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Atmospheric Chemistry and Physics Discussions



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

**Anonymous Referee #1**

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General statements

I recommend the paper for publication in ACP, although the content, which is basically related to the correct interpretation of airborne measurements (taking care of a sampling issue), would make it also a candidate for a more technically oriented journal like the AMS-JTECH.

Airborne measurements of turbulence involve, besides good calibrated sensors, also an accurate treatment of the movement of the airplane itself. Some of the platform providers (not all) take care of procedure calibrations by dedicated test manoeuvres. Nevertheless, there might be still open issues, one of them seems to be addressed by the present study.

The paper presents an interesting analysis, which should be relevant to many researchers involved in airborne measurements of turbulence. Therefore, it should be published.

Nevertheless, there are some comments (most of them of minor character):

The authors should comment the main point of the interactve comment by Yano, what would be the effect of strong convection, i.e. on the 1/f signiture of outflows of strong convective elements, on the universal scaling offered by the paper.

Abstract:

In general the abstract needs to state the central problem more clearly at its very beginning, sampling problems in the fluctuating velocity field due to altitude variations in the aircraft path.

It is not quite clear: is the general purpose to guide (support) aircraft measurements of turbulence by a revised or new theory of turbulence, or is the development of such a new (anisotropic) "theory" a goal on its own?

3872/3 It may be right that most relevant turbulence theories are isotropic, but there has been considerable work on quasi-two-dimensional turbulence and stratified turbulence (Pope, Lilly) or in magneto-hydrodynamics which is not isotropic and relevant. On the other hand, some of the famous isotropic turbulence theories concentrate on the smallest scales of a flow (e.g. Kolmogorov), scales much different from those analysed by the authors. So, a sentence like "Until now virtually all relevant theories have been isotropic." seems not to be an appropriate introduction to the present work. In contrast to "Until now ..." this paper does not develop a "new theory", rather it addresses scaling issues in an older approach.

3872/10 Do not use "Gulfstream 4" uncommented in the abstract, the general reader might not know it, "high flying instrumented aircraft" would be a possibility instead.

1. Introduction



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3873/15 "mainstream turbulence". Who ever defines mainstream, many of those theories are made for the smallest scales, way below the energy containing eddies and below of the scales discussed in the present paper. They are not the theories to compare to in the present context. On the other hand, there are theories (see remark above), which are anisotropic. For the very large scale events (geostrophic scales) two-dimensional turbulence (e.g. Kraichnan-Montgomery) may be considered relevant, at intermediate scales "stably stratified turbulence" (D. Lilly) is a widely cited approach. (not only experimentalists care about this, 3873/19).

2: The data

It would be helpful to say something on the aircraft at the beginning of this section. I.e. how the horizontal wind is calculated from the prime sensor in combination with the aircraft positioning and attitude angle system (for some systems in the transition from the sensor dominated part to the navigation system dominated part a scale break is artificially introduced).

Although the Pacific 2004 experiment is described in Hovde et al. (2009) /not accessible to me, yet/ it would be very useful, if the authors could provide here some information on the general meteorological background. Weather features like fronts, strong convection, jet stream (i.e. information on the typical vertical shear of the horiz. wind would be necessary to judge about effects of the influence on altitude displacements of the measuring aircraft). Typical wind speed and shear information could be added to table 1.

3875/9 the subheading "The legs, slopes" does probably not unclose its meaning for many of the ACP readers. Try to find a more meaningful short wording here.

3875/17 with the sampling time 1s and aircraft speed only scales larger than 2 x 1 x 280m = 560 m can be resolved, way above the scales of Kolmogorov (and Bolgiano-Obukov) inertial range turbulence, which the authors use for comparisons at several locations in the text. The data and the "mainstream" turbulence theories are not on



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the same scale, so they should not be used for classifications e.g. 3876/25). Please consider this at the many locations throughout the paper.

2.2 Spectral analysis

Give a short description how spectra were calculated and smoothed (method and a comment on significance). How did You deal with the general trend in the data?

3877/16ff again the Kolmogorov scaling is cited (-5/3 in spectra); Kolmogorov's notion is only valid for the three-dimensional turbulent inertial energy spectrum (E(k) ˜ kˆ-5/3), and not for scales of many kilometres in the stably stratified Earth´s atmosphere. The -5/3 slope seen in some of the spectra of figure 3 might be more in agreement with Lilly's "stably stratified turbulence theory", which predicts a -5/3 slopes for horiz. wind components. A -5/3 slope is also in agreement with the linear saturated gravity wave spectrum (e.g. Van Zandt et al.). But not Kolmogorov inertial range turbulence theory.

2.3 Cospectral analysis

Give a short description how cospectra were calculated and smoothed (and a comment on significance).

3879/14ff it would be helpful to include the approx. level of statistically significant coherencies as a (dashed) line into the relevant plots. Or give the number in the caption.

3881/17 as comment on 3877/16 above.

3. Understanding ...

3882/7 as comment on 3877/16 above.

3884/27 "our Gulfstream data", better: Gulfstream data as analysed here.

5. Comparison with other aircraft studies

The reviewer is looking forward to a more general discussion among the schools, which might follow this publication.



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6. Conclusions

3893/3 delete "at"

3894/12 last expression, the word "dialectic" seems to be overdone for this short chain of reasoning (may be a matter of taste).

3894/14 shift sentence "See Tuck (2008) ...; " into the introduction may be to 3874 in line 24.

3894/20 replace "make" by "may"

3894/22 I guess a large amount of papers are written for purely scientific purposes. I hope the authors enjoyed tax-payers salary during this exercise. If the authors feel they need to state this, they should (also a matter of taste).

Figures:

The number of figures appears to be adequate.

Style:

Unnecessarily, in many of the figures the curves cross either the x or y or both axis, often some labels are hidden behind one ore more curves. This is annoying i.e. in1a, c, 3b,c,f,g. Try to avoid label crossing.

Figure 1c should have units on all axis and maybe a i to iv sub-identification.

The imported plots in figures 7a,b,d,e need a better reproduction, labels need to stay readable.

Captions:

Try to avoid starting figure captions with the phrase "(Black,) This shows ..." in figures 1c, 2, 3a, 3e, 5a, 6

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



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