisfyingly shows the opposite effects of the SLW layer in both parts of the spectrum. (see my similar comment of L475-476, for Figure 16)

L373: What is meant by “at a level higher”?

--

4.Perturbed diurnal cycle of the PBL

L377-380: this info could go in the “method” subsection I was recommending to add, to introduce at once both PBL cases investigated here (and possibly say something about their representativeness).

L392-393 – I am not sure to understand why it is said that the LIDAR does not detect a mixed-phase as well. Your LIDAR mask indicates SLW but this does not mean there is no ice at the same time. Does it? Fig10a suggests ice forms/falls below SLW layer I suppose and again it could be ice precipitating from the SLW layer where little crystals would have also already formed (as it is often the case for low-level mixed-phase clouds). Unless you demonstrate this, I don’t think you can say here that your observed cloud is not a mixed-phase one.

L400: the simulated cirrus cloud is above the area where the SLW layer is. This is not just a matter of sensitivity. This is most probably because the SLW layer extinguishes the lidar signals which cannot reach the cirrus cloud. The top right part of Fig10a suggests so (no signal).

L393 – As for the previous case study, please do clearly highlight the very little amounts formed (in g/kg) clearly showing that the model forms virtually no liquid at all…

L401 - SLW layer

L429 - Please avoid the use of “model data”. You could say e.g. “the model output” or “the model simulates a moistening…”

L449 - When you say “This is broadly…” it is not clear what “This” is referring to since you then speak about the events “prior the warm episode”, while “This” seems to refer to all three profiles. Please rewrite. I am not comfortable with this “broadly consistent” expression (see my previous comment on L323-324). Obs-Model differences in Fig15b and c appear even more larger here than in the previous case study.

L458 - Figure 16 (top)

L472 - the maximum of LWP appears rather to be 45 g m-2 at 13h00 UTC. However, it seems that you smoothed the data here – when comparing with Fig.12b where we see the maximum is 50 g m-2 indeed. Please make Figure/text consistent with each other.

L475-476 - As for the previous case-study you can note the decrease in SW up/down because of the reflection of sunlight by the SLW layer. Compared to previous case, however, SW up/down is continuously decreasing. Why is that? It seems, according to the pictures and the LIDAR detection that the cloud deck is thickening (hence the largest LWP values) and the cloud base lowering, preventing always more radiations to reach the ground, while for the previous case study, the cloud seemed more broken (see your pictures) and the cloud base altitude constant (see the LIDAR detections).

These types of observations should be commented on, here or in the discussion section. Please make the most of the combination of radiation measurements, cloud measurements, and visual observations. Again, rearranging the paper so that Figure 9 and 16 are in a same unique section about radiation would help.

Also, you don't comment on the fact the water vapour bias is clearly appearing in this case study so that you could expect also bias in LW radiations from water vapour since it is a strong GHG. Perhaps this can explain the larger biases observed in the second case study (in addition to the thicker SLW layer observed). What do you think? These are matters to discuss in a discussion part…

5.Conclusions

L496 - you have not mentioned any CALIPSO overpass for the 20th of December so far, only for the 24th. Both overpasses you were mentioning (although you only showed one) were for the 24th.

L498: underestimated – say by how much.

L505 - BSRN LW or net values?

Figures.

Figure 1 – one cannot see the red text, especially on the coloured background. Please adapt the color, and put the text a bit higher. For Figure 1c, use the blue colour for the "SLW layer".

Figure 2 – Please use a different color for the text in the plots. Also, this is confusing to have "liquid water" written in red, with that colour being used for the lidar observation as well (while also being part of the colourscale…) Could you perhaps use grey colour to indicate the SLW layer observation? Also: using white colour instead of black-red would help seeing the curve for PBL height more clearly.

Figure 4 – Can you put the names of the categories on the colourbars instead of numbers that are not explained in the Figure caption? If this Figure is a quicklook obtained from another source, this should probably be said.

Figure 5 - Please use a color other than red for text.

Figure 6 - Please use other color for observed SLW as it is the same red as in the colourscale.

Figure 9 – "simulated with" rather than "calculated by"?

Figure 11 – It might be better to use white colour for the PBL height.

Figure 13 - red text is not visible. Also, the red colour chosen to show the presence of the SLW observed by the LIDAR is the same red as the colour scale. Please change this. This was not so problematic in Figure 5 but it is here.

Figure 14 - using red colour for SLW and the same red for the colourscale shoud be avoided.