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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comment on "Reinterpreting aircraft measurements in anisotropic scaling turbulence" by Lovejoy et al. (2009)" *by* E. Lindborg et al.

Anonymous Referee #1

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This is a Commentary on the paper by Lovejoy et al (2009) who argue that the large-scale structure (corresponding roughly, but perhaps not exactly) to a kˆ-3 spectrum) in wind fields observed from in-situ aircraft data result from the vertical displacements of the aircraft combined with the vertical structure of the velocity field. This is presented as an alternative to the explanation that the large-scale structure corresponds to the enstrophy-cascading range of quasi-two-dimensional or quasi-geostrophic turbulence.

The Commentary makes the following points:

(1) There have been several previous papers that show a kˆ{-3} spectrum, or something similar, emerging from observations or models where the 'vertical displacement effect'

is not relevant. The finding in models is robust to modifications, e.g., imposed boundary conditions, to the original Charney theory that predicts a kˆ{-3} spectrum.

These seem to me be straightforward factual comments that are relevant to the Lovejoy et al (2009) paper. They do not rule out the Lovejoy et al (2009) mechanism – they simply say that there is no compelling need for it.

(2) Lovejoy et al (2009) emphasise the importance of fluctuations in geometric height along the aircraft trajectory and seem to imply that these are more significant that fluctuations in pressure. Why this should be the case? The fluctuations in horizontal velocity are too large to be accounted for by vertical displacements.

My reading of Lovejoy et al (2009) is that they are ambivalent as to the relative importance of fluctuations in pressure versus geometric height. Certainly they find that on the large scale fluctuations in pressure are weak (presumably since the pilot was trying to fly an isobaric trajectory). However in the text they tend to emphasise the relation between altitude, pressure and wind statistics. To me the question is – what is natural to take as a quasi-horizontal surface? Given that PV is actually being stirred along isentropic surfaces, one might argue that it is disturbance relative to isentropic surface that is important. So I don't find this part of the Commentary particularly compelling.

On the other hand I do think it is useful to consider magnitudes of displacements, velocity contrasts etc. It seems fair to point out that according to Figure 1c, a horizontal distance of 500km might correspond to a velocity contrast of about 10 m/s and a vertical displacement of about 100m. If the velocity contrast was interpreted as resulting from the vertical displacement this would imply a vertical shear of 0.1 sˆ{-1} (over a depth of 100m). The authors of the Commentary refer to observations such as those of Alisse and Sidi (2000) to question the plausibility of this. I note myself that this would imply very small Richardson number.

(3) Nastrom and Gage (1985) showed that spectra calculated from selected flight segments that were close to isobaric were no different from those calculated from the

whole set of flight segments.

My reading is that Nastrom and Gage (1985) certainly did something like this, though their criteria was that there be a vertical displacement over the flight segment of no more than 100m, – which still allows quite large displacements. So I don't see this comment as particularly telling.

My overall verdict is that this Commentary adds usefully to the Lovejoy et al (2009) paper and does not say anything incorrect or inappropriate. I therefore recommend publication in ACP.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 22331, 2009.