

Author's response to two anonymous reviews for ACP-2020-584

We thank the two anonymous referees for their constructive feedback, which significantly improved the quality of the manuscript. We are well aware of how much work such a report requires. Due to the extensive modifications of the manuscript, we compose a combined author's response for both reviews. First, we summarize general revisions of the manuscript. Second, we refer to the remarks of each reviewer individually. The original reviewer comments are marked in blue color.

General remarks

- The structure of the manuscript has been completely revised.
- A new section about technical aspects of humidity measurements under cold and cloudy conditions has been implemented. We want to make sure that the observed humidity inversions are real and not a measurement artifact. The main reason for this was the observation of systematic humidity differences when comparing ascents and subsequent descents. We discuss error sources for the RH measurements and improve the measurements with a revised time-response correction based on further laboratory investigations.
- Due to the improved correction of humidity observations, data observed during descents are now more consistent and therefore included in the data analysis. For the descents we observed an interesting phenomenon: During all flights, the cloud base descended between ascent and descent, but in a different way. This behavior was confirmed by remote sensing. Due to the increased number of profiles with different relative locations of temperature inversion, SHI and cloud top, further scientific questions could be analyzed.
- We revised the analysis of turbulent fluxes, see specific comments below.
- We tried to focus on the novelty of our measurements rather than on uncertainties, as suggested by reviewer #1.
- Both reviewers criticize that it is hard to generalize from case studies. In our study, we document and analyze the observed cases and agree that the results should not be generalized. Further observations over a larger measurement period are needed for a more general conclusion, as stated in our summary.
- Reviewer # 1 suggested shifting the LES to a separate paper, reviewer # 2 appreciated the combination of observations and LES. We decided on a compromise and now discuss one LES case to show the impact of the SHI on the cloud, leaving potential for a more detailed separate study. See also the answer to the specific comment of reviewer # 1 below.

Remarks to comments of referee # 1

- Excessively long and tedious title.

We agree and change the title to “Case study of a humidity layer above Arctic stratocumulus and potential turbulent coupling with the cloud top”.

- Coherent narrative instead of chronological description

We completely restructured the manuscript. We are confident that this revised version is much more narrative.

- There is no clear hypothesis to test.

We agree with this point and in the revised version, the main scientific question is raised in the introduction. We don't word it as a hypothesis, but we think this is a question of style.

- The text even starts with questioning the very existence of moisture inversions, which is off course fine! However, RH for the descending branch from BELUGA is not consistent with the suggested cloud outline; in the upper 50% of the cloud layer, $RH < 80\%$.

We completely agree with this point, the interpretation of this profile was misleading and not convincing. In the case shown in the first version of the manuscript (old Fig. 1), the cloud extent is estimated from Cloudnet data only for the ascent. For the revised version, we use radar reflectivity raw data with a much higher temporal resolution of 3 s (30 m in vertical) (see new Fig. 2). Here it becomes clear that the balloon descended into a much lower cloud top which partly explains the low humidity in that region. However, there is a general difference in measured RH around cloud top observed during ascents and descents, which motivated us to look deeper into the data resulting in the additional chapter about humidity measurements and an improved correction algorithm.

For the new technical section about the humidity measurements, we decided to use a different day (5 June, second profile) with a constant cloud top height to clearly show the efficiency of the new corrections.

- Remote sensing retrieval software is wonderful and multi-sensor retrievals, like Cloudnet, has many useful features. This is, however, only true when used carefully and from an understanding of limitations and applicability. Here the authors are using Cloudnet retrievals like a very black box and it doesn't help much.

To a large extent, we agree with the reviewer. As a consequence, we had many discussions with our in-house experts for remote sensing observations about this topic. We agreed on using the original cloud radar data with a 30 m vertical resolution to get the most accurate estimate of cloud top development (new Fig. 1 and 2). We considered also including a comparison of remote sensing turbulence observations with BELUGA in-situ

measurements, but finally, we decided that such an analysis - although very interesting - is a different topic which we will consider in a separate manuscript. Turbulence estimated from remote-sensing is only available for in-cloud regions and we focus on the region between cloud-top and the SHI above so remote-sensing does not help very much in this context.

- So instead they bring in LES, which is perhaps an even larger black box but also doesn't help much; what is needed here is some careful thinking, experience and a new analysis strategy. The LES discussion is quite short, and I don't understand why one case is relegated to an Appendix while the other isn't, and it doesn't help at all. I would suggest to expand the LES study and make it a separate paper; base it on this study, by all means, but do the proper set of simulations to figure out the optimal configuration and then do all the different sensitivity simulations you need to extend and generalize whatever it is you find in the analysis of the observations. There are so many ways an LES can be useful, but the way it is used here is not one of them. Multiple initial and boundary condition combinations can bring a simulation to appear similar to a single case-study profile, but there is only one that is correct and it is not always evident which one; most appear correct for the wrong reasons. There is much else to be said about this but most importantly, you should never use an LES to lend credibility to observations; it should be the other way around!

We appreciate the suggestion to publish the LES study in a separate paper and will keep this option for a more detailed study on how the additional humidity is processed in the cloud layer. However, here we suggest keeping the LES discussion but shift the focus: We don't use the LES as a validation for the measurements. Instead, we focus on one LES case in order to compare with observations to show how the SHI might influence the cloudy ABL. The second LES case, as shown in the appendix, has been removed to the revised version. The technical details about the LES setup are now shifted to the appendix not to distract the reader from the main point.

- [...] the trajectory calculations looks intriguing, but the discussion doesn't seem to go anywhere; you need to do more to be convincing, or should just drop this line of inquiry.

We agree and omit the trajectory discussion at this point. It might help to explain the source for the humidity layers but this is not the focus of our paper.

- What might help is to explore alternative analysis methods and/or looking at more sources of concurrent observations. I suggest looking more at the remote sensing data independently. For example, directly explore the Doppler data from the cloud radar. There are methods described in the literature how to estimate some turbulence statistics directly from the radar data (e.g. σ_w and ϵ). An upside to this is that you can find levels where the data comes from a constant altitude for well understood portion of time; flipside are the lack of resolution and that only one parameter can be derived. But do look at the native time resolution; not the Cloudnet-filtered data.

To broaden our expertise in remote sensing data analysis, we invited Hannes Griesche as co-author. He analyzes the PASCAL remote sensing observations. We considered including turbulence parameters, such as variance and ϵ derived from the cloud radar data, which gave some interesting insight into cloud dynamics (see Fig. 1 of this document). However, we realized that in-cloud turbulence does not really help to understand the coupling between the SHI and the cloud-top region due to lacking data above the cloud top. Therefore, we decided not to use this kind of analysis in this manuscript. However, these discussions about using radar data more directly helped a lot in improving the cloud top estimates from radar - see new Fig. 2.

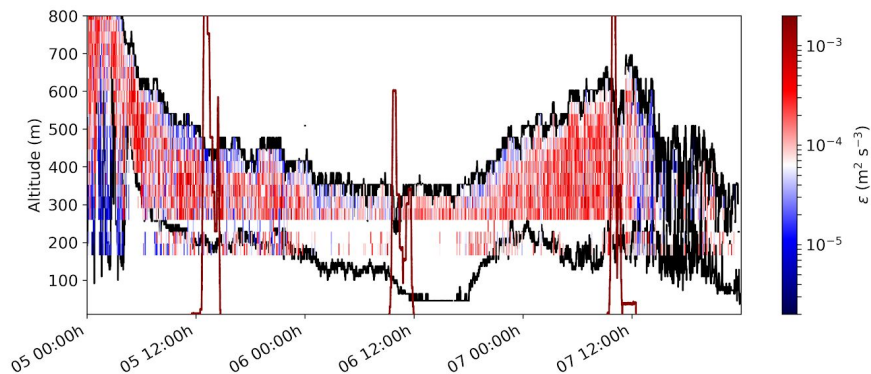


Fig. 1: Eddy dissipation rate estimated from cloud radar data for the observation period.

- Flux discussion:

- I fail to understand why knowing the size of that flux – from one case – is so important.

We agree with the concerns about the absolute number of a single flux estimate. The discussion around these numbers and uncertainties was misleading and went in the wrong direction. We now focus more on the general shapes of the vertical profile rather than on absolute magnitudes of fluxes.

- I would also not walk away from the slant profiles just yet, although they take really careful hands-on analysis. There are several old papers where slant profiles by aircraft have been used to tease out profiles of turbulence statistics with realistic magnitudes and shapes. It does require careful filtering, however, I submit that the vertical velocity of the platform should make aircraft profiles harder to work with than the BELUGA data.

We stick to the slant profiles, but with slightly changed filter settings.

Following arguments by Tjernström (1993) and Lenschow (1988), we set the filter window to 10 s to define the fluctuations from which the local flux is calculated. With this smaller filter window (compared to the ABL-dependent filter window of 50-100 s as applied before), we resolve the smaller structures around the SHI. The flux is then averaged over running 50 s windows on the slant profile.

- However, I would, in contrast, advise against filtering data from constant height flight legs. A numerical filter can never provide a signal with a power spectrum looking anywhere near realistic. So just give it up and use Ogive analysis instead, to analyze the magnitudes of fluxes and variances.

Ogives are definitely an interesting tool to analyze fluxes. However, as mentioned in previous points, we focus now more on the general vertical structure of the flux profiles instead of estimating fluxes from constant level records.

- A word of warning, however; if the signal looks like in Figure 7a, no filtering in the world will help. The interface between the cloud and the inversion layer is like the surface of a lake and what you see here is the effect of the sensor sometimes being under and sometimes above the “surface”. The resulting signal is from two different environments and filtering the signal to make it look smoother will not make those environments the same or even similar; averaging statistics for turbulence over the resulting signal is therefore meaningless, and you need to do something else.

The reviewer is absolutely right, and we agree that the way the mean fluxes are estimated by filtering records as shown in Fig. 7a is fundamentally wrong. Following the argument that a single value of the flux is not meaningful in this context, we have not included another analysis technique such as Ogive analysis (although we have tried this technique). But we are convinced that a figure like the old Fig. 7 (new Fig. 13) - especially because of the remarkably constant measuring height - can give a valuable impression of the situation around the inversion, and therefore we discuss the observations based on time series. We agree that the reason for a varying z_i is less important here and therefore we will refrain from a corresponding discussion at this place.

- I see no reason to expect the turbulent flux here to be in any other direction than that dictated by the gradient; counter-gradient fluxes appear in deep convective boundary layers, and this is essentially either a near neutral layer close to the upper boundary, in the cloud layer, or a stably stratified environment, in the inversion. So using the flux-gradient approach makes a lot of sense, however, I don't understand the efforts to use parameterizations of the eddy-exchange coefficient, K_q , based on filtered higher-order moments. Why not get it directly from the sensible heat flux and the temperature gradient? If you anyway assume that $K_q = K_H$, this should give you what you want. With the method you use, you can both measure (by eddy-covariance) and calculate (with the flux-gradient method) the sensible heat flux; if the two are different, then you can't trust the parameterized moisture flux either. However, I would say that if the gradient is positive and the flow is turbulent, there's no question in my mind the flux is negative (downward); it just stands to reason, with what we know about turbulent flows. How large it is, is a different question; one that we likely cannot get a useful answer to from one case.

We adopt the reviewer's suggestion and calculate now K_H from the slant profile measurements. We had some internal discussion if it is worth to calculate $K = K(z)$ or to estimate a single K for the region of main interest. With a constant K , we definitively underestimate the flux in the more turbulent cloud layer, but in that region we would have to apply some careful averaging to smooth the local gradients avoiding too much scatter for the K values. We, therefore, decided to estimate K just around the base of the SHI and use this value for the entire profile. These K values differ only slightly among the different days, which gives us some confidence that the method is robust.

- Finally, many are the papers that have tried to explain peculiarities in the results with gravity waves; There are, however, methods to show if what you see are indeed buoyancy waves and not just something that happens to look wavy. So – either show up or let up; either you provide some evidence that there are gravity waves present or drop that line of hand-waiving arguments all together.

We deleted the discussion of possible gravity waves and instead followed the reviewer's argumentation that z_i moves up and down around the instrument, producing those temperature variations. We agree that for our manuscript the exact reason for the variability of z_i is of less importance.

Remarks to comments of referee # 2

- 1) I appreciate the discussion regarding the potential biasing of humidity inversions due to sensor wetting during the ascent through a cloud layer; this has been a caveat or concern in the community for some time, considering many of our climatological frequencies of SHI occurrences have been derived from radiosoundings from field campaigns. It is great to see the ascent/decent profiles of humidity from the BELUGA system do in fact show similar thermodynamic structures to the radio soundings. Have any additional tests been made to attempt to isolate cases where the radiosounding-derived SHIs are potentially biased by sensor wetting, in which case these profiles could be removed from the analysis? I wonder if it would be helpful to broadly estimate the adiabatic liquid water content of the cloud layer from the thermodynamic profiles, and make a comparison with the absolute increase in specific humidity within the SHI (i.e., sensor wetting should likely not exceed the maximum LWC value in the profile). Surely the amount of sensor wetting must be limited by the maximum amount of cloud liquid water content(?).

We think that the sensor wetting can exceed the maximum LWC in the cloud, as liquid water can accumulate on the sensors. Hence, it is difficult to quantify wet-bulbing, as we don't have an indicator for the extent of wetting. We could not identify wet-bulbing events in the radiosoundings we analyzed. Instead, one case in our BELUGA measurements, where wet-bulbing probably occurred, is the 12 June (see Fig. 2 of this document, not included in the manuscript), where RH increased by almost 10% at cloud top. On this day, we observed wet sensors when they returned to the ground. However, this RH increase causes only a small increase in q as part of the actual SHI.

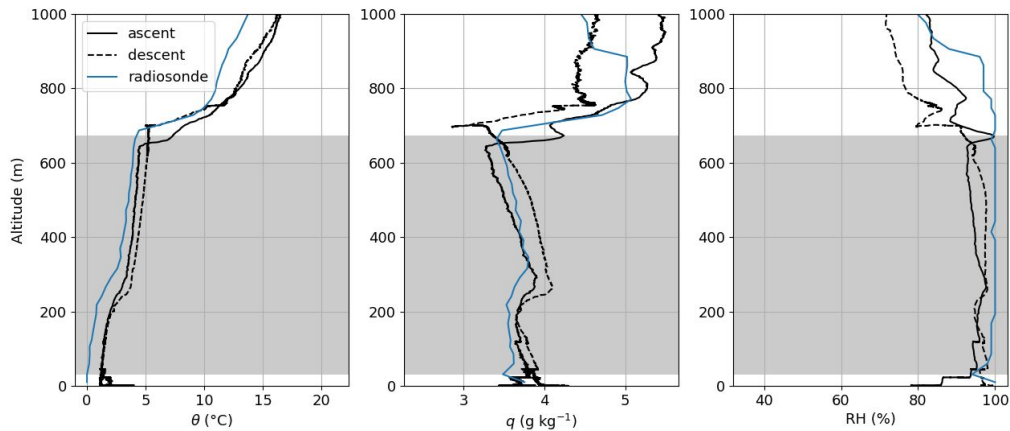


Fig. 2: Vertical profiles for 12 June 2017 (not part of the manuscript).

- 2) The analysis and conclusions derived in this study come from really only 2 profile cases. And even these 2 case have substantial variability in the physical properties of the inversion structures, the flux magnitude estimates, and the turbulence characteristics. I am missing an attempt by the authors to characterize or relate the flux estimates(negative) to the properties of the temperature and humidity inversion layers. How might the displacement depth between SHI base/max and level of largest infrared divergence (cooling) affect the results? I would like to see some more of this substance in the discussion Section 5.

We address this comment in the new sections 4 and 5 by discussing the descents, where the SHI relates differently to temperature inversion height and cloud top,. However, we cannot relate a flux magnitude to the SHI properties, as we focus on the vertical structure of fluxes rather than a number for cloud-top fluxes.

- Line 26: See/include reference to Devasthale et al. (2011, ACP: “Characteristics of water-vapor inversions observed over the Arctic by Atmospheric Infrared Sounder(AIRS) and radiosondes”)

Thank you for the reference to this paper about SHIs from radiosondes and satellite data under clear-sky conditions. We inserted the reference in the introduction.

- Line 52. The section heading “Observational” is an adjective, and therefore requires a noun to follow. Please adjust accordingly.

We changed the heading to “Observations”.

- Line 95. It seems to me, from Fig. 2, that the other two balloon flights during the 5-7th June also correspond with the 12 UTC sounding time and have a continuous ascent and descent profile. The authors should explain, or show, why the results from this soundings and balloon profiles are not shown or described in the text. Do the profile comparisons not look as convincing as in Fig. 1?

We included the new Fig. 2 to show the single flight profiles more in detail and with regard to the cloud. The first flights of 6 and 7 June have constant height steps on the descent. For a comparison between ascent and descent, we now show the second, smaller but continuous profile of 5 June with a constant cloud top height. However, we also included the radiosoundings in the vertical profiles of mean parameters for each day.

- Line 100. It would be helpful to include the cloud boundaries from Cloudnet at the time of the balloon decent as well. This may help to explain the discrepancy between RH and cloud boundaries.

We now discuss the cloud tops (based on irradiance data) on the ascents and descents for all flights in detail in the new Sect. 4. To discuss the humidity measurements (with a comparison of ascent and descent), we now show another day (5 June) with constant cloud top height.

- Line 114-115: I am confused. I thought the Cloudnet retrievals included ceilometer base heights, MWR liquid water path estimates, and thermodynamic profiles from soundings to retrieve cloud boundaries?

In the first manuscript version, we showed the cloud base from a separate Ceilometer, which was part of the Polarstern standard meteorological observations. We now use the cloud base data derived from the lidar PollyXT near-field channel, which is part of the Cloudnet sensor suite. The lidar has a resolution of 7.5 m and 30 s. Using native lidar data, not processed with the Cloudnet algorithm, allows detecting cloud base heights below the lowest Cloudnet range gate of 155 m, which is determined by the cloud radar.

- Line 124-125: It would be helpful to include the cloud base and top heights (as colored symbols) on the normalized profiles, in order to show whether (and how deep) the cloud top extended into the temperature and humidity inversion structures.

We included a separate panel to show the cloud top height (derived from irradiance profiles). We observe almost no cloud tops extending into the inversion.

- Line 145-146: Note additional studies as references: Sedlar et al. (2012, JCLIM); Shupe et al. (2013, ACP); Sedlar and Shupe (2014, ACP); Brooks et al. (2017, JGR).

We included the suggested references. However, we did not observe that the cloud tops penetrated into the SHIs, as discussed in those studies.

- Line 157: Between which depths in the layer are the Ri number calculated?

This question is answered by the new columns in Fig. 8-10, showing the vertical profile of the Richardson number.