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Interactive Comment

Interactive comment on "Reinterpreting aircraft measurements in anisotropic scaling turbulence" *by* S. Lovejoy et al.

S. Lovejoy et al.

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Response to Referee 3:

Referee:

For a non-specialist in turbulence it is hard to understand what you mean with "anisotropic scaling". Since the term is already used in section 2 in a quite special context, the reader probably will be lost. However, in section 3 a broader explanation follows. Thus, I would recommend introducing these basic definitions and the short introduction into the theory earlier (maybe interchanging sections 2 and 3).

Response: Good point, our response is to add a paragraph right near the beginning of the introduction so as to carefully clarify our meaning. We hope it is now clear that "anisotropic scaling" means fluctuations varying as power laws in both the horizontal

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and vertical directions but with different exponents.

Referee: Please also explain your notation more carefully, e.g. the notation of Fourier transforms for the cospectral analysis in section 2.

Response: We have added mathematical details in section 2.3.

Referee: Aircraft measurements are interpreted in the light of the anisotropic turbulence and used as a corroboration of this theory. However, the data does not contradict this theory but to my view it also does not really strongly confirm it. There are some weak points, which should be clarified: In Sect.2.1 the data analysis needs flight legs that have altitudes varying by up to + - 450 m (i.e. the total range is 900 m), which sounds as a very coarse "constancy". On the contrary, the spectra < |deltaz(deltax)| > are then obtained from data with a mean slope of 0.025 m/km (i.e. O(10**-5)). These two orders of magnitude seem incompatible: How is it possible to derive conclusion on something of the order 10**-5 when the data baseline itself varies with a much larger order of magnitude? The mean slope given by the authors in line 5, page 3876, does not appear in Fig. 2, and line 7 pg. 3877 give a range of slopes that does not contain this mean; how can they be related?

Response: Thank you for spotting this error. In the initial analyses we based our calculations on a sampling frequency of 4 Hz and only discovered later that the correct rate was 1 Hz. This lead to numerous factors of 4 changes - and in the case of this graph - there were errors in fixing the problem. We have replaced fig. 2 with a correct version, this time using the long legs rather than the short legs. We have also put the mean slope at half the leg distance as a dashed line for reference (the value 0.025 m/km should have been 0.12 m/km).

Referee: To my impression the discussion of altitude/velocity correlations around the turn of pgs. 3876 and 3877 is unclear and a bit hand-waving.

Response: We hope that the correction of the slope figures and fig. 2 will make this

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more convincing.

Referee: The authors claim that there is a kink in the fit to the two lowest lines curves in Fig. 3d. This is all but obvious, although it is better visible in Fig. 3e. But also there it seems not to be evident that the slope of the right part of the fitted velocity curves is indeed -5/3. It could be steeper as well and might then be combined with the left fit to a single fit. Perhaps it would help to give some error bars to see whether the fits are plausible or what else might work. Line 9 pg. 3881 gives such an error bar: slope 2.2 ś 1.4, i.e. the standard deviations are 63

Response: If these graphs were the only information available, we would agree that they are not totally convincing. However, the slopes shown are not regressions but reference slopes equal to the theoretical values Hh, Hv; this is really a test of the theory. However, as explained in the following sections, all by itself it is not a great test since we show (when analyzing leg by leg) that the transition point between the two power laws (deltaxc, table 1) varies tremendously from leg to leg so that the averages in fig. 3d, e "smear"; the transition point out leaving the smoother wide, curved transition region apparent in the figure. The is no relation between the spread (not error!) in the ratio of isobaric slopes to geostrophic slopes (2.2+-1.4) and the accuracy with which the spectra and their logarithmic slopes can be estimated. The key point of our paper is the existence of aircraft-induced systematic variations, we attempt to show that the data is compatible with that hypothesis. The magnitude of the spectral fluctuations is of the order of the dispersion of the points around the lines.

Referee: The goal of the article is not clear: I suspect that the aircraft data are used to corroborate anisotropic turbulence in the atmosphere, but this is not clearly stated. Thus, a revision in terms of stating the goals more clearly and to give a thread through the manuscript would be very helpful and would strengthen the conclusion of this work.

Response: This comment is very similar to one made by referee 1, the basic point being that the paper is primarily methodological, i.e. about "reinterpreting"; the data.

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Indeed, we have just written another paper attempting to quantitatively exploit the data for estimating turbulent exponents/characteristics. We refer the referee to our response to referee 1, as well as to systematic modifications in the text to better reflect this goal.

Referee: On page 3879, the authors mention a number of 4000 points of a flight leg and then they state in the same line that this imply n=24? I do not understand this, could you please clarify this?

Response: It is simply that in our entire data set with 16 legs of varying lengths, there were a total of 24 flight sections 4000 points long. We have tried to improve the explanation in the text.

Referee: 2. References: The use of references is a bit sloppy and should be checked carefully. For instance, the first reference Adelfang, 1971 is not correct at all. This is the correct version: Adelfang, S.I., 1971: On the relations between wind sheers over various altitude intervals. J. Appl. Meteorol., 10, 156-159.

Response: Thanks for this correction and the typos.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 3871, 2009.

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