Authors’ Response to the Anonymous Referee #2

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We are grateful to the Referee #2 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.

General comments

This manuscript discusses an inherently difficult problem, local isotropy, in relation to aircraft turbulence measurements. It calls attention to the fact that the predicted 4/3 ratio of transverse to longitudinal velocity component spectra and structure functions in the inertial subrange is not observed in many such flights. Because isotropy is related to the accepted values for the Kolmogorov constant(s) in one-dimensional spectra, this is an important issue when one wants to estimate the rate ε of dissipation of turbulence kinetic energy.

A related issue of the slope of the power spectra and structure functions is also investigated, and a large scatter around the predicted exponents is found.

The manuscript raises awareness to the problem, but does not bring a solution or new insights. This does not prevent it from being timely and deserving of publication. My comments therefore should be taken by the authors not as obligatory changes that need to be made to the manuscript, but rather as an interested dialogue about a few facets of a very difficult question.

First, I would like to mention that balloon measurements appear to agree more closely to isotropy; see Siebert et al. [2006].

True. We cited Siebert et al. (2006) in the introduction but mistakenly wrote that their ACTOS platform was carried by a helicopter instead of tethered balloon. A helicopter was indeed used for the same platform but in many subsequent studies by the same group, e.g. Nowak et al. (2021). In the revised introduction we reviewed in more details the tethered balloon measurements of Siebert et al. (2006) and Kaimal et al. (1976). See also our response to your specific comment below

Secondly, numerical analyses may bring insightful results: Akinlabi et al. [2019] obtained results for the P_T/P_L ratio larger than 4/3 from DNS (contrary to the current manuscript’s results). The authors may find their discussion of physical causes of anisotropy in the ABL useful.

Thanks for suggesting this study. We discussed the possible physical causes for turbulence anisotropy in sec. 5. However, we consider them unlikely to explain our results. See also our response to your specific comment below.

Finally, LES of the flow around the sensor has produced some very useful results regarding flow distortion in the case of sonic anemometers: see Hug et al. [2017]; maybe something similar could be proposed as a future study regarding aircraft measurements?

We agree that numerical modeling can be really useful in quantifying the influence of flow distortion on measurements; in particular in the situations where no laboratory or wind tunnel characterization is possible as is the case for aircraft fuselage. However, the velocities relevant for aircraft are much larger than mean wind in the ABL. Therefore, the common assumption of incompressibility is no longer valid and an adequate model needs to account for it. Definitely, simulations of the flow for the case of a 5-hole probe on the aircraft nose would be welcomed and interesting contribution but are beyond the scope of this study. We added a remark about potential benefits of numerical modeling to sec. 5.

Specific comments

Fitting of power laws in figures 1 and 2 may be a little deceiving. Compensated spectra often display a concave curve, rather than a flat (horizontal) plateau in the assumed range of frequencies associated with the inertial subrange. Maybe you can discern further details about the departs from —5/3 and 2/3 by plotting, for example, $k^{5/3}P(k)$ versus k? As an example, see the figure below from Akinlabi et al. [2019].

We plotted and inspected the compensated statistics as suggested, see them below for the example segments included in Figs. 1 and 2. They show our choice of fitting range is reasonable although there is non-zero slope throughout the range in some cases. Please note that this choice needs to be practical enough to involve sufficient number of data points in the fit and universal enough to be applicable to various segments. Also, for our analysis the fitting range needs to be the same for all three velocity components to obtain meaningful ratios in the next step.

In the revised manuscript, we decided to keep the non-compensated statistics in Figs. 1 and 2 so that in the very first figures of our paper the data is presented in a simple form common for experimental works. Nevertheless, we improved their style, drew reference 2/3 or -5/3 scalings and the extent of the fitting range. One compensated plot was used in a new Fig. 3, which is intended to illustrate the computations of scale-by-scale transverse-to-longitudinal ratios.

Worth to mention, Eqs. (5) and (6) involve only single free parameter and our method of fitting them to non-compensated data is numerically equivalent to taking the mean of compensated data within the selected range. Therefore, the choice between non-compensated and compensated does not affect those results.

Another rather interesting plot would be P_w/P_u versus k. This would allow to detect if at least the ratio is increasing with k, which would be indicative that local isotropy is being approached at higher (unresolved) frequencies. As usual, care has to be taken regarding noise, aliasing, and other high-frequency effects. In this regard, see next comment.

We extended our analysis to consider the composite scale-by-scale transverse-to-longitudinal ratios. Please see the revised manuscript. However, the observed tendency is opposite, i.e. the ratios decrease and increasingly depart from 4/3 with decreas-

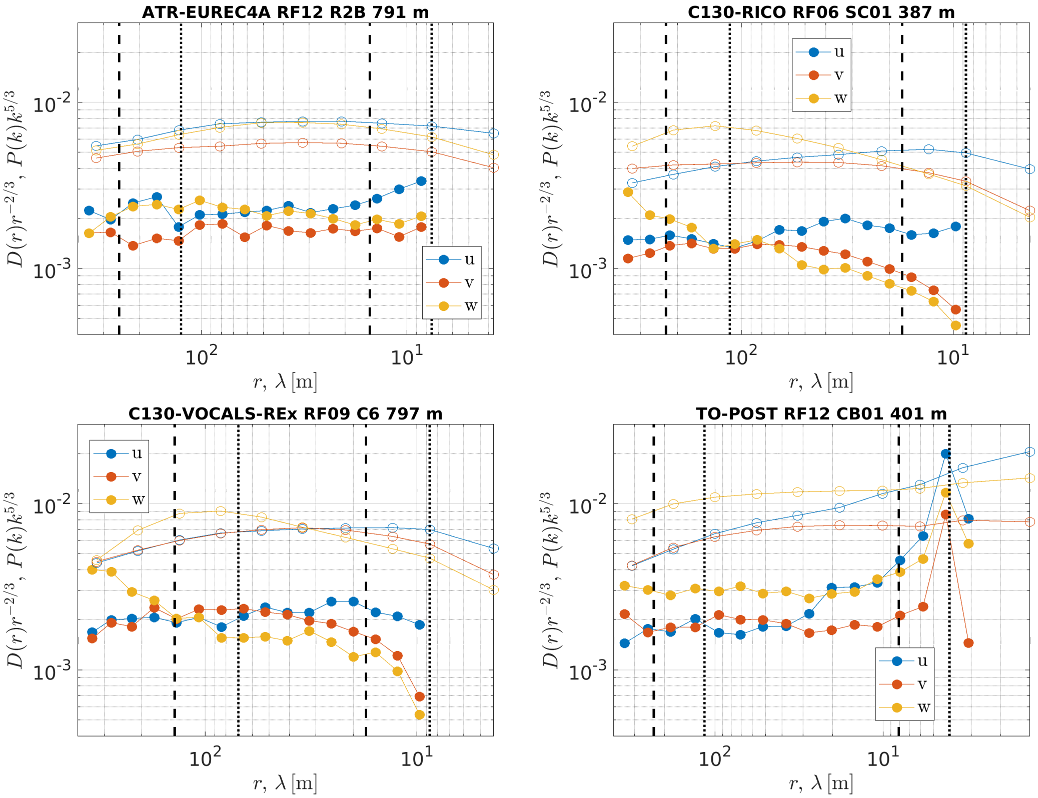

Figure R1. Compensated structure functions (open circles) and power spectra (filled circles) for a single segment from each experiment. The black dotted and dashed lines denote the chosen fitting ranges for structure functions and power spectra, respectively.

ing scale, in contrast to previous experimental studies on local isotropy and against the intuition that the anisotropy of large scales should be lost along the cascade towards small scales. See also our response to the comment below.

l. 130-134 Is it possible that the coarser spatial resolution of aircraft measurements in comparison to helicopter and balloon measurements is part of the problem? For example, Siebert et al. [2006] found ratios closer to 4/3 from sonic anemometer data. The onset of isotropy may be gradual across a perceived inertial subrange. See the figure below, from Kaimal et al. [1972].

It is very reasonable intuition that local isotropy should appear gradually with decreasing scale as showed by Kaimal et al. (1972) for the surface layer. Siebert et al. (2006) calculated P_v/P_u and P_w/P_u in the range of scales about 0.4–8 m. Actually. they analyzed two measurement series collected in shallow cumulus clouds. In the first one, both spectral ratios were approximately 4/3. In the second, also relatively close to the isotropic value, however with P_v/P_u systematically higher and P_w/P_u systematically lower than 4/3. Due to larger TAS of aircraft, we investigated larger scales: for power spectra from $4\Delta r \approx 16$ m in the case of ATR and C130, and from $6\Delta r \approx 8.4$ m in the case of TO. Hence, our range is disjoint from the range of Siebert et al. (2006).

On the other hand, Nowak et al. (2021) analyzed measurements from the same platform ACTOS, however carried by a helicopter, in coupled and decoupled marine stratocumulus-topped ABLs during ACORES campaign (Siebert et al., 2021). They showed that P_w/P_u in the coupled ABL does reach 4/3 for the scales 5–100 m at a few sampled levels. Such scales should be at least partly resolved by research aircraft. In our revised introduction, we reviewed in more detail the findings on scale-by-scale spectral ratios from the studies mentioned above (Kaimal et al., 1972; Siebert et al., 2006; Nowak et al., 2021).

Following the suggestions, we computed the composite scale-by-scale transverse-to-longitudinal ratios for our data. In order to average over various segments in a meaningful way, we considered the non-dimensional scales r/L and λ/L for structure functions and power-spectra, respectively. The results exhibit no tendency to approach 4/3 with decreasing scale. Actually, the ratios decrease and increasingly depart from 4/3 with decreasing scale. This trend is consistent with the absolute values of the fitted scaling exponents which are larger for transverse than longitudinal velocity components.

Moreover, similarly to previously presented bulk parameters, the scale-by-scale ratios seem to be to a large extent aircraft specific as the differences between the characteristic levels of the ABL are not substantial. The exact reason for the discrepancy in the observed turbulence isotropy between the measurements performed with research aircraft and other platforms remains uncertain. Aircraft-dependence in our results suggest deficiencies of the measurement technique. The goal of our study is in fact to raise awareness of this issue as well as stimulate further work to explain the discrepancies and potentially improve measurements.

l. 140 Please clarify: if your coordinate system xyz is such that x is the direction that the aircraft flies, then there is a mean wind (with respect to the Earth) that in general will not be in the direction of x. On land stations, it is customary to rotate the data so that the mean wind vector is $(\overline{u}, 0, 0)$, but you do not mention a similar procedure. Therefore, it appears that in the aircraft reference frame there will be a $\overline{v}$ and possibly a $\overline{w}$. How does that impact, if at all, your results? Is this irrelevant because the aircraft's speed is so much greater than the average wind speed with respect to the Earth?

We wrote an additional paragraph in the theoretical part of the introduction to explain how frozen flow approximation is applied and which directions are longitudinal/transverse in two typical experimental configurations: rapidly moving aircraft and fixed ground-based mast.

Our coordinate system xyz is such that x is the direction in which the aircraft moves with respect to air. For stabilized segments it coincides with the orientation of the aircraft fuselage projected onto horizontal plane (true heading angle). x is not the direction in which the aircraft moves with respect to Earth (course over ground). The air velocity with respect to Earth (i.e. wind) and the aircraft motion with respect to Earth are irrelevant for the definition of the coordinate system. Mean wind vector expressed in our coordinate system is generally ($\overline{u}$,$\overline{v}$,0), i.e. has non-zero longitudinal and lateral components. The measurement records are anyway detrended before calculating structure functions or power spectra, therefore we do not expect the values of $\overline{u}$ and $\overline{v}$ to influence the results.

l. 254 Can the authors discuss more at length how buoyancy and possibly other effects impact isotropy? Some interesting discussion (as a starting point) can again be found in Akinlabi et al. [2019].

We described the potential influence of buoyancy and wind shear on isotropy in sec. 5. However, although buoyancy and wind shear certainly affect the character of turbulence, we believe it is unlikely these factors explain our results. It is because D_w/D_u, P_w/P_u are smaller than 4/3 at all levels of the ABL (see Table 2) and almost all considered scales. Even if there was very strong wind shear, it should be restricted to the surface and the top of the ABL. In the middle, one would then need stable stratification to weaken D_w, P_w but it is not the case.

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