Review 1

We thank the reviewer again and followed the suggestion to improve Figures with respect to legends. We avoided now blocking of data by the legends.

Review 2:

We thank for the very positive comments and we are grateful again for many useful and constructive suggestions. This helped to improve the paper ones more.

All comments of the reviewer were made in the pdf of the first revised manuscript. We followed almost all suggestions and our improvement can be seen in the submitted rerevised pdf-version with marked changes. Most of the suggestions were smaller issues like improvements of language. Other hints were more related to contents and in the following we explain what we have done in the most important cases. Page numbers refer to the pdf annotated by the reviewer. Reviewer comments are in red.

Abstract:

Avoid imbalance in text volume in the abstract between the four cases

In the new version we avoided this imbalance. Please note also that the first half of the abstract is also related to all cases.

Page 2: And you add four more cases; how is that a significant change?

This has been reformulated stating that just the data base has been enlarged and the expression ‘significant’ is avoided.

Page 3: Why would sea ice make clouds appear in layers? Prototypical stratocumulus appears over oceans in subtropical high-pressure. That is not the case in the Arctic, so why should clouds not be multi-layered; they are practically every where else all the time.

Yes, we agree. This was misleading and we reformulated the text. We write now:
As stressed by Curry et al. (1988) Arctic clouds occur frequently in multiple layers. In such cases, the strongest radiative cooling as well as the buoyancy production of turbulence occurs in the uppermost cloud layer, which is far from the surface and thus decoupled.

Page 9: In one of these two profiles the layer is very shallow and the direction turns lower below the cloud base, while in the other - which actually sticks out - it is the cloud layer that is anomalous. I think this is more random variability and not coupled to decoupling.

We write now:

This might hint to a decoupling between the well-mixed cloud layer and

the surface-based boundary layer in some profiles but might also represent random variability.

Page 10: Also note the maximum in the spread of H near cloud base, possibly indicating that the heat flux was carried by eddies of varying size, some which did and some which did not penetrate below the cloud base.

Thanks for the hint. We improved the text combining the hints on this page according to the suggestions.

Page 13:You say this as if you had some evidence of this. Without showing tangible evidence of gravity waves, I would be a bit more careful and speculate rather than state this here.

We write now: But the visually observed undulating cloud top, as
well as possible gravity waves in the stably stratified inversion layer might have contributed also to an increased temperature variance near the capping inversion.

Page 13: Horizontal temperature changes within the PBL and the sloping inversion may generate changes in the wind speed due to the thermal wind law as integrated from the free troposphere and down to the surface, like is frequently happening along a coast.

We agree with the reviewer that the sloping inversion might be the reason for the low-level jet. In fact, in the first version of the manuscript we had a longer discussion on this. In order to shorten the paper and to have a better structure we deleted it. In the revised version, we shortly address this issue again. We write now: Such a low-level jet could have been produced by the sloping inversion. As shown by Chechin and Lüpkes (2017), the increase of the ABL height towards South in the presence of a capping inversion would result in an increase of the easterly wind component in the ABL.

Page 15: Is this true; looks to me sigmau is more like unity and sigmav significantly smaller, while sigmaw is smaller still; indicative of shear-production by along-wind shear then redistributed to the cross-wind and vertical components, exactly as expected with shear production.

A very helpful comment. Indeed, we overlooked that the values in the text were corresponding to the old version of the figure with nonrotated coordinate axes. We modified the text accordingly.

Page 16: Without more discussion these numbers are somewhat useless; I don't believe you more just because you quote numbers

In the revised version we explain now what the numbers (Rib and z/L) mean for our cases (here and later for the stable case, page 19)

Page 21: This is a misnomer here; there is no detection limit for heat fluch with eddy covariance in the sense that the absolute error is larger for small fluxes than for large. It can be negative, zero and positive. Do the authors suggest that a higher accuracy is needed for a neutral PBL than for any other stability? There *is* an issue of

statistical significance and bias but that would be the same as for any heat flux; no difference because it happens to be near zero here.

We write now: The uncertainty of these values is in the order of the statistical error…