REVIEW 1

We thank the reviewer for thorough reading and very valuable and constructive comments.

In the following the Reviewer's text is cited in italic fonts, our answers in roman fonts.

This paper takes advantage of the ACLOUD airborne observations, and partly also from PASCAL on RV Polarstern, to explore different Arctic cloud turbulence dynamics. It is a very useful contributions and although it doesn't really reveal anything previously unknown, it is unique in the sense that it rests on actual in-situ profiling of turbulent properties whereas most previous studies has relies on either indirect evidence, based on the vertical structure of mean parameters, or on retrievals from remote sensing, both obvious limitations. The study uses both level flight legs in a traditional sense but also analysis of slant profiles which adds substantial value and I commend this choise. This is a welcome contribution and I have no doubt that it should eventually be published, however, it still needs some more work to become acceptable; hence I recommend that it is ***accepted after a major revision****.*

Major concerns

When reading the main part of the paper, there is plenty of references to previous studies of Arctic stratocumulus, but the physics of Arctic liquid-bearing stratocumulus is no different from the physics of subtropical stratocumulus. The differences lies in the mixed-phase properties often occurring in Arctic clouds and in the surface characteristics, but much of the turbulence dynamics studied here is not sensitive to the presence of ice - neither in or below the clouds. Both cloud-top cooling and the associated buoyancy generation and cloud-forced mixing and the presence of cloud decoupling are features also present in subtropical stratocumulus.

1) It would therefore be much more useful to see the contrasting of these results to the wealth of data that exists from stratocumulus outside of the Arctic, e.g. in the subtropics. How is cloud-top cooling and mixing different here from that in the subtropical "cloud cousins" and how are the decoupling different?

Answer: We agree with the reviewer and find this point important. In the completely rewritten Discussion section we address this issue (as well as summarize it in the Conclusions). In particular, several differences from the subtropical stratocumulus are stressed. The consequences of some of the differences for the ABL turbulences are analyzed with the help of the mixed-layer model. Below are the pieces from the Discussion addressing the differences:

The first difference is related to the often observed multilayer structure of Arctic clouds:

First of all, we can confirm their conclusion that one of the differences of the Arctic stratocumulus clouds as compared to their marine counterparts in lower latitudes is the frequent occurrence of multiple cloud layers. Curry et al. (1988) concludes that in such situations, strong turbulent mixing occurs in the uppermost cloud layer, which is decoupled from the surface. The lowermost cloud layer can occur as a kind of fog in a stably stratified boundary layer. Exactly such a scenario was observed also by us on 20 June and thus it represents a further hint that such cases might be typical for the summertime Arctic.

The second difference is associated with the fact that the surface latent heat flux as well as the total humidity jump are small over the Arctic sea ice. The consequences for the latent heat release and buoyancy flux in the cloud layer and, in turn, for the turbulence intensity are demonstrated using the mixed-layer model:

The mixed-layer model also highlights an important difference between the Arctic and subtropic stratocumulus. For subtropics, a substantial latent heat flux at the bottom of the mixed layer is typical, while at the top of the mixed layer dry air is entrained. In a mixed-layer model, this leads to an increased buoyancy and heat flux in the cloud layer and consequently to a stronger turbulence. This is illustrated by the sensitivity experiments with the mixed-layer model (Fig. 15). The parameters of the experiments are the same as for the 5 June experiment, apart that we prescribe a gradual increase of the latent heat flux a the bottom of the mixed layer as well as an increase of the total humidity jump at the top of the mixed layer. The typical values for subtropical stratocumulus are $\rho L_e(w'q_t')_0 = 115\ Wm^{-2}$ and $\Delta q_t = -7.5\ gkg^{-1}$ (Duynkerke et al., 2004; Stevens et al., 2005). In contrast, in the Arctic the surface latent heat flux is small. During the ASCOS campaign, latent heat flux in the surface layer did not exceed 5 W m^{-2} (Brooks et al., 2017). Furthermore, a humidity inversion often occurs at the top of the Arctic mixed layer, which leads to the entrainment of moist air. As a result, an increase of buoyancy due to condensation is much smaller in the Arctic stratocumulus. In other words, in the Arctic, the latent heat release during condensation in updrafts is compensated or can be even exceeded by the cooling during evaporation in downdrafts.

The third difference is the shallowness of the mixed layer over sea ice as compared to lower latitudes. This leads to smaller sensible heat flux maximum in the cloud layer and weaker turbulence, as shown by the mixed-layer model:

To understand why turbulent heat flux can be much smaller than ΔLW it is important to consider the following property of the mixed layer. Namely, the total flux of the conservative variable θ_l has to change linearly with height as expressed in equation (C3). There, the total flux is the sum of LW_{net} and $w'\theta_l'$. Due to the fact that usually LW_{net} changes nonlinearly with height (Figure B1), some amount of turbulent heat flux is needed to compensate for this nonlinearity. The needed amount of turbulent heat flux is proportional to how far from linear the LW_{net} profile is. Obviously, for a very thin layer (e.g. a layer between z/h = 0.8 and z/h = 1 in Figure B1) LW_{net} alone is already close to linear so that the cloud layer would cool almost uniformly with height even with a small amount of turbulent mixing. This is not the case for thick mixed layers, where the strong longwave cooling close to the cloud top generates a strongly nonlinear profile of LW_{net}. For a uniform cooling the loss of heat at cloud top has to be redistributed throughout the mixed layer by strong turbulent motions. In such clouds, the maximum of the turbulent heat flux would be closer to ΔLW. Apart from this, the presence of a negative entrainment flux also leads to a decrease of the turbulent heat flux maximum in the cloud. Thus, we can conclude that in shallow or thin mixed layers, indeed, turbulent heat flux can be substantially smaller than the longwave cooling expressed by ΔLW. This explains why not the whole amount of ΔLW is used to force the mixed-layer convection.

2) The analysis of the mean profiles (Figures 3, 7, 11 & 14) and for the turbulent properties (Figures 5, 8, 12 & 15) are different and I wonder why? I would suggest that these plots are made exactly the same, with the same variables/properties in the same spots so they can be put side by side for comparison.

Answer: We agree with the reviewer's point. In the revised version of the manuscript, the figures presenting turbulence profiles (Fig. 4, 7, 10, 13) as well as the mean-profile figures (Fig. 3, 6, 9, 12) have exactly the same structure.

3) I also wonder why the four different case studies are not chronological, starting with 2 June, then 5 June and finally 14 and 20 June?

Answer: We have included the following explanation in the revised version before Section 3.1:

The description of cases does not follow a chronological order but starts with two single-layer low-cloud cases and ends with multi-layer cloud cases. A weak-wind single-layer case is described first in detail because it serves as a reference case where the cloud effect on the ABL structure was clearly dominant throughout the ABL.

4) While the logical structure is based on dynamics characteristics, the text is very case oriented and I recommend that the two cases with multiple cloud layers are merged into one sub-section and analyzed together, following the logical narrative: cloud-driven, surface-driven and dual-layer cases. Or perhaps drop the second two-layer case all together if there's not enough data; having two separate cases is just a bit awkward.

Answer: The structure of the whole paper has been improved and some sections of the paper were shortened, so that we find it possible to keep the description of the two multi-layer cloud cases in separate sub-sections. We find it important not to drop the second case, as we want to stress the important effect of stability on the ABL turbulence structure. To our knowledge, such a comparison has never been presented elsewhere.

5) The turbulence profiles are shown in scaled heights using the total boundary-layer top and indicating the cloud layer with gray shading. However, while incorporating the slant profiles covering substantial geographical distance means that there is no single cloud geometry and I suspect that the increased spread in some turbulence statistics around the cloud base is in fact due to the cloud bases – or rather the base of the cloud-mixed layer - for the different individual profiles is different. I would therefore suggest a two-layer scaling where the cloud layer is scaled separately from the sub-cloud layer.

Answer: This is a good suggestion and we followed it by using such a two-layer scaling for the 5 June case where the cloud bottom height differed between different profiles. This improved the presentation of turbulence profiles. However, it was not useful in other cases because there cloud base was the same at different positions and/or we had just one measurement height below cloud base. Also this is mentioned in the revised paper.

6) The total manuscript is made rather long by the addition of two appendices and I wonder if these are really necessary. Obviously, one need to trust the observations before trusting the analysis but I wonder why this is accounted for here and not in a separate paper or report. This type of tests should be standard background information for the platforms and should not need to be repeated differently in different papers.

Answer: First of all, we shortened some sections of the paper and improved its structure. Although indeed, there is the overview paper and data report, but we bring more details here and address especially the accuracy in clouds which received little attention in the literature. Also the second reviewer stressed the importance of the uncertainty estimates. Thus, we decided to keep the Appendices, but as a compromise, shortened the first one addressing the accuracy considerably.

7) Also, no amount of filtering or correlation while flying up and down in slant profiles trough the top of the cloud changes the fact that different parts of a profile is flown in dynamically completely different domains and analyzing turbulence incorporating both is questionable.

Answer: First of all, we excluded from the time series used to obtain turbulence statistics the parts where the aircraft crossed the cloud top. Secondly, in the revised manuscript, we further stressed the difficulties concerning slanted profiles. We cannot get rid of this uncertainty and only once more stress this fact. From the revised version it should become very clear to the reader that the most reliable information is based on the horizontal sections, but the comparison with the slanted profiles shows that the latter are clearly within the scatter of the horizontal legs and everything can be well interpreted physically.

8) The same is the case for the mixed-layer model; surely there are references to mixed layer models that could be used; moreover, the concept of a single mixed-layer is questionable when there may in fact be two different mix-layers on top of each other when the cloud-mixed layer is completely or partly decoupled from the surface-mixed layer.

Answer: We are applying the mixed-layer model to the case and, in particular, to the well-mixed part of the atmospheric profile where we believe the mixed-layer model represents a simple and adequate approach. Moreover, in the new version of Discussion we base several of our conclusions on the mixed-layer model results. Thus, we find it important to write out the exact equations that we are using. This is justified also because existing mixed-layer models differ in details. Our model is using the simplest representation of the capping inversion and of the entrainment parametrization. Moreover, we moved some equations related to mixed-layer scaling to the Appendix, so that all the equations related to the mixed-layer model are there and they do not interrupt the narrative.

9) Finally, I would wish for the paper to be more concisely written with a clearer narrative and there are also some language issues here an there.

Answer: This is an important point. We dropped many unnecessary details and tried to make the narrative as clear and concise as possible.

Minor comments

Page 5, lines 17-19: What about LWP from the remote sensing on Polar 5?

Answer: Reliable LWP estimates from remote sensing on Polar 5 are not available yet. Current retrieval of LWP from microwave radiometer and solar spectral imager often suffer over sea ice surfaces and in the presence of cloud ice particles. LWP Data so far are published only over open ocean. Advanced improved retrieval are in development but not available at this point.

P6, l5: I think "accuracy" is the wrong word here; data from slant profiles is as accurate as any other data. The question instead is what they represent.

Answer: we reformulated the sentence and explained in more detail what the slanted profiles represent, namely:

Turbulence statistics were calculated using a moving window of about 100 s width corresponding to a layer of 100 m thickness and to a flight distance of about 5-10 km. Within each moving window a polynomial trend was removed. One can interpret the obtained profiles in a way that every point represents an approximation of the true values averaged over the corresponding height interval. Thus, this method provides continuous vertical profiles but suffers from two drawbacks: 1) Relatively short time spent at a certain height resulting in a higher statistical uncertainty; 2) smoothing of the vertical profiles. It should be stressed that the obtained statistics from slanted profiles do not strictly approximate the values from horizontal legs, except the cases when turbulence statistics change with height very slowly. It is important that the flight segments crossing the inversion at the ABL top are excluded from the analysis of slanted profiles so that the considered profiles start below the ABL top and steep jumps of quantities like temperature and wind components in the capping inversion are not misinterpreted as turbulence effects.

P7, l1-16: Are all the formulas really necessary?

Answer: We skipped some of the most obvious ones and included the rest in the text rather than writing each formula at a new line.

P7, l4: Why is a (positive) scalar momentum flux used instead of the PBL-traditional along- and across-wind momentum flux?

Answer: In the revised version we rotated the coordinate system along the mean ABL wind and present the components of the momentum flux as the along- and across-wind. The same we do for the variances of u and v.

P7, l21: Either it is partly transparent or it isn't. If it "appears to be", then it is; the eye's doesn't lie. And I don't understand what "almost transparent" means.

Answer: We have reformulated this place to make it as clear as possible and avoided the word "transparent", namely:

On the MODIS image one can see the contours of sea ice floes (Figure 2) through the cloud layer, however, the clouds found during the flight, two hours after the satellite overpass, turned out to be rather solid (Fig. 3f).

P7, l23: How can it be "rather solid" if it at the same time "appears to be almost transparent"

Answer: see the answer above

P9, l10: The potential temperature doesn't have a defined freezing point. The real temperature has, but depending on pressure the potential temperature can very well be above 273.15, while the temperature is below and vice versa. For example, with a zero degree near-surface *temperature, the near-surface potential temperature may well be above zero and will remain above zero even when real temperature drops below zero with height.*

Answer: We agree with the reviewer. We shortened the description of mean profiles and avoid using “freezing point” with respect to potential temperature.

P*9, l33: Why is net LW defined positive; this is different from most other studies where it is defined as incoming minus outgoing, becoming negative at the cloud top. Moreover, what is the meaning of “positive upward” and how is that different from “negative downward”?*

Answer: We choose to use the same convention for turbulent and radiative fluxes: we define a flux positive when it is directed along the z axis upward. We find such a convention useful especially when considering the turbulent and radiative fluxes together, e.g. in the mixed-layer model framework. We follow in this some of the earlier studies and textbooks, but we agree that there are also many studies where a different sign is used. We refer to this issue in the revised version as follows:

We assume here that LW_{net} is positive, when the net flux is directed upward (as in, e.g., Nicholls and Leighton (1986), their Fig. 7).

P10, Figure 3 and elsewhere: very different cloud thickness even when the cloud top, and hence PBL depth, are much more similar. Think about other ways to do the scaling of the turbulence profiles.

Answer: As discussed above, we followed the suggestion and used a different scaling.

P11, l1: At what level is the heat flux significant, or do you mean “substantial” here? I have found it useful to only use “significant” when actually discussing significance.

Answer: we followed the suggestion and replaced significant with substantial wherever it was relevant.

P11, l3: “cloud base” is better than “cloud bottom”.

Answer: We replace cloud bottom with cloud base throughout the text.

P11, l4: One flight leg agrees with the mean of the slant profiles while one is much larger; could be anything. And what is entrainment(?); just another word for turbulence!

Answer: The differences between slanted profiles and results of horizontal legs are largest close to cloud top and in the capping inversion. There, we have several possible sources of errors. The observed stratocumulus cloud top was not smooth but rather inhomogeneous with cloud towers reaching into the capping inversion. Sometimes, the aircraft is in the clouds sometimes slightly above the cloud top. Such inhomogeneity is seen especially by the horizontal legs and fluctuations may depend very much on the aircraft track. Also the aircraft might sometimes cross the capping inversion, which also leads to artificial effects. This is the reason why results should not be overinterpreted above $z/h > 0.8$. The slanted profiles represent more the local conditions so that the scatter is smaller also because the region above cloud top is excluded in the calculation of turbulence statistics from slanted profiles (see also below).

P11, l8: The slant profile TKE is height constant, but not that from the flight legs.

Answer: See above, the region near cloud top has lower accuracy. Nevertheless, we did not want to skip these results since at the capping inversion there might also exist intermittent turbulence so that the fluctuations along horizontal legs might point also to this physical phenomenon.

P11, l13-14: Change order in the sentence "The difference ... upper part". Start with what you see here and then how that is different from Lenschows old results. Then conclude on the difference…

Answer: We reorganized the manuscript and now the Lenschow parameterization is addressed in the Discussion

P12, F5 and elsewhere: The outlined cloud is only valid for one location; consider a two-layer scaling or indicate how the normalized-base height varies for the different profiles.

Answer: We did this

P13, l6: How is a "negative downward transport" not an upward transport?

Answer: Reformulated

P15, l11: Awkward "However, the ... 5 June"; consider revising text.

Answer: Revised

P16, l7-8: Awkward "s_w shows ... 2 June"; consider revising the text, e.g. "The cloud layer values of "s_w shows ...".

Answer: Revised

P16, l11: The wind speed increases with decreasing altitude in all the profiles but more in T5 & T6 than in the other profiles. However, the reduction in these profiles closer to the surface, probably giving rise to the mentioning of a low-level jet, is only due to the fact that these profiles reaches lower than the other. If you want this to be a "low-level jet" you'll have to be more distinct in what you mean and how you can be sure none of the other slant profiles does not also have one, albeit weaker.

Answer: A definition of what we consider a low-level jet is given and obviously the low-level jet was present over sea ice on 2 June.

P16, l29-34: Might as well drop this; adds nothing of value to this paper.

Answer: we dropped this part

P17, F8: How do you explain that the crosswind variance is larger than the alongwind variance ner the surface, when the stratification is stable and the wind direction nearly constant with height?

Answer: After the suggested coordinate rotation, the alongwind variance is larger than the crosswind variance, especially in the slanted profiles.

P20-21, F11-12: In the first figure the potential temperature seems to be linearly increasing with height below the capping inversion, while in the second figure it is constant up to z/h ~0.6-0.7?

Answer: The observations during descent and horizontal legs are combined in one Figure 9. A slight increase is indeed obvious. The cloud field in the ABL was inhomogeneous and at some locations cloud base height was lower, so the potential temperature profile switched from dry to moist adiabatic one at lower heights, and at some locations – at higher heights.

P22, l1: Is it more interesting to compare with 5 June than with 2 June? Both have a similar O(10 W/m^2) heat flux at the cloud top, driven by cloud-top cooling, absent in the two layer case.

Answer: In the revised manuscript, a comparison to both 2 and 5 June is included.

P22, l3: "half as large" is better than "twice as small"

Answer: revisited

P22, l5-7: There are two things at play here the presence or absence of cloud-top cooling and of substantial surface friction (i.e. wind speed).

Answer: We took this into account in the revised version.

P25, l2-3: So how is the PBL depth defined; I seem to have missed that or it isn't described. I would have though that z/h would be unity at the inversion base?

Answer: revisited, the PBL depth in this particular case was first not correctly defined as the cloud top, but when we define it at the inversion base this leads to better scaling.

P25, l11-14: Drop this; this is not the place. If you here feel a need to convince the reader that your turbulence data is actually accurate you need to go back and consider the whole manuscript.

Answer: We decided to keep it as it is because we find it important to stress it exactly when the reader can see the results in the Figures. This piece is in the very end of the Results section and, to our mind, it does not interrupt the narrative. Also, we find this result impressing since the agreement of measurements from independent nosebooms in situations with very small signals has never been demonstrated.

P27, l17-27: Too much text devoted to discussing and ascertaining the accuracy of the methods, especially the slant profiles. This has been tested and used before many times by other authors; nit needed any more!

Answer:

We partly agree with the reviewer that we are not the first who used the combination of horizontal legs and slanted profiles. Mahrt (1985) and Lenschow et al (1988) presented the comparison of both as well as the uncertainty estimates. Thus, our approach is not new and we decided to reduce the emphasis on the agreement of the two methods in the Conclusions. Nevertheless, the above mentioned studies are based on flights in a stable boundary layer. The turbulent and mean structure of mixed layers in the presences of clouds is often quite different from a surface-based stable layer due to different eddy sizes. Thus, we still think that the comparison of the two methods demonstrating their agreement or disagreement is valuable, but probably should receive less attention in the Conclusions section. Therefore, following the reviewer's comment we shortened the rather broad discussion of the agreement of two methods and instead stress that using both methods proved to be beneficial also in the cloud-topped mixed layers.

P29, l3: So is it low here? Turbulence in the cloud is driven by cloud-top cooling; is that lower because the cloud is over sea ice rather than open water?

Answer:

In the revised version we clarify, that the cooling-driven turbulence is not weak, but that the values of sensible heat flux are much smaller than in a surface-driven convective ABL during cold air advection over open water. Previous estimates of accuracy for the turbulence probes on P5 and P6 were obtained only for the latter conditions and, thus, need to be reassessed. In the revised version of the considered paragraph, we also note that:

Moreover, as in earlier studies (Curry, 1986; Finger and Wendling, 1990) it is important to quantify empirically the uncertainty of the derived turbulence statistics.

P33, l19-29: Not quite sure what is going on here. Regardless if you fly in a vertical gradient or not, if you keep the same altitude you stay at the same temperature and therefore need no correction. Or you fly up and down, and then you're not at the same place in the vertical anymore. Or the layer thickness varies along the flight. So explain this better.

Answer:

Also the best aircraft and best pilot will never be able to fly exactly over 10 Nm in one altitude. There is always fluctuation in height of plus/minus 10 m, sometimes more depending on the aircraft and turbulence. However, in the capping inversion, potential temperature has a strong vertical gradient so that a correction is useful. Since the effect is not large we shortened this point and do not show anymore the figure. We write now:

Also during horizontal flight sections an aircraft cannot fly always exactly in one altitude. Especially in the uppermost part of the cloud with the capping inversion the remaining fluctuation of aircraft height of about ± 10 m is therefore correlated with changes of potential temperature caused by its vertical gradient. We corrected this impact on the temperature series based on the mean measured vertical temperature gradient along the flight leg.

REVIEW 2

We thank the reviewer for thorough reading and very valuable and constructive comments.

In the following the Reviewer's text is cited in italic fonts, our answers in roman fonts.

Overall comments:

In this paper, the authors reported the vertical structure of atmospheric boundary layer thermodynamics and dynamics, and in particular, turbulence quantities derived from measurements by airborne instruments for a few cases with weakly forced liquid clouds. I recommend the publication of this paper if the following issues can be adequately addressed.

I think the paper can be improved with

1. More meaningful interpretation of the results.

Answer: The Discussion has been completely rewritten. In the revised version, we provide a deeper analysis of the obtained results and provide a comparison of our results and progress with earlier studies of summertime stratocumulus over Arctic sea ice. Also, we stress the differences of Arctic stratocumulus from the subtropical ones.

2. Highlighting the new knowledge from the uniqueness of this dataset and reducing descriptions/discussions of features that have been very well known.

Answer: We agree with the reviewer that the paper would benefit from a better focus on new results. After revision, the description of cases was significantly shortened and the new results relative to previous studies are stressed. We summarize the main new features in the following.

1. Our study presents for the first time (to our knowledge) a case of Arctic stratocumulus with near-zero wind speed, so that the effect of radiative cooling on turbulence is documented separately from other mechanisms of turbulence generation.
2. Also for the first time we present a comparison of the two multi-layer cases which differ by stability in the ABL, namely, stable vs unstable. Previous studies using aircraft data documented only cases with a stably-stratified ABL in the presence of multiple cloud layers. Thus, our study broadens the parameter space in which observations of the ABL turbulence structure in the presence of clouds exist. This is clear from the new Table 2, where our cases are listed together with previously observed ones.
3. Using a mixed-layer model we present the analysis of why the observed turbulent heat flux in the cloud layer is substantially lower than the jump of the net longwave flux at the cloud top (see the Discussion). This was first observed in Arctic clouds by Curry (1986). We clearly show that in shallow mixed layers the maximum of turbulent heat flux is smaller than in thick mixed layers.
4. We stress the difference of the Arctic stratocumulus from the better studied subtropical stratocumulus clouds (see the Discussion). In particular, using the mixed-layer model we show that small latent heat flux at the base of the mixed layer as well as a small total humidity jump at the top of the mixed layer (or even a humidity inversion), as typical for

stratocumulus over Arctic sea ice, leads to much smaller buoyancy forcing in the mixed layer and, consequently, weaker turbulence, as compared to subtropical stratocumulus.

3. More discussions about (a) the uncertainty of the statistics from the slanted legs, especially near cloud boundaries (e.g., cloud top) where the size of a feature may be small and (b) the uncertainty in high-order moments, which are more difficult to obtain than low-order moments. It is critical to describe both bias and variance in the derived quantities if the motivation of this paper is to provide a benchmark for model evaluation/parameterization development.

Answer: As suggested by the reviewer, we added the discussion of the uncertainties of the statistics from slanted legs as well as the uncertainty in higher-order moments. Namely, the following paragraph was added to the Discussions section:

In connection with the above discussion, one should also keep in mind the uncertainty of the observed turbulence statistics. The magnitude of the uncertainty for heat and momentum fluxes obtained from horizontal legs is discussed in Appendix A. Similar to earlier studies (e.g. Curry et al., 1988; Finger and Wendling, 1990; Inoue et al., 2005), the uncertainty is estimated empirically by comparing results from shorter segments of a long horizontal leg. Theoretical estimates of sampling errors due to an insufficient length of flight legs also exist (Lenschow and Stankov, 1986; Lenschow et al., 1994). Based on large Eddy simulation (LES) Petty (2021) showed that there is a good agreement between the empirical and theoretical estimates. However, neither theoretical estimates nor LES account for the horizontal non-turbulent inhomogeneities that exist in nature and complicate the analysis of the observations. It is even harder to obtain theoretical estimates for slanted profiles as statistical properties of turbulence are functions of height within the ABL. Thus, we rely on empirical estimates of the uncertainty also for slanted profiles. So, we can consider the range of the slanted profiles (red hatched regions in Figures 4 and 7) obtained at different positions as a measure of accuracy obtained from these profiles. This is not more than a rough estimate and we expect that especially near the inversion (above z/h = 0.8 the measurement errors are large for the slanted profiles but also for the horizontal legs. This hold also for previous aircraft campaigns (Lenschow and Stankov, 1986). In the future, flights should be performed repeating the same profiles and including horizontal flight legs at the same positions to determine the accuracy. Finally, it should be noted that theoretically the uncertainty for third moments is higher than for the second ones. It can be reduced by considering longer averaging intervals, but the drawback of long horizontal legs is that horizontal inhomogeneity starts to affect the results. In case of slanted profiles, it is simply not possible to increase the leg length, as it will either cover a very large height interval or require a very slow ascent or descent, so that the horizontal distance flown during ascent/descent would become too large.

4. The addition of a concise summary of the main characteristics of all cases (maybe using a table).

Answer: Such a table, including also the cases from previous studies has been added (Table 2 in the Discussion section)

Specific comments:

1. *Aren't the markers corresponding to 12 W m^-2 also measured at PS? The authors said the value 'became close to zero for slanted profiles'. Does it mean the mean measured value dropped to close zero or 'mean minus 1 standard deviation' got close to 0?*

Answer: This piece was clarified. We meant that the flux at the lowest flight level was positive (about 10 Wm^{-2}) at the PS location and close to zero in the slanted profiles at that height.

2. In Figs. 5c and 5f (and similar plots), the profiles at PS and flights are quite different. Please reconcile them. For Fig. 5f, the near constant TKE only occur in slanted flights, why? Even for the cloud layer, shouldn't there be height-dependence of TKE? The data from PS seem to suggest a very noisy profile. Please explain.

Answer: A height-constant TKE is typical for convective mixed layers according to previous studies. Close to the ABL top and inversion the increase of TKE according to the horizontal legs is most probably associated with proximity to the region of a jump-like change in wind direction across the ABL top. Horizontal variability of the cloud top height lead to the fact that sometimes the aircraft might have sampled the air from above the ABL top. One should interpret the results close to the ABL top with caution. The discussion of the increase close to the ABL top uncertainty is included in the revised version.

3. In Fig. 5g, if the positive S_w is associated with some surface-driven turbulences, why don't we see a signature in other panels, e.g., Fig. 5d?

Answer: Indeed, the shift from negative to near-zero and even slightly positive values is most pronounced in the vertical profiles of S_w. Nevertheless, one can observe changes also in other profiles. This became more obvious in the revised version of the figure where we use a different vertical coordinate. Clearly, a decrease of sensible heat flux, change in momentum flux, variances and TKE is visible across the cloud base. Also, a jump-like change in wind direction is seen in the subcloud layer in several profiles. This points to a possible weak coupling between the cloud and surface-based mixed layers. To reflect this, the following paragraph was added to the revised manuscript:

Based on the vertical profiles of several turbulence statistics, as well as on the profile of mean wind direction (Fig. 3c) we can conclude that the fully mixed layer associated with the cloud was perhaps weakly coupled to the surface-based mixed layer. As discussed above, this is most obvious in the vertical profile of S_W. The absolute values of other statistics such as H, σ_u, σ_v, TKE, TSH, and TTV also show a slight, yet a clear decrease in the subcloud layer. It is important to note that based on horizontal legs alone it would be harder to observe this. This shows the advantage of considering slanted profiles together with level flights.

4. In Fig. 7b, please be specific of the features that indicate 'low level jets'. There is no clear peak in wind speed in any profiles. This is different from the profiles (T4 and T5) in Fig. 3b.

Answer: Based on the finding that wind speed decreased with height in all profiles on 2 June and assuming that wind speed has to be zero at the height of the roughness length, we concluded that there must be a maximum of wind speed in the boundary layer. Thus, the low-level jet was present. In the revised version, the occurrence of the low-level jet is reported only for those profiles where the maximum of wind speed in the ABL is clearly visible.

5. P. 16 Lines 4 and 7, what is a profile flight? Do you mean slanted flight?

Answer: corrected to slanted flights

6. In Fig. 9b, why is sigma_w^2 so large near cloud top? Shouldn't they be limited near cloud top (like in Fig. 5d)?

Answer: This figure and the corresponding discussion is now omitted. The first reason is that we needed to shorten the paper and make it more concise. The second reason is that horizontal flight legs were not available for this location. Nevertheless, we agree with the reviewer that one expects a decrease of σ_w^2 close to the ABL top. One possible reason why this was not observed is that each point giving turbulence statistics derived from slanted profiles represents a height range of about 80-100 m.

7. In Fig. 9c, I agree that S_w for T1 is largely negative, but certainly not for T2. Please explain.

Answer: As described above we dropped this figure partly because there were no horizontal legs which would bring more confidence to the results.

8. Is it possible to add a panel for LW cooling rate profile in Figs. 11 and 14?

Answer: In the revised version, the profiles of the net LW flux as well as of the cooling rate are presented in a single figure, namely B1 in the Appendix B.

9. In Fig. 12, if the conditions for all 3 profiles are so similar, why not use the space to show profiles associated with inhomogeneous surface? It would be interesting to see how different they are from the profiles associated more homogeneous surface.

Answer:

We did not include this because of the following reason:

Concerning results of the horizontal flight sections, Polar 6 could not complete the two southern staggered patterns due to icing problems. The southernmost pattern by Polar 5 did not show substantially different results apart from smaller values of heat flux and TKE due to lower wind speed and warmer air temperature. Thus, we concentrate here only on the three northernmost cross-wind staggered legs (Figure 10).

This text is included now also in the paper.

10. In Fig. 12g, why should S_w be so positive near cloud top? The surface-driven eddies should not be able to penetrate the inversion on top of the lower cloud layer anyway.

Answer: The point above the cloud top (above the strong capping inversion) should not be trusted since the turbulence signal is very weak there and nearly all statistics are near zero. Especially S_w should not be trusted because $S_w = \overline{w'^3}/\overline{w'^2}^{3/2}$ and division by a very small number can result in a large value.

11. The discussion section is pretty poorly written. One would expect a rigorous *estimation of uncertainties, connection with other studies, the key insights from the presented research, and so on (although not necessarily all of them) in discussions. Instead, the authors presented a comparison with mixed-layer model calculation without a clear motivation. (Also, I am not sure why only the cloud-top driven case is discussed. The authors said they showed that the cloud top cooling is strongly affecting the turbulence profiles. But most cases they presented were surface driven.)*

Answer: Following the reviewers suggestion, the discussion is completely rewritten. The revised version focuses on a comparison with earlier studies, as well as on insights from mixed-layer modeling in comparison with observations. The usage of the mixed-layer model is now clearly motivated. Uncertainties are also discussed. There is still some focus on the cloud-driven case because, as we stress it, it is, to our knowledge and as follows from Table 2, the first time when a case with low wind speed is presented.

Technical issue:

Please use consistent line types (e.g., why lines for Figs. 11a-11d but dots for Figs. 11e-11f?), variable names (V in Fig. 12b, U_hor in Fig. 15b, but 'wind speed' everywhere else?), and scales (T_sh in Fig. 12j but 1000 T_sh everywhere else?). Also, if different ranges of axes are used for same variable across different cases, please make sure there are common ticks/grid lines for easier comparison across cases.

Answer: in the revised version, the same format is used for all figures. Unfortunately, it is not possible to use the same axis ranges everywhere because the absolute values of some variables, for example of TKE, strongly differs between cases, e.g. because we have cases with stable and unstable conditions. In the stable cases signals are much smaller, nevertheless reasonable but this would not be seen when the axes are the same as in other cases. Using one single range would result in plots which are difficult to comprehend.