Authors’ Response to the Anonymous Referee #1

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We are grateful to the Referee #1 for the 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 manuscript describes the ratio of transverse to longitudinal structure functions and power spectra in the inertial subrange, as measured by aircraft, and how the derived ratios and scaling exponents deviate from the classical theory of homogeneous and isotropic turbulence. Derived ratios are closer to 3/4 than to the theoretical 4/3 value. The authors conclude that the derived ratios and exponents mainly depend on how the velocity components were measured on the aircraft compared to a minor influence of ABL regimes and across different field experiments.

General remarks

The manuscript provides a profound and insightful analysis of the spectra and structure functions, with a rigorous and detailed description of methods used to derive the ratios and scaling exponents, including uncertainty analysis and sensitivity to the fitting range of the spectra. I only have a few suggestions for improving clarity and better contextualizing the work within the existing literature.

We followed the suggestions, expanded the literature review in the introduction and extended the discussion on potential solutions.

Specific comments

1. It would be helpful to the reader if the abstract could clearly state the overall aim of the manuscript before outlining the methodological details. Is it to document the derived ratios and exponents and their deviation from theoretical values?

   We added a relevant sentence to the abstract. The goal is to document the statistics of the ratios and exponents derived from aircraft observations, quantify their departures from theoretical predictions and point out the differences among the aircraft.

2. I think the introduction could be re-structured to lead from a broad scope to the specific purpose of this paper. For example, the second paragraph is a rather technical explanation of turbulence measurements on aircraft, which I find hard to contextualize when reading for the first time.

We reordered the content in the introduction so that it starts with theoretical background, then describes the previous experimental studies and closes with the overview of our study.

3. The introduction (L. 63ff) mentions a few studies of isotropy from ground-based measurements. It would be interesting to mention what these studies find, since they exclude aircraft-specific errors. Also, are there studies available from tall towers (several hundreds of meters high), reaching above the surface layer?

   We described the relevant findings from the ground-base measurements in the surface layer (Kaimal et al., 1972; Katul et al., 1995, 1997; Siebert and Muschinski, 2001; Chamecki and Dias, 2004). Moreover, to our literature review we added studies involving tethered balloon (Kaimal et al., 1972) and tall tower measurements (Kaimal et al., 1982). Those experiment reached above the surface layer into the mixed convective layer. Besides, we corrected the information given about the work by Siebert et al. (2006) who performed measurements in shallow cumulus clouds. In fact, their ACTOS platform was then carried by a tethered balloon, not a helicopter. A helicopter was indeed used for the same platform but in subsequent studies by the same group, e.g. in Nowak et al. (2021) whose results on anisotropy in the stratocumulus-topped ABL were also delineated in more detail in the revised introduction.

4. L. 72: Could you explain what the upwash distortion is? (Since it is mentioned several times.)

   Upwash flow distortion appears forward of the aircraft due to the air being deflected above and below the wing when approaching an airfoil. We added a short explanation to the introduction. In the revised manuscript we used the term ’upstream flow distortion’ instead for better comprehension.

5. L. 134: Could you still briefly mention TAS and sampling frequency for the ATR?

   The TAS and sampling frequency for the ATR is the same as for the C130. We clarified this in the text.

6. It would be helpful to add an explanation about the applicability of the findings to the undisturbed ABL (and reference coordinate system along the mean wind), since the longitudinal and transversal components refer to the aircraft-referenced coordinate system (L. 136ff), including aircraft heading and pitch if I understand correctly.

   We wrote an additional paragraph in the theoretical part of the introduction to explain how frozen flow approximation is applied and what directions are longitudinal/transverse in two typical experimental configurations: rapidly moving aircraft and fixed ground-based mast. Together with the clarified definition of the coordinate system in sec. 2.2., this should help readers to understand the conventions we use.

   We are not entirely sure what is meant by undisturbed ABL. If the lack of detectable mean wind, then with a fixed point measurement (e.g. at a mast) one cannot obtain multi-point statistics like structure functions or power spectra. Actually, this is a case of extreme invalidity of Taylor’s hypothesis because the turbulence intensity, i.e. the ratio of turbulence velocity scale to mean wind magnitude, becomes infinite. On the other hand, no mean wind does not pose a problem for aircraft measurements because the velocity of the aircraft with respect to air is relevant here.

7. L. 139: Add the reference coordinate system.

We specified the reference coordinate system by referring to an extra explanation of longitudinal/transverse directions given in the introduction.

8. L. 142: “Note that both vertical and lateral wind components are considered transverse.” It might be helpful to mention this earlier, since both terms are used multiple times before.

   It was mentioned in the extra paragraph about frozen flow approximation and longitudinal/transverse directions added to the revised introduction.

9. L. 210: Does the 3/4 value have a special meaning, or is it just by coincidence the closest value for the ratios?

   No, it is coincidence. We removed the 3/4 lines from the plots.

10. Looking at Fig.3-Fig.5, one could interpret that filled symbols show less scatter than open circles. Could one conclusion of the study be that wind-parallel flight legs provide statistically more robust results than wind-perpendicular flight legs?

    Discerning whether wind-parallel or wind-perpendicular flight legs provide statistically more robust results is not straightforward. In general, it may depend on the dominant convection pattern as some of the structures are expected to align with the mean wind direction and some are not.

    The specific difference in the apparent scatter in our Figs. 3-5 is, to a large extent, due to the different number of legs in each category. For example, during EUREC4A most legs were wind-perpendicular. Therefore, we added information about the numbers of wind-parallel and wind-perpendicular legs to Table 1. Please note that it already involves the change in segment classification according to levels for POST which we described at the end of this document in the section "Other changes".

    We suppose that "statistical robustness" in the situation of different number of elements can be interpreted quantitatively as smaller standard error of the mean $s_{\overline{x}} = \sigma_x/\sqrt{N}$ where σ_x is standard deviation and N is the number of x samples. The results on $s_{\overline{x}}$ given in the Table R1 below are ambiguous: depending on the variable and level the values are smaller either for the wind-parallel or wind-perpendicular segments.

11. L. 289ff: To investigate aircraft-specific errors leading to the deviation from theoretical values: What about a comparison to tall tower measurements or airborne Sonic anemometer measurements? Would the spatial distance between aircraft and reference measurement hinder a direct comparison?

    We agree that a comparison to tall tower measurements will help to investigate aircraft-specific errors and eventually find physical explanations for our results. Similar strategy was applied by Kaimal et al. (1982) but over gently rolling terrain and with a different measurement technique used on the aircraft. Unfortunately, the tower of Boulder Atmospheric Observatory no longer exists. Ideally, a tower should be on a horizontally homogeneous surface and reasonably high to offer a meaningful comparison with aircraft. We added a paragraph proposing such an experiment to the discussion section.

Table R1. Standard error of the mean $s_{\overline{x}} = \sigma_x/\sqrt{N}$ for the variables given in the head row, separately for characteristic levels and flight segment orientation with respect to the mean wind.

Level	**Wind**	**N**	D_v/D_u	P_v/P_u	D_w/D_u	P_w/P_u	s_u	s_v	s_w	p_u	p_v	p_w
ATR-EUREC4A												
cloud-base	$\perp$	116	0.008	0.007	0.010	0.011	0.004	0.006	0.006	0.011	0.011	0.008
top-subcloud	$\parallel$	11	0.008	0.009	0.022	0.027	0.009	0.009	0.018	0.036	0.032	0.019
top-subcloud	$\perp$	9	0.020	0.014	0.041	0.036	0.012	0.020	0.026	0.014	0.017	0.026
mid-subcloud	$\parallel$	9	0.020	0.018	0.026	0.021	0.016	0.008	0.010	0.018	0.017	0.016
mid-subcloud	$\perp$	10	0.016	0.015	0.024	0.012	0.010	0.011	0.020	0.016	0.018	0.019
near-surface	$\parallel$	5	0.010	0.015	0.028	0.023	0.018	0.027	0.025	0.022	0.045	0.031
near-surface	$\perp$	5	0.023	0.018	0.013	0.024	0.018	0.011	0.014	0.049	0.022	0.035
TO-POST												
cloud-top	$\parallel$	9	0.058	0.080	0.097	0.127	0.015	0.056	0.036	0.017	0.055	0.055
cloud-top	$\perp$	32	0.025	0.031	0.039	0.051	0.009	0.016	0.018	0.015	0.021	0.021
cloud-base	$\parallel$	11	0.046	0.062	0.048	0.065	0.016	0.023	0.020	0.046	0.047	0.043
cloud-base	$\perp$	11	0.064	0.074	0.068	0.082	0.015	0.054	0.034	0.028	0.024	0.049
sub-cloud	$\parallel$	4	0.020	0.030	0.060	0.101	0.020	0.026	0.056	0.048	0.026	0.107
sub-cloud	$\perp$	38	0.013	0.014	0.020	0.021	0.009	0.010	0.010	0.019	0.010	0.015
near-surface	$\parallel$	4	0.011	0.018	0.029	0.013	0.046	0.045	0.028	0.056	0.109	0.189
near-surface	$\perp$	45	0.010	0.012	0.013	0.014	0.007	0.010	0.008	0.019	0.014	0.018

Concerning a comparison against airborne sonic anemometer, we expect it would be more difficult to discern the influence of platform-specific aerodynamic issues. A sonic anemometer was flown onboard ACTOS platform carried by a helicopter, e.g. in Nowak et al. (2021). They reported that P_w/P_u strongly decreases already for $\lambda < 5$ m while the sonic path is 15 cm. The reason for such a behaviour at those wavelengths was not thoroughly explained.

12. Appendix C: Does the conclusion of appendix C mean that the insensitivity to the included scales extends beyond the inertial subrange, meaning that even the larger scales are close to anisotropy in the ABL? It would be interesting to see if measured anisotropy in the inertial subrange is related to anisotropy at larger scales. Were strongly anisotropic cases (at larger scales) considered in the analysis?

    We did not investigate large-scale anisotropy and, in particular, did not condition the data on the properties related to larger scales. Therefore, strongly anisotropic cases at larger scales could be included in the analysis. We suppose the range of scales considered in Appendix C does not extend far beyond the inertial subrange if at all. The largest considered scales are 2.8 L, which for L ~200 m (c.f. Table 1) means ~560 m. This is typically smaller than the ABL depth z_i while

Kaimal et al. (1976) and Kaimal et al. (1982) found that inside the mixed layer the peak of the power spectra is typically at $\lambda \sim 1.5 z_i$. Please also note that our particular method of computing L is based on the e-decay of the autocorrelation function. It provides rather lower bound on the estimation of integral length scale as the e-crossing is always at smaller scale than the zero-crossing.

Following the suggestions by Referee #2, we expanded the analysis to scale-by-scale transverse-to-anisotropy ratios. Those additional results clearly indicate that anisotropy changes across scales. However, the largest of the considered scales seem to be closer to isotropy than the smallest ones, which we cannot explain with physical mechanisms relevant to the ABL.

In general, in contrast to the inertial subrange large scales are expected to exhibit anisotropy due to vertical confinement, imposed by the surface and stable inversion layer at ABL top, and due to inherent anisotropy of the turbulence generation mechanisms such as heat fluxes or wind shear. Convection in the ABL often organizes into low-aspect-ratio structures resembling cells, rolls etc. It would be indeed interesting to study whether and how the particular organization of convection affects the anisotropy of turbulence in the inertial subrange but, unfortunately, it is beyond the scope of our study.

Technical comments

1. The figure labels are very small and hard to read, especially Fig. 1, 2, B1 and C1. Would it be possible to plot fewer cases, but increase label sizes?

   We increased font size and restructured the figures. The number of panels in Figs. 1 and 2 was decreased to 4 and their size increased. The layout of Fig. B1 was changed from 3x4 into 4x3, which allowed for increasing the size of panels, and its style was improved to be more clear. In Fig. C1, we zoomed in to present relevant ranges.

2. L. 22: Maybe rather use “turbulence strength” than “turbulence intensity” to not confuse it with the actual variable TI.

   Sure. Corrected.

3. L. 59: boundaries of what?

   The boundaries of the domain for a turbulent fluid, e.g. sufficiently far from the surface and top of the ABL. Corrected.

References

Chamecki, M. and Dias, N. L.: The local isotropy hypothesis and the turbulent kinetic energy dissipation rate in the atmospheric surface layer, Quarterly Journal of the Royal Meteorological Society, 130, 2733–2752, https://doi.org/10.1256/QJ.03.155, 2004.

Kaimal, J. C., Wyngaard, J. C., Izumi, Y., and Coté, O. R.: Spectral characteristics of surface-layer turbulence, Quarterly Journal of the Royal Meteorological Society, 98, 563–589, https://doi.org/10.1002/QJ.49709841707, 1972.

Kaimal, J. C., Wyngaard, J. C., Haugen, D. A., Coté, O. R., Izumi, Y., Caughey, S. J., and Readings, C. J.: Turbulence Structure in the Convective Boundary Layer, Journal of Atmospheric Sciences, 33, 2152–2169, https://doi.org/10.1175/1520-0469(1976)033<2152:TSITCB>2.0.CO;2, 1976.

Kaimal, J. C., Eversole, R. A., Lenschow, D. H., Stankov, B. B., Kahn, P. H., and Businger, J. A.: Spectral Characteristics of the Convective Boundary Layer Over Uneven Terrain, Journal of Atmospheric Sciences, 39, 1098–1114, https://doi.org/10.1175/1520-0469(1982)039<1098:SCOTCB>2.0.CO;2, 1982.

Katul, G., Hsieh, C. I., and Sigmon, J.: Energy-inertial scale interactions for velocity and temperature in the unstable atmospheric surface layer, Boundary-Layer Meteorology, 82, 49–80, https://doi.org/10.1023/A:1000178707511, 1997.

Katul, G. G., Parlange, M. B., Albertson, J. D., and Chu, C. R.: Local isotropy and anisotropy in the sheared and heated atmospheric surface layer, Boundary-Layer Meteorology, 72, 123–148, https://doi.org/10.1007/BF00712392, 1995.

Nowak, J. L., Siebert, H., Szodry, K. E., and Malinowski, S. P.: Coupled and decoupled stratocumulus-topped boundary layers: Turbulence properties, Atmospheric Chemistry and Physics, 21, 10 965–10 991, https://doi.org/10.5194/ACP-21-10965-2021, 2021.

Siebert, H. and Muschinski, A.: Relevance of a tuning-fork effect for temperature measurements with the Gill solent HS ultrasonic anemometer-thermometer, Journal of Atmospheric and Oceanic Technology, 18, 1367–1376, https://doi.org/10.1175/1520-0426(2001)018<1367:ROATFE>2.0.CO;2, 2001.

Siebert, H., Lehmann, K., and Wendisch, M.: Observations of small-scale turbulence and energy dissipation rates in the cloudy boundary layer, Journal of the Atmospheric Sciences, 63, 1451–1466, https://doi.org/10.1175/JAS3687.1, 2006.