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Atmospheric Chemistry and Physics Discussions

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9, S1857–S1862, 2009

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 #1:

Referee: General Comments: 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.

Response: The analysis of scientific data requires both a theoretical framework in which the basic quantities can be defined and understood, as well as a technical sampling/measuring part which addresses the issue of the adequacy of the measuring device, its associated errors i.e. the expected degree of correspondence between the data and the theoretical quantities. Somehow, the referee sees our contribution as primarily in helping to refine previous turbulence measurements i.e. that it is a contribution in the measurement/sampling/error part of the problem. We found this surprising since on the contrary our goal was methodological, we aimed to determine the appropriate theoretical framework in the which the measurements should be understood (hence the word "reinterpretation"; in the title). Should the data should be interpreted in the framework of a single anisotropic scaling regime or should it (continue) to be interpreted in terms of multiple (scale dependent) isotropic scaling regimes? In the former case there may be no scale breaks in atmospheric dynamics over huge ranges of scales and the aircraft successively measures the horizontal and then the vertical scaling exponent of the wind, whereas in the latter model the atmosphere has isotropic scaling regimes of limited extent which the aircraft measures successively. We actually did very little to address the second technical/sampling part - this will be the main subject of a future publication - on the contrary we were primarily concerned with determining correct the theoretical framework.

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

Our response to the interactive comments with Yano: We responded very quickly to Dr. Yano's comment; however our response contained several figures and hence the editors treated it as an accompanying publication rather than a response to a comment. Hopefully it will appear once the referees have a chance to evaluate it. Unfortunately, the APCD system doesn't even acknowledge the existence of such a response under review.

Referee: In general the abstract needs to state the central problem more clearly at

ACPD
9, S1857–S1862, 2009

its very beginning, sampling problems in the fluctuating velocity field due to altitude variations in the aircraft path.

Response: The primary goal is to use the data to empirically establish a new anisotropic scaling model of atmospheric turbulence, to show that previous interpretations (in terms of a series of isotropic regimes) are untenable. We have made some modifications in the abstract to make this more clear.

Referee: 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?

Response: This is not a theoretical paper in the sense that we simply examine the implications of an existing (anisotropic scaling) theory on aircraft measurements. On the other hand, if the new interpretation is correct then it has profound consequences for our understanding of the atmosphere.

Referee: 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.

Response: Yes, but the corresponding anisotropies are "trivial" in the sense that they involve the same scaling exponents in the horizontal and vertical directions. For example, this is true of Charney's quasi-geostrophic turbulence which involves a nonlinear coordinated transformation in the vertical, but uses the same exponent as in the horizontal. Hence in the title and throughout the paper we constantly put the words "anisotropic"; and "scaling" together to emphasize that we are discussing a model with different exponents in different directions.

Referee: 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 the-

ACPD

9, S1857–S1862, 2009

ories 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.

Response: We did not initially want to further burden this sentence in the abstract with the qualifier that we are speaking of theories in which the exponents are the same in all directions; we have reluctantly done so in the revised version. Also, the word "relevant" in the second sentence refers to the fact that the comment is restricted to theories used to interpret aircraft data. The new theory developped in this paper is not a theory of anisotropic scaling turbulence, but rather some minor theory needed to apply the latter to the interpretation of the aircraft measurements.

Referee: "Mainstream turbulence". Whoever 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).

Response: Again, we are restricting the discussion to anisotropic scaling theories and to our knowledge these are only the quasi-linear gravity wave theories and the 23/9D theory which were developed in the early 1980's. We believe that this is true independently of the scale range over which the theories purport to apply. We have nevertheless added information to this effect.

Referee: 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 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.

Response: Throughout the paper we tried to emphasize that while the classical theories of turbulence have the same exponents in all directions (are isotropic or at least quasi-isotropic), the atmosphere is scaling but anisotropic. Nevertheless, in as much as we find evidence for exponents near the Kolmogorov value 1/3 and the Bolgiano-Obukhov value 3/5, our model can be regarded as a strongly anisotropic generalization/extension of these classical theories. We have modified the text to make this more apparent.

Referee: 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?

Response: We used a standard Hanning window (information added) and then (as already explained) averaged the result of wavenumber bins using ten per order of magnitude.

Referee: 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) = k**-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.

Response: We hope to have now made it clear that neither the Kolmogorov theory nor other theories with exponents the same in all directions are realistic. The term "Kolmogorov scaling" is used simply to refer to the roughly kx**-5/3 law, where kx is a horizontal wavenumber but we hope it is clear that this does not imply that the vertical spectrum is kz**-5/3 (with kz a vertical wavenumber): the original Kolmogorov theory is not assumed to be valid anywhere in the atmosphere. We have added a comment

ACPD

9, S1857–S1862, 2009

Interactive Comment

to the already cited Van Zandt reference pointing out that his vertical spectrum is the same as that inferred here (=2.4).

Referee: Give a short description how cospectra were calculated and smoothed (and a comment on significance). It would be helpful to include the approximate level of statistically significant coherencies as a (dashed) line into the relevant plots. Or give the number in the caption.

Response: The only missing detail was the use of a Hanning window, there was no smoothing. The level of significance was already included in the plot (the dashed lines in figs. 3f, g) and was discussed in the text.

Response to the remaining comments: These are relatively minor points of style and the like and have generally been addressed in the text.

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

ACPD

9, S1857–S1862, 2009

Interactive Comment