Authors’ Response to the Anonymous Referee #2

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We are grateful to the Referee #2 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.

General comments

1. The paper provides interesting insights into the stratification and the turbulent properties of coupled (CP) and decoupled (DCP) marine stratocumulus-topped boundary layers. It would be interesting to have additional CP/DCP cases to investigate whether the same pattern is also observed in other cases. Are you planning to extend the analysis to more cases? It would be in particular interesting to have more data in the stratocumulus layer.

   We are grateful for the comment, which partly coincides with the first comment from the reviewer #4 (Ian Brooks). We will consider extending this analysis to other available cases, though, as usual this will depend on the available manpower. For this manuscript, we have chosen to focus on details rather than an extended data set. However, we also believe that extending similar analyses to more flights will make the results more robust.

   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.

2. In Sect. 2.2, you introduce the different instruments that have been used in the study. I would like to see further discussion regarding the uncertainties of the measurements. Currently, uncertainties are not discussed and the error bars only include the variability of the data. This makes it difficult to assess the conditions inside the different sublayers in the CP and DCP case (e.g., add error bars in Table A1 and A2).

   In our study, we present a great variety of turbulence parameters. We suppose it is impossible to apply one universal and rigorous approach of error treatment for such different variables. For this reason, we decided to report subsegment variability because it can be evaluated regardless of the details of a particular derivation method. On the other hand, we agree with the reviewers that the issue of uncertainties was not comprehensively addressed. In order to improve this, we complemented the manuscript with a few additional paragraphs discussing the aspects which we find the most relevant for the derived turbulence parameters.

We described the capabilities of the instruments onboard ACTOS in sec. 2.2.

> The standard deviations due to uncorrelated noise for sonic measurements are 0.02 $\mathrm{m\,s^{-1}}$ for wind and 0.02 K for virtual temperature (Siebert and Muschinski, 2001). The PT100 was calibrated prior to the campaign in a thermostated water tank using the Greisinger GMH 3750 reference thermometer which provides accuracy better than 0.05 K. The UFT was calibrated for each flight separately against the PT100. For the UFT records, the standard deviation due to uncorrelated noise is 4 mK (Siebert et al., 2003). The hygrometer provides q_v with a noise floor of about 0.005 $\mathrm{g\,kg^{-1}}$. This instrument was verified to agree well with a few hygrometers of different types and operate satisfactorily on the helicopter-towed system Helipod by Lampert et al. (2018). The PVM-100A measures q_l with the accuracy of 5 % and its noise floor was estimated by Siebert et al. (2003) for about 0.001 $\mathrm{g\,kg^{-1}}$. The exact sensitivity depends to some extent on droplet size distribution, see Wendisch et al. (2002) for details. For a more general discussion of the instrumentation on the ACTOS platform see Siebert et al. (2006a).

We discussed the sampling errors (systematic and random) for turbulent moments (variances, TKE, $\langle w'^3 \rangle$) estimated according to Lenschow et al. (1993, 1994) in sec. 4.1. The detailed procedure with all the specific values is delineated in our response to the Anonymous Reviewer #1.

> The accuracy of the results is severely limited by the length of the LEGs. Based on the methods of Lenschow et al. (1994), in the boundary layer the variances are subject to the systematic sampling error of about 5 % and the random sampling error of about 20 %. In the case of $\langle w'^3 \rangle$, those errors are accordingly larger (order of 10 % and 100 %, respectively, unless $\langle w'^3 \rangle$ is not very close to zero). Importantly, in the plots we provide the variability among subsegments which was found to be of the same order as the total sampling error, in most cases larger than it.

Similarly, we discussed the sampling errors for turbulent fluxes in sec. 4.2. Again, please see the response given to the Anonymous Reviewer #1 to find the tables presenting all the individual errors.

> Similarly to variances, the accuracy of the fluxes obtained with the method of eddy correlation is limited by the length of the LEGs. In the boundary layer, the systematic sampling error was estimated for about 5-10 % while the random sampling error for about 50 % (Lenschow et al., 1994), unless the flux does not vanish. The subsegment variability (marked with errorbars in the plots) is in most cases larger than the total sampling error.

We estimated the uncertainties of the derived dissipation rates and SFC/PSD slopes due to random errors in sec. 4.3.3. In addition, we proposed alternative methods of a rather qualitative assessment of the reliability of the results.

In order to roughly estimate the uncertainties of the results, we used the random errors of the fitted parameters (computed with a standard method from least-squares fit residuals). The random error of 'instantaneous' (calculated in 2 s windows and serving for the derivation of the profiles) dissipation rate equals $\sim$50 % in the boundary layer and $\sim$150 % in the FT. The error of the LEG-derived ϵ is $\sim$30 % for longitudinal component and $\sim$15 % for vertical component in the boundary layer while $\sim$150 % for both components in the FT. The random error of the fitted slopes is $\sim$0.04 for s and $\sim$0.16 for p corresponding to the 'instantaneous' estimations while $\sim$0.02 in the case of both LEG-derived slopes. Notwithstanding, the given values represent the uncertainties due to the random errors of the fit only. The reliability of the derived dissipation rates can be also assessed by comparing the results of the two derivation methods, by comparing the fitted SFC and PSD slopes with their theoretical values or using the deviation of the computed correlation coefficients from unity.

The uncertainties of further quantities derived from dissipation rate can be estimated by the method of error propagation. Additionally, we referred to the previous works to argue that the analysis of spectral anisotropy (sec. 4.4 and 5.4) is justified taking into account the quality of our data. Our data is sufficient for the analysis of the inertial range anisotropy as Siebert and Muschinski (2001) demonstrated that the spectra of velocity fluctuations measured with an earlier version of our ultrasonic anemometer-thermometer in a considerably turbulent environment follow closely the expected 5/3 power law, a flattening is observed only at frequencies larger than 30 Hz and the ratio of the transverse and longitudinal spectra equals 4/3, as predicted for isotropic turbulence.

3. The paper contains many abbreviations (e.g., for the different sublayers). I understand that it makes sense to introduce these abbreviations, as they are frequently used in the paper. However, for the readers that are not familiar with these abbreviations, it can be hard to remember the definition of the different acronyms and to follow the text. Please check if all the abbreviations are necessary. For example, "ENA" and "CTEI" are only used 2-3 times and could be removed. Furthermore, I would suggest including the abbreviations of the different sublayers in the figures, in order to make it easier for the reader to identify them (see specific comments).

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.

The abbreviations denoting different sublayers were added to Figs. 5, 6, 9, 10, 11, 12, 13, 14, 15, 16, 19, 20 as suggested.

Specific comments

4. Page 5, caption Fig. 1 and caption Fig.3: Please add date and time of satellite image.

   The captions were corrected as suggested. Fig. 1. shows the imagery acquired on 8 July 2017 at 15:45 UTC, Fig. 3. on 18 July 2017 at 14:43 UTC.

5. Page 5, Fig. 2 and Fig. 4: In Fig. 2 and Fig. 4 you show the time series of the ACTOS altitude. I think it would be beneficial to include more information regarding the cloud/BL structure. E.g., At what altitude is the cloud top/cloud base? You could indicated the different sublayers on the right side of the plot. Furthermore, you often refer to the different profiles (PROFs 1-5) and legs (LEGs 1-5) throughout the paper. You could consider adding the labels of the profiles and legs on top of the plot. In addition, the line style of the profiles is not evident in the figure due to the low contrast between the black line in the background and the black dotted line. I would suggest to remove the black line in the background or to change the color to get a better contrast.

   The figures were modified according to the suggestions. The labels denoting PROFs and LEGs were added at the top. The ordering of LEGs was changes into LEGX where X stands for mean altitude (m a.s.l.), following the request of another reviewer. 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. Instead of sublayer labels, the individual penetrations (determined manually) 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.

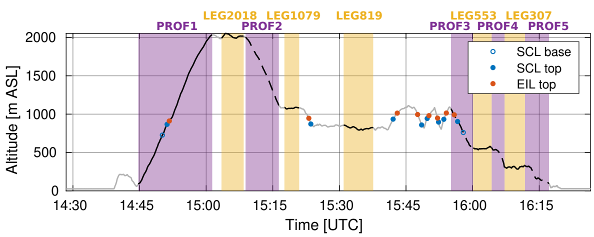

Figure 2 corrected. ACTOS altitude in flight #5 with marked selected profiles and horizontal legs. PROFs are ordered chronologically, LEGs are ordered according to their altitude. Line styles of the PROFs are consistent with the figures in following sections Altitude ranges corresponding to PROF2-PROF5 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.

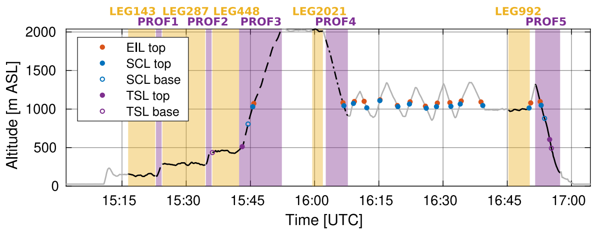

Figure 4 corrected. As in Fig. 2 but for flight #14. Line styles of the PROFs are consistent with the figures in following sections. PROF1-PROF3 are all marked with dotted lines because their altitude ranges do not overlap.

6. Page 6, equation 1: You defined ‘ql’ as the liquid water content on page 4, line 103. The liquid water content is usually defined as mass of liquid water per volume of air (i.e. g m -3). However, in equation 1 the liquid water mixing ratio (i.e. mass of liquid water per unit mass of air) should be used and not the liquid water content (see Betts 1973 or the following link: https://glossary.ametsoc.org/wiki/Liquid_water_potential_temperature). Please review your definition of ‘ql’ in the manuscript.

   In our calculations and throughout the manuscript q_l denotes liquid water mass fraction, i.e. mass of liquid water in a unit mass of moist liquid-ladden air. Its units are $\mathrm{g\,kg^{-1}}$. It is consistent with Eq. (14) of Betts (1973) and with $q_t = q_v + q_l$ being a conservative quantity. Such definition is related to liquid water content (mass of liquid water per unit volume of air) ρ_l and to liquid water mixing ratio (mass of liquid water per unit mass of dry air) r_l as the following:

   $$q_l = \frac{\rho_l}{\rho_d + \rho_v + \rho_l} = \frac{r_l}{1 + r_v + r_l}, \quad r_l = \frac{\rho_l}{\rho_d} \tag{1}$$

   where ρ_d is density of dry air, ρ_v is density of water vapor and r_v is water vapor mixing ratio. We corrected the erroneous definition in the text.

   > ... liquid water mass fraction q_l determined with the Particle Volume Meter ...

7. Page 7, line 150: According to J11, ‘qt’ should be the total water mixing ratio, which is defined by the sum of the liquid water mixing ratio and the water vapor mixing ratio (see also comment 6). Please review your definition of ‘qt’ in the manuscript.

   In our calculations and throughout the manuscript q_t denotes total water mass fraction, i.e. the total mass of liquid water and water vapor per unit mass of moist liquid-ladden air (see also the answer to comment 6 above):

   $$q_t = q_v + q_l = \frac{\rho_v + \rho_l}{\rho_d + \rho_v + \rho_l} = \frac{r_t}{1 + r_t}, \quad r_t = r_v + r_l, \quad r_v = \frac{\rho_v}{\rho_d} \tag{2}$$

   where q_v is water vapor mass fraction (specific humidity), q_l is liquid water mass fraction, ρ_d is density of dry air, ρ_v is density of water vapor, ρ_l is liquid water content. r_v is water vapor mixing ratio, r_l is liquid water mixing ratio and r_t is total water mixing ratio. Insofar, we indeed used slightly different criterion than J11 who applied total water mixing ratio r_t instead of our total water mass fraction q_t. This difference do not affect the conclusions reached with the use of the criterion because approximately $q_t \approx r_t$. This controversy was briefly explained in the text.

> The first criterion of Jones et al. (2011) involves the differences of θ_l and total water mixing ratio between the uppermost and the lowermost quarters of the boundary layer (instead of the latter quantity, we used our total water mass fraction $q_t = q_l + q_v$ which does not influence the conclusions because those two measures are approximately equal).

In contrast to the criterion of Jones et al. (2011), in the one of Yin and Albrecht (2000) we did use the water vapor mixing ratio r_v following those authors literally because there is derivative of this quantity involved (i.e. in general, under some conditions small discrepancies might affect the result).

8. Page 9, Fig. 5 and Fig. 6: As mentioned already in the general comment section, it is hard to remember the abbreviations of the different sublayers. In order to make it easier for the reader to follow and identify the different sublayers, I would suggest adding the abbreviations of the sublayers (color shaded areas) on the right side of the subplots (for all figures of this type; i.e. Fig. 5, 6, 9-16, 19-20). Furthermore, I would plot the lines on top of the shaded area to avoid any change in the line color (for example for LCL, qv).

   The acronyms denoting different sublayers were added to Figs. 5, 6, 9, 10, 11, 12, 13, 14, 15, 16, 19, 20 as suggested. Moreover, their full names were given in the tables in the appendix. We appreciate recognizing the issue with colors while plotting the shaded areas. We changed the order of drawing in our routine as suggested.

9. Page 9, line 207: So are both the upper and the lower BL portion internally mixed? If yes, you could change the structure of the sentence as follows: "This suggests that both the upper and lower BL portion are internally mixed."

   Yes, they are. We corrected the sentence as suggested.

10. Page 14, line 113: You applied a moving window of 2 s to the profiles. How was the moving window of 2 s determined? Did you conduct sensitivity tests with different time windows?

    In a few previous studies which utilized the same type of data, the window of 1 s was proven to operate satisfactorily while deriving the instantaneous dissipation rate (e.g. Siebert et al., 2006b; Katzwinkel et al., 2012). The choice of such window length by Siebert et al. (2006b) followed from their own sensitivity tests and the works of Muschinski et al. (2004); Frehlich et al. (2004). Because we determine not only the dissipation rate but also the slope of the SFC or the PSD in the inertial range, we decided to increase the window to 2 s so that the linear fit covers the considerable portion of the inertial range (0.4-40 m) and the sufficient number of logarithmically equidistant resampled points (eight per decade, see sec. 4.3 of the manuscript). We did not conduct additional strict sensitivity tests within the present study. The appropriate explanatory comment was added to the text.

    > Our approach follows earlier studies which determined the instantaneous dissipation rate utilizing the same type of data as ours (Siebert et al., 2006b; Katzwinkel et al., 2012). Siebert et al. (2006b) have chosen

the window of 1 s based on their sensitivity tests and the arguments provided by Frehlich et al. (2004) and Muschinski et al. (2004). Because we derive not only ϵ but also the slopes and correlations, we increased the window to 2 s so that the linear fit covers considerable portion of the inertial range and the sufficient number of logarithmically equidistant resampled points (see sec. 4.3.1).

11. Page 20, Fig.13: Why is there such a large discrepancy between some of the PROFs and LEGs properties (e.g., s_u, R_u, R_w) in the FTL?

    First, the free troposphere is not expected to be turbulent. Under such conditions, the assumptions of Kolmogorov theory exploited in both the methods of dissipation rate derivation are far from being fulfilled. Structure functions and power spectra are not guaranteed to follow any specific scaling. The inertial range cannot be defined.

    Second, even in the STBL the results on small-scale turbulence, including ϵ, 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. 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"). Also, horizontal segments may cover various air volumes differing in turbulence intensity and its properties, e.g. dissipation rate or inertial range scaling. According to the refined Kolmogorov hypothesis (Kolmogorov, 1962), due to turbulence intermittency ϵ distribution depends on the scale on which it is evaluated. This dependence inside clouds was investigated experimentally by Siebert et al. (2010).

    We added a comment about 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.

12. Page 24, Fig. 17 and Fig. 18: I would use the same scale on the y-axis for Fig. 17 and Fig. 18 for better comparison between the coupled and decoupled case. Furthermore, I would suggest adding the sublayer in brackets next to the altitude.

    The figures were modified according to the suggestions.

13. Page 25, line 506: “Lengthscales” should be changed to “Length scales” throughout the paper.

    The text was corrected accordingly.

14. Page 25, line 512 and line 524: One of the “λ_u” in the ratio should be replaced by “λ_w”.

    Obviously. We are sorry for the typo.

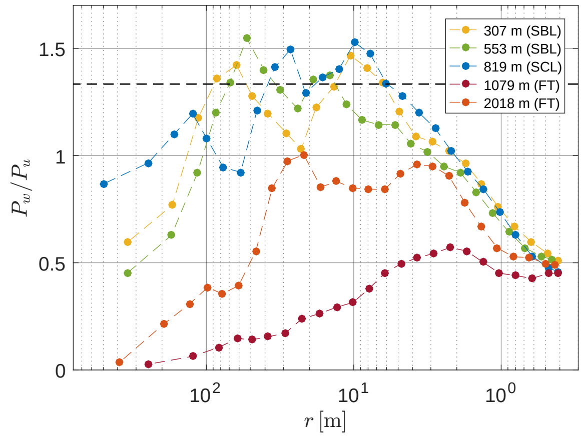

Figure 17 corrected. Spectral anisotropy ratio in the coupled STBL (flight #5). The horizontal dotted line denotes the 4/3 level expected for isotropy in inertial range.

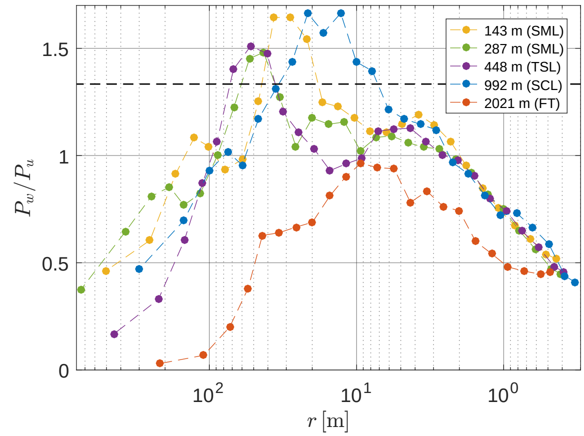

Figure 18 corrected. As in Fig. 17 but for the decoupled STBL (flight #14).

15. Page 32, line 669: Change “imortant” to “important”

    Corrected.

16. Page 32, line 670: Change “proprties” to “properties”

    Corrected.

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