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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Why anisotropic turbulence matters: another reply" *by* S. Lovejoy et al.

S. Lovejoy et al.

lovejoy@physics.mcgill.ca

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We thank the referee for his remarks; we will react point by point (our remarks prefaced by "Au", the referee's by "R2").

R2: "With respect to the first point, the claim of "competing statistical turbulent frameworks" seems exaggerated. That horizontal and vertical dynamics are qualitatively different isn't exactly a surprise to atmospheric dynamicists. Isotropy is invoked (though typically in the horizontal rather than the vertical) simply to facilitate analytical progress. At some later stage its validity is (re)examined: nobody is fixated on isotropy."

Au: Since the early 1980's there have been several anisotropic scaling theories proposed in the literature; there is no disagreement about the coexistence of "competing

statistical frameworks", the referee claims only that their significance is "exaggerated". But surely that's a question for the history of science to decide? If the atmosphere really has different scaling in the horizontal and vertical directions, then the nearly 30 years of existence of a corresponding theory would surely on the contrary be highly significant?

R2: "... in the absence of a careful error analysis, the difference between a spectral slope of -2.4 and one of -3 is questionable at best. As far as I can tell the authors have only plotted reference slopes."

Au: It seems that the referee missed figs 4a, 4b of LTSH. Indeed, his further comments suggest that he also missed the key sections 3 and 4 which contain the careful error analyses that he sought. In fig. 4a, b regression analysis was used to determine the optimum exponents and critical distances as well as error estimates for the exponents. The fitting function was simply the sum of two power laws so that there was no a priori assumption about the values of the exponents for either the small or large scale ranges. The uncertainties in the exponent estimates were of the order of $\pm$0.1 and for the ratio, $\pm$0.05. The large scale exponent had values Hv $\approx$0.65$\pm$0.04, 0.67$\pm$0.09 for the transverse and longitudinal components respectively, corresponding to spectral exponents (without intermittency corrections) of β = 1+2H $\approx$ 2.30$\pm$0.08, 2.34$\pm$0.18. With the intermittency corrections these values are reduced by K(2), which here is for the spectrum are 0.05 (Lovejoy et al., 2010), although see the discussion since this estimate is affected by the aircraft trajectories. These values are many standard deviations from the value 3 so that we can confidently eliminate the β = 3 hypothesis.

R2: "I am similarly unpersuaded by the new theoretical arguments. Everybody knows that the quasi-geostrophic model has many limitations; among meteorologists, it is equally well known that it provides a very good description of synoptic scales, i.e., large scales where, the authors claim, the shallower, k**(-5/3) spectrum would be observed if not for contaminating vertical motions."

Au: “Everybody knows” is not a scientific argument; it is not even a sociologically correct assertion. Contrary to what is “equally well known”, we pointed out its strong theoretical shortcomings and contradictions with observations in both the horizontal and in the vertical.

R2: “A corollary to this is that the destabilisation of isotropic 2-D turbulence mentioned by the authors is not strictly applicable: the breakdown of (approximately quasi-geostrophic) balance would be the proper paradigm.”

Au: This question has been carefully considered by Ngan and Bartello 2004, we need not revisit it here.

R2: “With respect to the influence of vertical motions, the authors state that the criticism levelled by Li10, namely that an uncertainty of the order of 1 m/s due to vertical motion of the aircraft cannot account for the steepening of the spectrum at large scales, is “simplistic” (Sec. 3.1). They point out that, for some flight legs, instantaneous variations in the velocity can be larger, around 7 m/s. This plausibility argument does not prove anything, as the authors themselves concede.”

Au: The referee went through this too quickly and missed the essential point: first, we showed that the mean - not “instantaneous” - deviations are of the order 7 m/s: the “instantaneous” deviations can of course be much larger. The original graph upon which this is based and the argument given in the text therefore does indeed prove that Lindborg’s back of the envelope argument is irrelevant. What we do we freely acknowledge is that it doesn’t prove that the large scale follows k**(-5/3) along isoheights.

R2: “What the authors need to do is demonstrate the importance of vertical motions without invoking the scaling expressions (5) in LTSH. I strongly suspect that their conclusion is an artefact of these scaling expressions.”

Au: This not an artefact, but indeed as we argued, a consequence of these scaling expressions!

R2: “Physically, the character of turbulent fluctuations changes as one goes from microscales, where 3-D turbulence prevails, through the mesoscale where convection and other physical processes dominate, to synoptic and global scales, where the dynamics are quasi-horizontal.”

Au: Here the referee candidly reveals his own paradigm which even advocates of the 2D/3D isotropic turbulence model find difficult to theoretically justify. This type of eclectic theorizing – simultaneously combining a statistical turbulence theory (which shows at most one break in the scaling, and this at about 100 km) with classical (deterministic) phenomenological interpretations (involving many characteristic scales) - is what we are trying to overcome by systematically studying the scaling properties of all the fields in both the horizontal and vertical directions. The referee has fallen into the “phenomenological fallacy” of inferring mechanism from form. In anisotropic scaling systems, form can change systematically with scale even though there is a unique scale invariant mechanism. In any case, the standard model is a special case of eq. 5 above, i.e. when Greekphi Subscript h = Greekphi Subscript v (a single flux dominates both the horizontal and vertical dynamics) and Hh =Hv, so it is hard to see how it can be any more of an artefact of than the standard (more restrictive) ansatz.

R2: “Ideally the authors would verify the scaling expressions via aircraft measurements of Δv at fixed x;”

Au: We don’t understand: this is exactly what we did throughout. . . or does the referee mean at isoheights (i.e. at fixed z, not fixed x?)

R2: “. . .by basing their analysis on these expressions, which are unlikely to hold over the entire range of scales, the horizontal, Δx dependence can be incorrectly attributed to variations in the vertical, i.e., to Δz.”

Au: Science advances most efficiently by hypothesis testing, not curve fitting. Our primary object was to test our theory, i.e. the scaling expressions (eq. 5 of LTSH, above); this was indeed the subject of most of our analyses; they all gave support

to our theory. However, as we pointed out above, we also (fig. 4a, b) provided simple regressions which are of a common form predicted by both our theory and the standard 2-D/3-D model; there was nothing preventing us from finding Hv =1 as predicted by the latter, but we found instead Hv $\approx$0.65$\pm$0.04, 0.67$\pm$0.09.

R2: "Since, presumably, such aircraft measurements do not exist, the authors should devise another independent test.. . ."

Au: Again the referee must mean isoheight data, otherwise, this is what was used.

R2: "For example, they could show that the transition occurs at larger scales for legs with smaller vertical slopes. I note that there is little evidence of this. In Fig. 3a of LTSH, the spectra look rather similar, even though the maximum Δz ranges from 70 to 900 m."

Au: Unfortunately, as we pointed out, even the simplest model of the trajectory in which the aircraft has a constant slope throughout is not trivial to analyse since the transition scale depends sensitively on the sphero-scale, and this, by direct measurement was found to vary substantially from flight to flight (this is discussed in detail in LTSH). In addition (also discussed) were some of the complications: the slope is scale dependent, also, it is correlated with the horizontal shear (if only due to geostrophy and hydrostatic balance). Finally, in our response we alluded to yet another effect: the non-instantaneous nature of the measurements. Indeed taking into account the temporal lags between the measurements along a trajectory (see (Lovejoy et al., 2008) we find that if the vertical velocity changes by Δw over the trajectory duration t, then this is statistically equivalent to a vertical displacement of Δz = tΔw. Physically, this is a consequence of the Gallilean invariance of the equations and it simply means that we must take into account not only the aircraft flying along a slope, but also the fact that the wind field itself is being advected in the vertical. If the aircraft flies at speed va, then this effect is the same as if the aircraft was flying on a slope sa = Δw/va. Fig. 1 (which we propose to add to the revised paper) shows the estimates of w using first order

structure functions taken from the ECMWF interim reanalysis averaged over latitudes 30 -45o N and the year 2006. We see that at 1000 km and 200 mb, typical differences in vertical velocity are of the order of 4 -5 cm/s; this combined with an aircraft speed of 280 m/s lead to sa $\approx$ 1.6x10**(-4); this is slightly greater than the mean physical slope s $\approx$ 1.2x10**(-4); therefore, this "temporal slope" effect will often dominate the physical slope effect (although both will be highly intermittent: this constant slope model is clearly simplistic).

R2: "I suggest that the legs with the smallest variations in Δz be compared to legs that failed to meet the criterion for a "straight flat leg" (Li09, p. 5009) and a scatter plot of the transition scale (obtained through a proper error analysis) against Δxc be produced. If the authors' explanation is correct, there should be a statistically significant correlation."

Au: The detailed analyses presented in LTSH show these connections already. The main point was that the slopes combined with the observed sphero-scales combined with the observed exponents leads to transition scales of the same order as those observed. In addition, the cross-spectral coherency and phase analyses showed that the wind fluctuations lag behind the pressure fluctuations over the entire low wavenumber regime, but that they lead in the high wavenumber regime. Although we agree that correlations are not causality, in this context, the complete reversal in phase exactly at the spectral transition point is pretty unequivocal. In order to drive the point home, we refer the reader to the new fig.2 (which we propose to include in our revised ms.) which is an original analysis of the mean horizontal wind shear along the 24 legs analysed in LTSH. This figure differs from others in that we have taken all the pairs of points on a trajectory and calculated: and also by fixing Δz and averaging over all the Δx. The figure shows that for Δz>1 m (corresponding to the mean critical transition scale of about 25 km that the wind follows almost exact the theoretically predicted Bolgiano-Obukhov scaling Δv$\approx\Delta$z**(3/5) (the line is for reference, it is not a regression). At the smaller scales, Δv is independent of Δz as would be expected if the slopes of the aircraft were unimportant. If the sloping nature of the isobars were irrelevant (so that isoheight and

isobaric exponents were the same), they why is there a systematic variation of Δv with Δz (and why does it follow so perfectly the Bolgiano-Obukhov predictions?). In addition, the slope of the Δx curve (Hz =5/9, also indicated as a reference) is very near to that predicted by the same theory for the shape of isolines in a 23/9 D turbulence.

Figure Captions:

Fig. 1: The first order structure function for the vertical wind at 200 mb (top), 300 mb (bottom) from the ECMWF interim reanalyses averaged over latitudes 30-45o N and the year 2006.

Fig. 2: This plot shows the first order structure functions for the longitudinal component of the horizontal wind (<Δv>, bottom, i.e. f = v, units m/s), and the horizontal distance (<Δx>, top, i.e. f = x, units km) as functions of vertical separations (Δz), estimated for 24 aircraft legs each at 280 m resolution in the horizontal, 1120 km each (the same legs as discussed in LTSH 2009). For altitude fluctuations less than about 1 m (corresponding to $\approx$ 25 km), the wind fluctuations are independent of the vertical lag; for larger scales they follow almost exactly the Bolgiano-Obukhov $\Delta z^{**}(3/5)$ law (the lower reference line). Similarly, the mean horizontal displacement as a function of vertical lag (<Δx>) closely follows the predictions of the 23/9 D model for isobars (reference line slope 5/9).

References:

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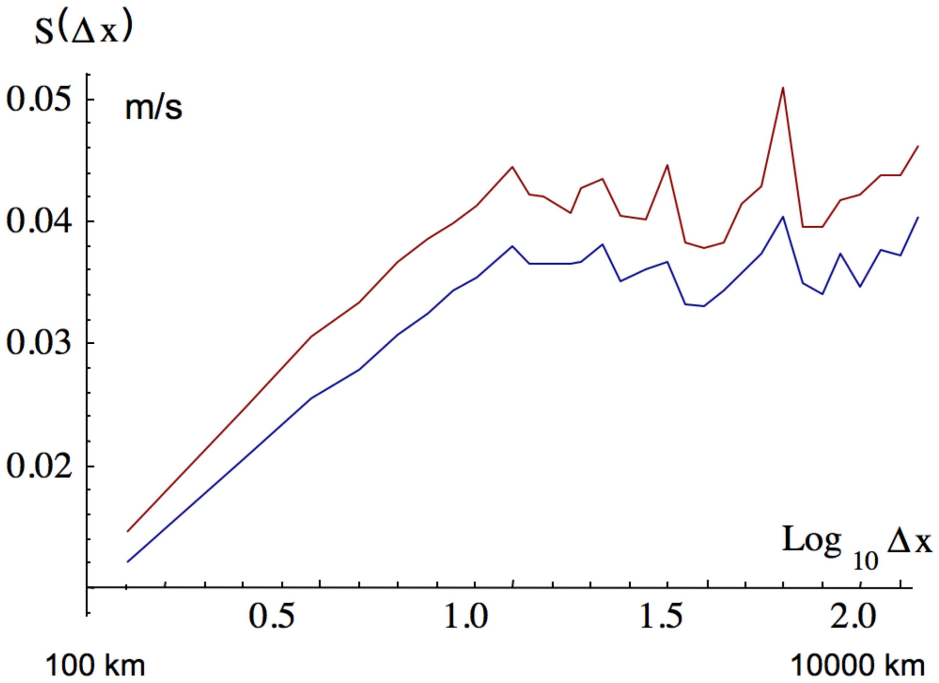

Fig. 1.

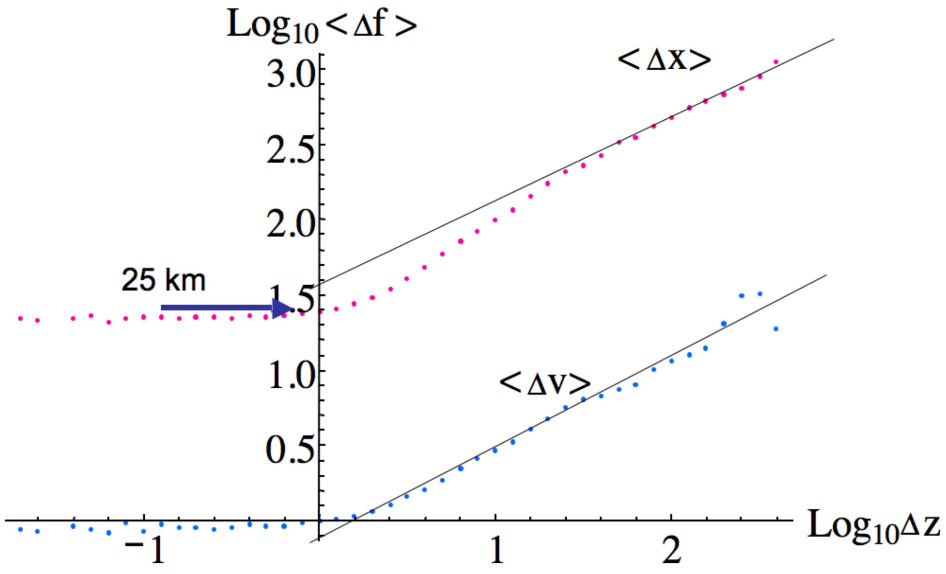

Fig. 2.