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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

Anonymous Referee #3

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General comment:

In this manuscript aircraft measurements are interpreted in terms of the theory of anisotropic turbulence as proposed in former articles by some of the authors. Although this is an interesting interpretation and maybe also a confirmation of the anisotropic turbulence theory, some issues have to be clarified. Additionally, the overall style of the manuscript should be revised: since "Atmospheric Chemistry and Physics" has a broad variety of themes, this work is very specialized and requires special knowledge on general turbulence theories and recent developments in turbulence research. A broader introduction on these topics would be very helpful for a better understanding for interested readers but non-specialists in turbulence. Last but not least the handling

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Interactive Discussion

Discussion Paper

of references is a bit sloppy, this must also be improved. Thus, I recommend publication after some revisions

Major points:

1. 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). Please also explain your notation more carefully, e.g. the notation of Fourier transforms for the cospectral analysis in section 2. The authors should keep in mind that ACP is not focussed on dynamics and turbulence but has a broader variety of themes. So, a few more words on anisotropic turbulence would be appropriate.

2. 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 $\pm$450 m (i.e. the total range is 900 m), which sounds as a very coarse "constancy". On the contrary, the spectra $< |Dz(Dx)| >$ 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?

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- To my impression the discussion of altitude/velocity correlations around the turn of pgs. 3876 and 3877 is unclear and a bit hand-waving.
- 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 ± 1.4, i.e. the standard deviations are 63% of the mean. If this also applies to figs. 3d/e, then it will be become quite difficult to distinguish even between $k^{-2.4}$ and $k^{-5/3}$.

3. 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.

Minor points:

1. 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?
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:

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Adelfang, S.I., 1971: On the relations between wind sheers over various altitude intervals. J. Appl. Meteorol., 10, 156-159.

Technical comments:

1. page 3872, line 11: “inertia”
2. page 3883, lines 12/13: This sentence does not make sense, maybe the second “that” is superfluous?

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

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