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

Anonymous Referee #2

Received and published: 17 May 2010

In their reply to the comment by Lindborg et al. (2010) (hereafter Li10), the authors summarise the differences between "scaling isotropic" and "scaling anisotropic" turbulence (Sec. 2) and respond to criticisms about the putative influence of vertical motions on aircraft measurements of the atmospheric kinetic energy spectrum (Sec. 3).

Main points

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.

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The critique of isotropic turbulence recapitulates points made in Lovejoy et al. (2009) (hereafter Lo09; Sec. 2.2) and introduces some new arguments (Sec. 2.3). I am not persuaded by the empirical evidence presented there: a break in the temperature spectrum has been observed in other studies (cf. Gage & Nastrom 1985); moreover, 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. 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. 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.

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.

What the authors need to do is demonstrate the importance of vertical motions without invoking the scaling expressions (5) in Lo09, $\Delta v = \phi_h \Delta x^{H_h}, \Delta v = \phi_v \Delta z^{H_v}$. I strongly suspect that their conclusion is an artifact of these scaling expressions. 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. Ideally the authors would verify the scaling expressions via aircraft measurements of Δv at fixed x; by basing their analysis on these expressions, which

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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.

Since, presumably, such aircraft measurements do not exist, the authors should devise another independent test. 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 Lo09, the spectra look rather similar, even though the maximum Δz ranges from 70 to 900 m. 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 Δx_c be produced. If the authors' explanation is correct, there should be a statistically significant correlation.

Recommendation

The present paper clarifies aspects of Lo09 but does not contribute much that is new. I therefore do not recommend publication unless the authors produce additional evidence along the lines adumbrated above. Given that there has already been a fair amount of talking at cross-purposes in this ongoing discussion/debate, it seems to me that further publication is warranted only if the key issue — do vertical motions play a role or don't they? — is resolved.

Personally I happen to agree with Li10 that the hypothesis of Lovejoy et al. is almost certainly wrong. But given that the Lo09 results (among others: see Bacmeister et al. 1996 for another study with a slope of approximately -2.5) suggest our understanding of the large-scale spectrum may be incomplete, the authors should be given an opportunity to make their case. Nevertheless, the burden of proof rests with them, as pointed out in another interactive comment (Smith 2010). An independent test that makes no assumptions about fractal scaling is mandatory.

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