# Authors’ Response to the Review by Ian Brooks

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We are grateful to Ian Brooks for the insightful comments and suggestions on our manuscript. We respond to them in detail below. The original review is given in black, our answers in blue. The responses also mention the specific corrections which were applied to the manuscript.

The results are, for the most part, routine – such boundary layers are well studied (even if our understanding of all the interacting processes is incomplete), and most of the results are in broad agreement with previous studies (as noted in the conclusions). They remain, however, a useful contribution to the field, and do include some unique results – those of very small-scale turbulent properties: profiles of dissipation rate, and isotropy.

We agree with the general assessment that part of our analysis confirms known findings. From our point of view, these analyses are nevertheless important at this point in order to be able to correctly classify and evaluate the observations of small-scale turbulence. This makes the manuscript a bit longer, but we think - like another reviewer - that this is justified in this case. We believe that a reader should be able to navigate through the text using the section and subsection titles, e.g. skip some of the analyses without losing the thread and find the information which is relevant for him.

There is a limit to how much can be gained from analysis of individual case studies. I would encourage the authors to consider expanding their analysis in future to include all the flights from this campaign (many more than the two used here) to produce a more general synthesis of turbulent behaviour for the coupled and decoupled boundary layers.

This is definitely a suggestion that will be considered for the future. Here, we had to find a compromise between detail and scope, so we first decided to focus on two case studies with a lot of detailed analysis. For the future, we will explore individual aspects with more data to make the results more statistically significant.

On the other hand, the ACORES data alone might not be sufficient to provide statistically sound conclusions and we consider extending the analyzed dataset with the available data from other field experiments. In total, there were 17 research flights during the ACORES (Siebert et al., 2021, Table 5). Five of them correspond to clear-sky conditions, four to already dissipating or not yet developed stratocumulus clouds which limits the true STBL observations to 8 flights. Each flight lasted up to about 2 hours. This flight time was always disposed between sampling the cloud top structure and the boundary layer itself.

**Specific comments**

1. The overall structure of the manuscript follows the conventional pattern of background / methods / results / conclusions. This is fine, but I found that the sheer number of different variables being defined resulted in a very long methods sections, where it wasn't always clear what the real utility of a particular parameter was. By the time the reader (or this reader anyway) gets to the relevant results, they've forgotten what all the different symbols and parameters are. It might be worth considering modifying the structure to mix parameter definitions and results – defining/explaining particular quantities immediately prior to presenting the results on them. This is very much a decision to be made on personal preference regarding the readability, I'm sure another reviewer would argue against doing this.

   We did consider the suggested structure of the manuscript. Actually, our first internal draft followed exactly this approach. However, we came to the conclusion that the multiple explanations of the applied methods disturb a consistent presentation of the results. Another argument in favor of a classical structure is that the reader knows where to expect which content. We are aware that our manuscript is rather long and with its structure we intended to enable readers to easily find the information which is the most relevant for them. Some might be interested in the results only, some in the very details of our methods.

2. On a related note, there are a LOT of acronyms defined here, not all of them are used very often (eg 'CB' is only used 6 times after being defined... not worth the space saving traded off against having to go back and find out what it means'. I found it easy to confuse many of these because of minor inconsistencies in how the layer names mapped to acronyms– I kept reading 'SCL' as 'sub-cloud layer' instead of 'stratocumulus layer', whereas 'SBL' (sub-cloud layer) I wanted to read as 'stable boundary layer'... which is a common usage, but irrelevant here.

   Following the suggestion of this and other reviewers we reduced the number of acronyms by replacing them with the corresponding expanded expressions, in particular those which were not used frequently in the text. This group includes: SC, BL, ENA, TAS, CTEI, J11, WB04, YA00, CP, DCP. The last two were shortened to C and D, respectively, and only kept in sec. 6 to order the list of conclusions. We prefer to keep the acronyms of the following types:

   – denoting the sublayers of the atmosphere: STBL, SML, TSL, SBL, SCL, EIL, FT, because they are used very frequently in the text as well as in tables and figures,
   – denoting our flight segments: PROF, LEG, for the same reason,
   – commonly used abbreviations: TKE, LCL, SFC, PSD, because we expect them to be familiar to the readers,
   – names of instruments or platforms: ACTOS, SMART-HELIOS, MODIS, GPS, for the same reason.

   Moreover, we added the expanded names of the sublayers to the headings of Tables A1 and A2.

3. Figure 2 and 4 – it might be useful to indicate cloud base and top on the figures so the reader can immediately see how the flight legs relate to cloud level. The line style for different sections of the flight track are consistent with those used on the later profile plots - this is clear for fig 4 (flight 14) where the profile plots show 3 distinct profiles; but less so on

fig 2 (flight 5) where there are only 2 line types. It appears that in the profile plots the dashed line, which looks like a single deep profile, is actually a composite of several profile sections separated in time, and spanning different altitude ranges. This is fine, but should be made explicit since it has a bearing on variability of the data.

The figures were modified. The individual penetrations (determined manually based on $\theta_l$, $q_v$ and $q_l$ records) of the EIL top, SCL top (=EIL base), SCL base (=SBL top), TSL top (=SBL bottom) and TSL base were indicated in the altitude profile with additional symbols. The labels denoting PROFs and LEGs were added at the top. The grey line illustrating altitude profile for the whole flight was plotted only outside the colored segments to improve the visibility of the black line used within the segments.

Indeed, the same line style was used for several PROFs which do not overlap in their altitude ranges. We agree this might have been confusing. Therefore, additional short explanation was added to the text and the captions.

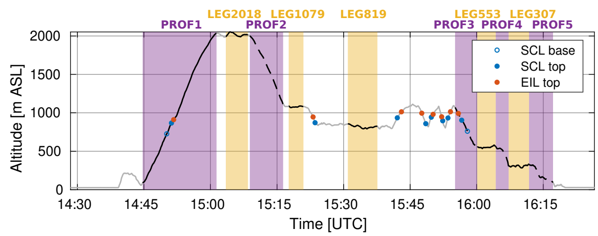


**Figure 2 corrected.** ACTOS altitude in flight #5 with marked selected profiles and horizontal legs. PROFs are ordered chronologically, LEGs are ordered according to their altitude. Line styles of the PROFs are consistent with the figures in following sections Altitude ranges corresponding to PROF2-PROF5 of this flight do not overlap and are all marked with dotted lines. Dots indicate the penetrations of the boundaries of the specific sublayers described in sec. 3.

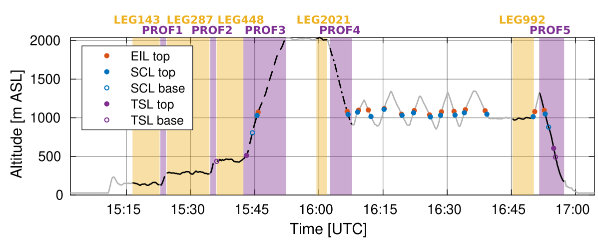


**Figure 4 corrected.** As in Fig. 2 but for flight #14. Line styles of the PROFs are consistent with the figures in following sections. PROF1-PROF3 are all marked with dotted lines because their altitude ranges do not overlap.

4. At various points in the discussion of results, specifically the plots of profiles and leg-averaged values, reference is made to a particular flight leg ‘LEG2’, ‘LEG3’ etc. I found this unhelpful, since I couldn’t immediately identify which leg was which on the plots... what altitude was it? It would be more useful to simply refer to the altitude of the leg.

   The legs can be identified by referring back to figures 2 and 4, but (a) that requires the reader to go searching back for the relevant figure, and (b) there is a potential cause for confusion, because the leg numbering (assuming it is chronological. . . this is never explicitly stated) appears to be inconsistent when referred to the profiles, since for flight 5 the legs start high and word down, and on flight 14 start low and work up (and then down again for final leg). All we really need to know in the discussion is the altitude, the leg number is a distraction'

The numbering of LEGs and PROFs in our manuscript was chronological which was stated in line 111, page 4. We agree that such a convention may be confusing for the reader. Therefore, the ordering of LEGs was changed into LEGX where X stands for mean altitude (m a.s.l.). For PROFs, we kept the chronological numbering. We improved the clarity of the relevant explanation in sec. 2.3. In addition, the labels denoting PROFs and LEGs were added at the top of Figs. 2 and 4.

> Segments of two types were selected from the measurement records: profiles (PROFs) and horizontal legs (LEGs). For convenience, for each flight PROFs are ordered chronologically according to their time of execution and referred to as PROF1-PROF5 while LEGs are ordered according to their mean altitude (m above sea level).

5. Line 169: 'Negative values suggest instability. . . ' – for clarity it would be useful to explicitly state the variables involved here 'Negative values of Dql suggest. . . '

   Corrected accordingly. 'Negative values of $\Delta\theta_l$ and $\Delta z$ suggest instability ... '

6. Line 171 'The parameter of YA00. . . ' – again, be clear and name the parameter, not (just) the paper where it was first defined. . . make it easy on the reader.

   Corrected accordingly. 'The parameter $\mu$ is plotted ...'

7. Line 186: 'probably there were some clearings. . . ' – while the effects of such clear air regions will get averaged out by the vertical binning/averaging/smoothing applied to generate the profiles, it ought to be possible to identify if they actually occur from the raw, high rate data, and not have to rely on a vague statement of 'probably'.

   We verified there were indeed some cloud clearings indicated by our high rate data of $q_l$. See the plot below. The text was updated accordingly.

   > There were cloud clearings penetrated as ACTOS moved along the slanted path, visible in the high rate records of $q_l$ (not shown here).

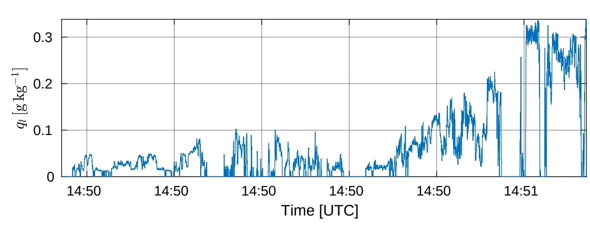


**Figure.** Cloud penetration during PROF1 of flight #5.

8. Line 202-203: ‘Suitable normalisation. . . ’ – Purely my preference, but I’d cut this line. I don’t think it adds anything useful unless you go into detail about the normalisation & averaging referred to.

   We removed the sentence. By suitable normalization we meant the method of Ghate et al. (2015). They identified STBL sublayers in each sounding, normalized the height so that each sublayer has the depth of 1 and then averaged the relevant properties at the normalized heights. This method of profile averaging preserves the sublayer structure despite the depths of the sublayers may vary between the individual soundings.

9. Line 232: what are the instrument issues that resulted in problems with the lateral wind components? It’s not essential to document this, but, depending on the cause, might be useful for other researchers trying to make similar measurements.

   We observed artificial jumps of the amplitude 0.1 - 1 $\mathrm{m\,s^{-1}}$ in the records of the lateral horizontal component which seems to appear for true air speeds above about 12 $\mathrm{m\,s^{-1}}$ or so. There were virtually no signs of such behavior in the records of the other two velocity components. As far as we know, the issue is specific for that ultrasonic anemometer model. Wind tunnel investigations suggest a problem with the transformation between the axis aligned coordinate system into an orthogonal system. The artifacts were almost not visible along the transducer pairs. However, you have to make a decision which system you want to select for data storage before the flight - we stored in an orthogonal system. Nonetheless, we are not absolutely sure about the reason. Therefore, we prefer not to share the speculations in the published paper until a proper investigation is completed. We added short information to the text:

   > The lateral channel of the ultrasonic anemometer was affected by a substantial level of artificial fluctuations (up to 1 $\mathrm{m\,s^{-1}}$ in amplitude) due to instrumental issues. The origin of this problem is under investigation. It seems to appear for true air speed above about 12 $\mathrm{m\,s^{-1}}$ which makes it relevant for most of the flight time.

10. Line 294 and 307: both reference a ‘lateral component’ when the parameter referred to is derived from vertical velocity. Yes, w is ‘lateral’ with respect to the mean wind vector, but it might be clearer here to be explicit and refer to the ‘vertical component’, not least because you have previously noted problems with the ‘lateral’ velocity measurements, where lateral refers to the horizontal cross-wind component, and so is a potential source of confusion.

    We are grateful for pointing out this inconsistency. Indeed, the reason for the confusion was that we used the word ‘lateral’ to name two different aspects: (1) the component $v$ and (2) the orientation with respect to the mean wind vector which is relevant for turbulence theory and the choice of the constant $C$ (then both $v$ and $w$ are considered lateral in contrast to longitudinal $u$). We corrected the description according to the suggestion by sticking to the meaning (1).

11. Line 374-377: The unexpectedly high variances above cloud are presumed to be artefacts resulting from the presence of gravity waves. While I agree that is quite likely, it should be possible to demonstrate it. Coherence/phase/amplitude plots of the correlation between vertical velocity and the other variables should show a clear scale of waves. Power spectra or ogive plots of variances/covariances should also show that most of the variance/covariance results from a narrow range of wavelengths that can be related to gravity waves.

We prepared a brief analysis of the fluctuations recorded in the first half of LEG1079 for which we noticed some quasi-regular oscillations. The figures below present power spectra of $w$, $u$, $q_v$, $\theta_v$ and cospectra of $wu$, $wq_v$, $w\theta_v$. We applied Welch scheme implemented in MATLAB functions `pwelch` and `cpsd`.

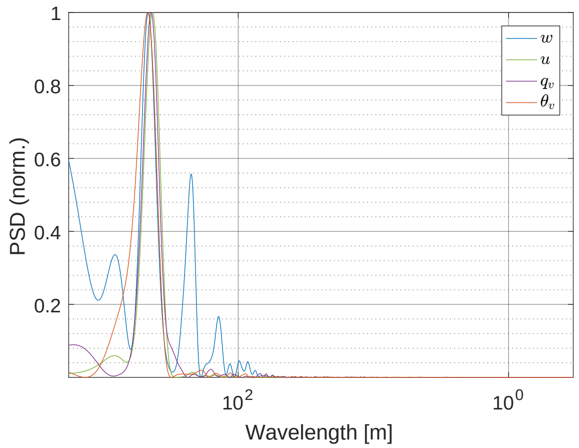


**Figure.** Power spectrum density of the first half of LEG1079 normalized by its maximum value (linear scale).

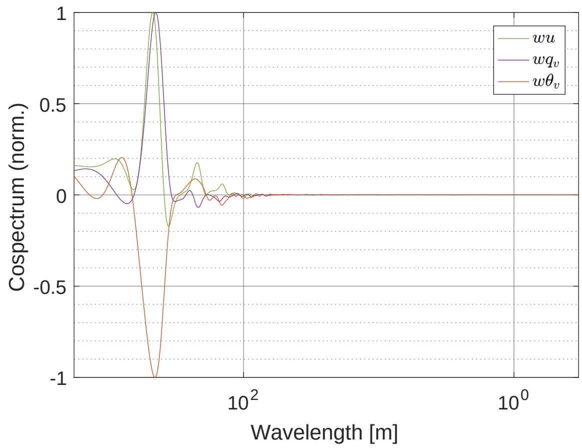


**Figure.** Cospectrum for the correlations in the first half of LEG1079 normalized by its maximum absolute value.

All the power spectra exhibit a pronounced peak at the wavelength of about 450 m. Hence, most of the variance can be attributed to such oscillations. The same can be observed in the cospectra. Thus, most of the covariance can be attributed to those range of wavelengths. We added a short comment justifying our speculations to the manuscript.

> Estimated values of the TKE are also large in the FT above the temperature inversion. This is rather an artifact due to the presence of gravity waves favored under stable conditions (the power spectra of $w$, $u$, $q_v$, $\theta_v$ and the cospectra of $wu$, $wq_v$ and $w\theta_v$ indicate the dominant contribution of the wavelength of about 450 $\mathrm{m}$). Recall that LEG1079 was flown very close to the EIL and the cloud top which often features undulated interface.

12. Line 390: The statement regarding T and q as being passive tracers with no significant sources at the transition layer is…arguable. There is no real 'source', but for the SML, the gradient across decoupling transition layer acts as a source/sink term, entrainment mixing brings drier/warmer air down to top of SML (local effective source/sink). There must be some mixing to give high T/Q variances here.

    Then…' The TSL features the gradient of qv (c.f. Fig. 6) which might explain increased local fluctuations.' – what other source of increased fluctuations could there be here?

    This is a misunderstanding due to the unclear formulation of our point. We just do not consider gradient production (Term IV of the variance budget equations in the formulation of Stull (1988), his Eqs. (4.3.2) and (4.3.3)), as 'sources'.

By ‘sources’ we meant the processes of the type described by Term VIII in Stull’s Eq. (4.3.3), e.g. radiative cooling. Obviously, gradient production is present in our case. The sentence was rewritten to clarify this issue:

> $T$ and $q_v$ can be considered passive scalars which undergo mixing. The increased variances are caused by gradient production (Term IV in the variance budget equations in the formulation of Stull (1988), his Eqs. (4.3.2) and (4.3.3)) rather than by any diabatic sources.

13. Line 420: You ‘speculate that the drivers of convection, i.e. radiative and evaporative cooling, are not efficient in this situation’. What is different about ‘this situation’ that either of these processes should be different? You can evaluate the evaporative cooling and CTEI parameter… is this weaker than for the other case? Certainly the latent and sensible heat fluxes are much smaller in cloud here than for flight 5.

    Radiative cooling is more difficult to assess without direct measurements of the radiative fluxes, but there may be clues available. You mention the availability of the ARM remote sensing data… does that show a higher cloud deck that might reduce the radiative cooling from cloud top? This case does have a slightly thicker cloud and so higher LWC at cloud top… this will slightly modify (sharpen) the LW cooling and SW heating profiles, and maybe shift the relative positions of their peaks in the vertical, changing the balance of heating/cooling.

    Our speculative comment resulted from the observation of relatively small $Q_s$, $Q_l$ and $B$ in the cloud (LEG992). This LEG was performed below the cloud top but not exactly at the interface or in the EIL, so the fluxes do not represent the entrainment of warm and dry air from the FT but rather parcels which were cooled in the cloud top region and descend through the cloud (Gerber et al., 2016).

    Regarding evaporative cooling, the CTEI parameter $\kappa$ (its estimation was given in sec. 3.2) is indeed smaller for the decoupled STBL (0.34) than for the coupled (0.71), yet exceeds the critical value for buoyancy reversal in both cases. This is consistent with the claim of less efficient evaporative cooling in our decoupled stratocumulus.

    Regarding radiative cooling, there is evidence that there was no overlying cloud layer during flight #14. No additional clouds were reported by the scientists onboard the helicopter. There are also no overlying clouds visible in the measurements performed by the Ka-band cloud radar and by the ceilometer at ARM ENA site (see the figures below). Moreover, in the substantial region around the operation area, the satellite products derived from MODIS onboard Aqua (NASA Worldview portal) indicate cloud top temperature in the class of 285-290 $\mathrm{K}$ and cloud top height in the class of 800-1600 $\mathrm{m}$, both consistent with our observations of stratocumulus top.

    Radiative fluxes were measured in the course of the ACORES campaign by the radiometers onboard ACTOS and SMART-HELIOS. However, the data require validation and careful interpretation (e.g. related to the platform inclination and orientation changing during flight maneuvers). This is subject of ongoing work performed by the Atmospheric Radiation research group at the University of Leipzig.

    Following the points raised by the reviewer, we reformulated the argumentation in the manuscript:

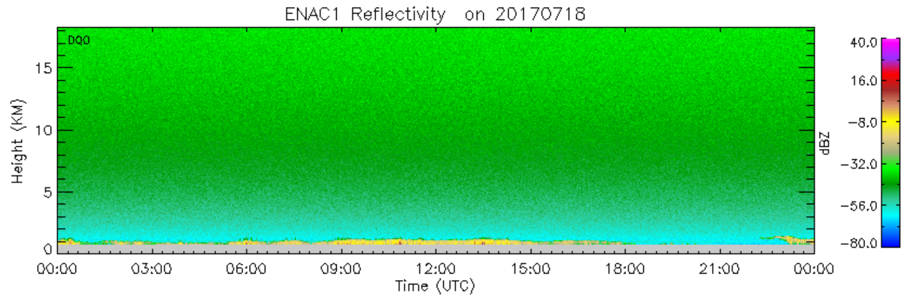


**Figure.** ARM ENA Ka-band Zenith Radar reflectivity. Figure downloaded from ARM Plot Browser (https://plot.dmf.arm.gov/plotbrowser/).

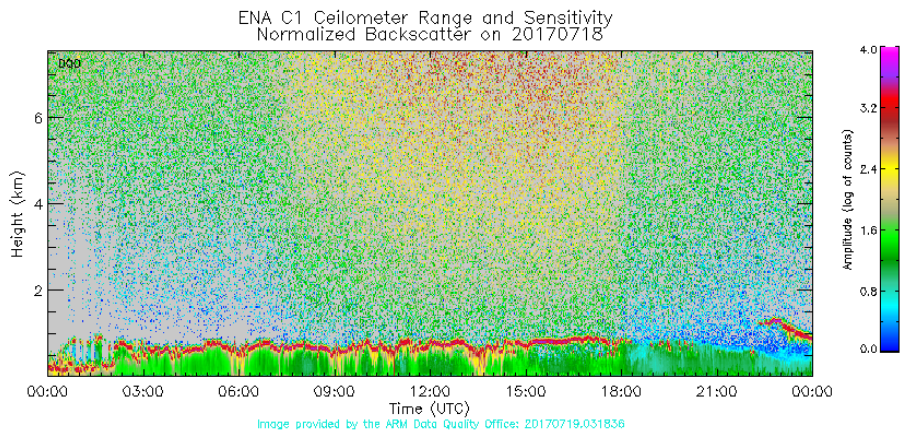


**Figure.** ARM ENA Ceilometer normalized backscatter coefficient. Figure downloaded from ARM Plot Browser (https://plot.dmf.arm.gov/plotbrowser/).

Both sensible and latent heat fluxes observed in the cloud (LEG992) are small, in contrast to the coupled case. Together with rather moderate $B$ in the cloud this suggests that the drivers of convection, i.e. radiative and evaporative cooling, are not as efficient in this situation which might have been one of the reasons why decoupling occurred. Cloud top entrainment instability parameter $\kappa$ (sec. 3.2) is indeed smaller in the decoupled cloud in comparison to the coupled one which implies less efficient evaporative cooling. However, the comparison of radiative cooling effects between the cases requires further investigation.

14. Line 422: ‘moisture delivery from the ocean surface to the cloud might be more difficult in the decoupled STBL’ – yes, it ought to be much more difficult.

    We changed ‘might’ into ‘ought to’.

15. Line 458: is the departure of measurements from theoretical expectations for homogeneity, isotropy and stationarity here a result of evaluating them from slant profiles? You note the horizontal legs are in much better agreement with theory, suggesting the profile results are not truly representative.

The results on small-scale turbulence, including $\epsilon$, $s$ and $p$, should not be compared between PROFs and LEGs in a straightforward way. They are representative for small and large fluid volumes, respectively. According to the refined Kolmogorov hypothesis (Kolmogorov, 1962), due to turbulence intermittency $\epsilon$ distribution depends on the scale on which it is evaluated. This dependence inside clouds was investigated experimentally by Siebert et al. (2010).

In our opinion, the observed discrepancy between PROF-derived and LEG-derived parameters stems from the combination of horizontal inhomogeneity and intermittency of turbulence. In fact, horizontal segments may cover various air volumes differing in turbulence intensity and its properties, e.g. dissipation rate or inertial range scaling. In the region of decaying turbulence, which is likely in the SCL and the SBL of the decoupled boundary layer, there can be even laminar patches. Under such conditions, the scaling of a power spectrum or structure function is dominated by the most intensive portions. In contrast, PROFs are considered to capture local properties. Note that the climb rate of the helicopter is much higher than of a typical research aircraft (c.f. Siebert et al., 2021, sidebar "Profiling with aircraft and helicopter"), so the averaging in horizontal is rather limited in comparison with typical aircraft data.

We commented on this issue in sec. 5.3.:

> In contrast to the PROFs, the LEG-derived exponents stay mostly close to 2/3 or -5/3, accordingly, while the correlations are close to one. We suppose that the observed discrepancy results from the combination of horizontal inhomogeneity and intermittency of turbulence. PROF-derived and LEG-derived parameters should not be directly compared because they represent small and large fluid volumes, respectively. Unfortunately, none of the horizontal segments was performed in the SBL.

16. Line 555: ‘which suggests important contribution of moisture to buoyancy’ – I agree, but this could be evaluated properly. Buoyancy flux (virtual potential temperature flux) can be broken down into the sensible and latent heat contributions and their ratio determined.

We evaluated the terms contributing to virtual potential temperature flux using the approximation valid under dry conditions (e.g De Roode and Duynkerke, 1997):

$$\langle w'\theta_v'\rangle = \langle w'\theta'\rangle + \theta\varepsilon\langle w'q_v'\rangle \tag{1}$$

where $\varepsilon = R_v/R_d - 1$. The results listed in the table below confirm that the moisture term plays a significant role in the lower part of the boundary layer.

The information about this fact was added to sec. 5.2 and sec.6.

> At low levels in the atmosphere (at the surface and in LEG307) the contribution of moisture transport to buoyancy is of the same order as the contribution of heat transport (not shown).

> In the lower part of the STBL (at the surface, in LEG143 and LEG287) the contribution of moisture transport to buoyancy is of the same order as the contribution of heat transport (not shown).

**Table 1.** Contributions to virtual potential temperature flux inside the boundary layer. Left column for each term is a LEG-derived value, right column is the variability among subsegments.

| | Height [m] | $\langle w'\theta' \rangle$ $[10^{-3}\text{mK s}^{-1}]$ | | $\varepsilon\theta\langle w'q'_v \rangle$ $[10^{-3}\text{mK s}^{-1}]$ | |
|---|---|---|---|---|---|
| **Flight #5** | 0 | 9.0 | - | 7.6 | - |
| | 307 | 3.4 | 2.2 | 3.0 | 1.4 |
| | 553 | 6.1 | 5.6 | 0.3 | 1.4 |
| | 819 | 33.1 | 17.8 | 6.5 | 4.8 |
| **Flight #14** | 0 | 5.5 | - | 6.3 | - |
| | 143 | 7.4 | 2.7 | 4.6 | 2.1 |
| | 287 | 1.4 | 3.4 | 2.4 | 1.7 |
| | 448 | 3.1 | 2.3 | 0.4 | 2.7 |
| | 992 | 2.2 | 3.4 | 0.7 | 1.5 |

In both cases, latent heat flux qualitatively resembles the profile of $B$ which is consistent with the considerable contribution of moisture transport to buoyancy in the lower part of the STBL.

**Minor issues (grammar, typos, etc)**

While overall, the manuscript is clear and well written, there are many minor grammatical issues – notably missing definitive articles: '…in the cloud top region…', '…in the inertial subrange…' etc. I have noted all those that jumped out at me below, but I'm sure I've missed more.

We are impressed by the language editing provided by the reviewer. As we are not native speakers, we rely on him and applied all the minor corrections suggested.

1. Line 4: '…in cloud top region' -> '…in the cloud top region'

   Corrected.

2. Line 12: 'in inertial subrange' -> 'in the inertial subrange'

   Corrected.

3. Line 22: 'They occupy…, preferably in the conditions of large-scale subsidence.' – 'preferably' is the wrong word (implies an ideal choice or active preference', 'preferentially' is closer to the meaning required (with greater likelihood)

Changed to ‘preferentially’.

4. Line 28: ‘Primary mechanism…’ -> ‘The primary mechanism…’

   Corrected.

5. Line 29: ‘Additional source of turbulence…’ -> ‘ An additional source of turbulence…’

   Corrected.

6. Line 32: ‘…dependent on the level in which SC is coupled…’ – ‘in’ isn’t the right word here, and the meaning intended isn’t entirely clear, either ‘…dependent on the level at which SC is coupled…’ (if the issue of concern is the altitude at which decoupling occurs) or ‘…dependent on the level to which SC is coupled…’ (if the issue is whether, or how strongly decoupled the BL is).

   Changed to ‘to which’.

7. Line 35: ‘…structure features adiabatic lapse rate (dry below cloud, moist inside), strong capping inversion at the top, near-constant concentration of moist-conserved variables…’ -> ‘…structure features an adiabatic lapse rate (dry below cloud, moist inside), a strong capping inversion at the top, and near-constant concentration of moist-conserved variables…’

   Corrected.

8. Line 40: ‘ Stable or…’ -> ‘A stable or…’

   Corrected.

9. Line 62: ‘…in conventional rationale…’ -> ‘…in the conventional rationale…’

   Corrected.

10. Line 105: ‘…depended on local cloud…’ -> ‘…depended on the local cloud…’

    Corrected.

11. Line 106: ‘usual strategy involved:…’ -> ‘the usual strategy involved…’

    Corrected.

12. Line 109: ‘…and flight pattern…’ -> ‘…and a flight pattern…’

    Corrected.

13. Line 140: ‘Brunt-Vaisala frequency…’ -> ‘The Brunt-Väisälä frequency…’

    Corrected.

14. Line 143: '. . . quantifies vertical gradient. . . ' -> '. . . quantifies the vertical gradient. . . '

    Corrected.

15. Line 166: '. . . as BL mean.' -> '. . . as the BL mean.'

    Corrected.

16. Line 183: '. . . where it features the increase of. . . ' -> '. . . where it features an increase of. . . '

    Corrected.

17. And '. . . analogously, with the decrease of. . . ' -> '. . . analogously, with a decrease of. . . '

    Corrected.

18. Line 184-185: '. . . capped by the layer of. . . ' -> '. . . capped by a layer of. . . '

    Corrected.

19. Line 239: 'Described modification. . . ' -> 'The modification described. . . '

    Corrected.

20. Line 243: '. . . with simple. . . ' -> '. . . with a simple. . . '

    Corrected.

21. Line 244: '. . . from original signal.' -> '. . . from the original signal.'

    Corrected.

22. Line 249: '. . . taking average along LEG. . . ' -> '. . . taking the average along the leg. . . '

    Corrected.

23. Line 252: 'Worth to remember. . . ' -> 'It is worth remembering. . . '

    Corrected.

24. Line 260: '. . . such approach. . . ' -> '. . . such an approach. . . '

    Corrected.

25. Line 266: 'Range of scales. . . ' -> 'The range of scales. . . '

    Corrected.

26. Line 266: '. . . limited by the smaller among spatial resolutions of two multiplied signals. . . ' the word smaller here might be read as implying the smaller scale (ie, higher resolution), suggest -> '. . . limited by the lowest spatial resolution of the two multiplied signals. . . '

    Corrected.

27. Line 267-268: following the previous statement, you note the scales of individual measurements, but it would be helpful to be explicit and state the resulting scale for the final fluxes.

    Sentence added: ‘As a result, $\langle w'\theta' \rangle$, $\langle w'u' \rangle$ and $\langle w'\theta' \rangle$ are resolved down to ∼0.5 $\mathrm{m}$ while $\langle w'q'_v \rangle$ down to ∼1 $\mathrm{m}$.’

28. Line 317: ‘In case of LEGs. . . ’ –> ‘In the case of LEGs. . . ’

    Corrected.

29. Line 338: ‘Similar approach. . . ’ -> ‘A similar approach. . . ’

    Corrected.

30. Line 340: ‘Such value of. . . ’ -> ‘Such a value of. . . ’

    Corrected.

31. Line 345: ‘Integral lengthscale. . . ’ -> ‘The integral length scale. . . ’

    Corrected.

32. Line 347: ‘. . . integral of autocorrelation function. . . ’ -> ‘. . . integral of the autocorrelation function. . . ’

    Corrected.

33. ‘. . . in formal definition. . . ’ -> ‘. . . in the formal definition. . . ’

    Corrected.

34. Line 353: ‘At Taylor microscale. . . ’ -> ‘At the Taylor microscale. . . ’

    Corrected.

35. Line 365-366: The statement ‘Depending on flight segment type, they are illustrated with continuous profiles (PROF) and/or dots with errorbars (LEG).’ Is redundant, delete.

    Ok, removed.

36. Line 372: ‘. . . reaches minimum value. . . ’ -> ‘. . . reaches a minimum value. . . ’

    Corrected.

37. Line 387: ‘. . . resemble typical mixed layer. . . ’ -> ‘. . . resemble a typical mixed layer. . . ’

    Corrected.

38. Line 390: ‘. . . exhibit maximum. . . ’ -> ‘. . . exhibit a maximum. . . ’

    Corrected.

39. Line 398-399: The statement ‘while the shear production at the bottom and at the top of the boundary layer’ is incomplete…needs some statement about the shear production.

    Corrected to ’while the shear production is expected to be significant at the bottom and at the top of the boundary layer’.

40. Lines 395, 402, 406: statements about results are phrased as ‘seems to’, ‘appears to be’ etc. Unless there is real doubt, be definitive…is it as stated or not?

    Corrected. Speculative verbs were removed.

41. Line 495: ‘…immensely stable…’ -> ‘…strongly stable…’ (immensely might be overstating things a bit).

    Corrected.

42. Line 561: ‘…vanishes at the level…’ -> ‘…vanishes at a level…’

    Corrected.

43. Line 568-569: ‘Vertical velocity variance suggests the profile somewhat different than the convective similarity scaling’ -> ‘The vertical velocity variance suggests a profile somewhat different than the convective similarity scaling’

    Corrected.

44. Line 659: ‘Main processes…’ -> ‘The main processes…’

    Corrected.

45. Line 669: ‘imortant’ -. ‘important’

    Corrected.

46. Line 670: ‘relevant systematical…’ – ‘…relevant systematic…’

    Corrected.

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