Author‘s response to one anonymous review for ACP-2020-584, second revision

Reviewer # 1

We would like to thank the reviewer once again for his/ her helpful comments, which have significantly improved the quality of the revised version. We hope that we were able to address all critical points in the manuscript. Below you will find our responses to the individual points (the reviewer's comments are highlighted in blue).

Three overriding concerns:

- The main message: When reading the manuscript, the narrative shifts from the effects of SHI, to questioning the very existence of SHI and then back to the effects again. The introduction covers the SHI in previous studies and sets the stage for the analysis. Then with the long section with the discussion of potential measurement errors the very existence of SHI is called into question and it is concluded they do exist. This is a relief given the intro; there wouldn’t be much of a paper if the conclusion had been the opposite. However, the discussion of potential errors is then mostly forgotten in the rest of the text, which reverts back to accepting their existence; it is not even mentioned in the concluding section. This makes the section with the error analysis (Section 3) seem like a long and embedded appendix; it doesn’t fit well into the rest of the text. There’s nothing fundamentally wrong with the text seen section by section; I’m just sensitive to the narrative when looking at all the sections as part of one paper.

  Thank you very much for bringing this point to our attention. We agree with this concern and in the new version, we tried to better integrate section 3 into the scope of the manuscript by mentioning the issue of possible measurement errors already in the introduction and incorporating the technical part also in the summary and abstract. We also swapped sections 2.2 (BELUGA) and 2.3 (Observation period) to bring the technical sections closer together.

- The error analysis (Section 3): This is worthwhile but very detailed description also with some rather trivial content. Does it have to be this long(?); it’s a third of the main text. With this degree of detail, it could have been published separately as a technical note. The authors list three potential errors. The solar heating is not considered further, leaving the wet-bulb:ing and time constant. Then, if I read this correctly, the wet-bulbing seems to be folded into a time-constant problem; then at the end it is not again (see final sentence). But viewing wetting this way makes things so much more complicated; it means that the system has two time constants; one related to the time it takes for the wetting to evaporate and one relating to the time constant of the instrument. In fact, there may even be three time constants; these two plus one related to the instrument housing. I recommend that either Section 3 is revisited and rewritten so that it fits the purpose of this paper, or that it is extracted from this paper and published separately. If it is kept as a part of this manuscript, the results should be reflected in the following text and especially in the summary section.

Thank you again for this point. We agree that section 3 is quite long compared to the more science-related sections. However, we think that the topic should be presented together with this study and would like to keep the content, but modify it. We shortened the section and left out some less important content, such as the figure showing the influence of the time constants on *q*. We shifted the subsection about determining the time constants of our humidity sensor in the appendix, as it is additional information and no prerequisite for the rest of the paper. The aim of this section is to show what can distort the SHI observations and to make sure that the SHI is not the result of one of these errors. We show that we can quantify and eliminate the time lag errors and minimize the influence of solar heating by using measurements inside the same sensor housing. We cannot quantify wet-bulbing and rule out that this effect exists in our observations, but we can exclude it as the main reason for the observed SHIs because the SHIs are seen also during the descents. This is an advantage of BELUGA measurements compared to radiosoundings. We try to better transport this message in the revised text.

- The LES study: I indicated previously that I didn't think the LES study fitted in this manuscript. The way the revised paper comes through, I take that back. While I do feel that one can always get an LES to agree acceptably with observations if one tries hard enough (there are so many degrees of freedom to play with and usually not enough observations to constrain) the trick is to use the LES for something valuable and with the with and without SHI comparison I feel the authors succeed with only these two runs. They may both be way off to reality in some aspect, but they are then off the same way! But more work is still needed to make it fit in the paper. The text introduces just about enough information about the LES to irritates me to have to go to the Appendix to get the rest. I get to know the size of the domain, but not the resolution; for that I need to go to the Appendix. I can't find information on how the LES is initialized and get no useful information on what the authors mean by "Lagrangian"; yet this particular information is repeated in both the text and the Appendix. The Appendix say the LES is "constrained by the soundings", but those were done at the location of Polarstern which is the trajectory end point. So how is that compatible with a Lagrangian perspective? In short, the text describing the LES and the experimental setup needs to be revisited; I would either put all technical information in the Appendix or expand the Section in the text to get rid of the Appendix.

We are glad that the reviewer shares our opinion that including an analysis of two targeted LES experiments, one with and one without an SHI, is meaningful and brings added value to this study. In the revision, we tried to address all of the reviewer's comments concerning the discussion of the LES. Firstly, as recommended, the technical description of the numerical experiments has now completely moved to the Appendix; the details mentioned there allow complete and independent reconstruction of the conducted experiments. This includes information about the Lagrangian setup (which is well established and has often been used in idealized LES studies of cloudy boundary layers), the model initialization, and the adjustments made to yield good agreement with the observed basic state and structure of the cloudy mixed layer. The main body of the text now only includes a general introduction to the simulations, as well as a brief motivation for their use in this study. We hope that we managed to strike the right balance in this respect and that this has helped in improving the flow of the narrative.

- The SHI gap: All the results from the LES and the observations concerning the case where the SHI is disconnected from the cloud top makes sense to me, but the whole thing begs the question: Why? With the accepted hypothesis on the existence of SHIs being related to large scale advection of a deeper and moister upstream PBL that adjusts to the shallower PBL forcing over the sea ice, I don't immediately quite see how this could happen. Where did the moisture go? In to the cloud and precipitated out, while the cloud top then proceeded to evaporate?

  We agree with the reviewer's opinion that this observation raises many questions and also leaves most of them open. In the end, the structure of the SHI remains essentially unaffected, while the cloud top temporarily - see the radar data - drops sharply, leaving the gap behind. We have tried to find some indication of this boundary layer behavior in the continuous ground data but without success. We, therefore, decided to formulate this observation as an open question for further investigation. Unfortunately, this is not satisfying for us either, but together with our radar colleagues, we will continue to pursue the question of what could be the cause for such a sudden drop of the cloud top and whether this has been observed in this form more often.

Minor comments:

- Line 10 and elsewhere: There is considerable discussion of "latent heat flux", but isn't it the turbulent flux of water vapor that is important to this paper. Not the effects on or by the heat transport (energy) but the transport of mass; water vapor. So why convert it to W m-2?

  We agree that the mass flux of water vapor is of interest. Since it is proportional to the latent heat flux (by L_v), we use the latent heat flux in W/m^2 for consistency with the other flux values.

- Line 35: Why confine it to advection from continents? It could equally well be marine air from south of the ice margin.

  Agreed, we removed "continental".

- Line 43: "Despite their importance …" implies an causality between knowledge and importance that isn't necessary there. Some of the most important issues in science have turned out to be the most difficult to solve. Take "climate sensitivity" as an example.

  That's probably true and we agree to remove the first part of the sentence.

- Line 79 and elsewhere: The sonic anemometer provides a so called "sonic temperature". This is not equivalent to the virtual temperature, although it is close enough, especially in dry environments (not necessarily low RH but low q).

Thanks for the hint, we added in the manuscript: “Especially at low specific humidity, the sonic temperature is close to the virtual temperature, which will be used in the following.”.

- Lines 79-80: Considering what comes in Section 3, this is not nearly enough discussion of these sensors. It wasn’t until the end of Section 3 I realized the housing of the T/RH sensors may be a problem.
  Lines 219-224: See above; it’s not until here that I get the information that there might be a problem with the instrument housing.

  We added some information about the RH sensor and its housing combined with internal temperature measurements in section 2.2.

- Lines 181-187: This paragraph is actually a repetition of the previous discussion. Setting the time constant of one sensor to zero, which is already done, is consistent with setting it to any other value much shorter than the other. And setting both to zero is – in a relative sense – the same as setting both to any other single value, say for example 60 s.

  The paragraph was shortened to provide only new and essential information. The figure showing the sensitivity to different time constants was removed, to omit redundant information.

- Lines 213-214: How can the warming lead to a change in RH when RH is the measured variable?

  Thanks for the comment, this was misleading. We changed the sentence to “Furthermore, the sensor underestimates RH in the cloud on the descent, which might indicate solar heating.”.

- Line 224: Confusing; reading the preceding text, I though wet-bulb:ing was THE problem, and that is what was considered above?

  We agree the formulation was not clear enough. The revised text reads: “We can exclude wet-bulbing as the main reason for the observed SHIs because the SHI is also present during the descent. The influence of solar heating and time-lag errors is minimized. Our conclusion also strengthens the confidence in SHIs as frequently observed by radiosondes.”

- Figure 7: Please display the cloud base also in Figure 7c

  Good point, the figure now shows also the cloud base.

- Line 237: I disagree; while near-neutral through the PBL, it is weakly stable through the whole layer, and there is no easily distinguishable point where this increases below the inversion base.
  Lines 236-239: Note that the scaling used here does not apply to the lower free troposphere, so there is no reason a priori that the profiles above the PBL top should be similar or comparable.

  We re-phrased the paragraph with the suggested points.

- Line 244: Disagree again; there are clear capping temperature inversions in all the profiles. Some profiles may have embedded internal structure but that is not the same as not showing a “clear temperature inversion”.

  We removed the formulation of “no clear temperature inversion”.

- Figures 8-10: Why is there sometimes such a large absolute difference between the sounding and the Beluga temperature profiles? Sometimes the sounding is several degrees colder that the tethered sounding; this presumably also affects the specific humidity profiles.

  A comparison to ground data (mast, ship) showed no systematic difference to BELUGA near-surface values. The temperature difference seems to be greater at higher altitudes. We explain the temperature difference by ABL variability. The strongest difference between radiosonde and balloon is observed on 5 June: Here some short but strong warming events occur (which are visible in mast data).

- Lines 280-281: A difference of 20 m could well be just coincidence; the cloud top is not at one fixed height but rather goes up and down following the characteristics of the up- and downward motions of the turbulent eddies.

  We added: “... 20m below z_i, which could possibly result from cloud top heterogeneity.”.

- Section 5.3: This Section doesn’t really add much information other than as a motivation for using slant profiles instead. Therefore, either move it up as Section 5.1 and use it that way. Or drop it…

  We changed the order of the sections and now use the old section 5.3 as new section 5.1 to motivate the vertical profile measurements.

- Lines 387-388: The conclusions on the distance to the PBL top rests on an assumption on a constant vertical gradient. I submit that the sections of the time series may be equally semi-constantly distant to the ABL top, but as the latter is slowly descending, the fluctuations change character from going in and out of the PBL to being entirely inside the inversion.

  We took up this suggestion and changed the text accordingly.

- Lines 413-414: The simulated dq = 0.6 g kg^-1 is a factor of two smaller than the observed ; this is hardly “close to”.

  The SHI strength was adapted to the radiosonde observation of dq = 0.9 g kg^-1, which is closer than the BELUGA observation. We changed that in the manuscript.