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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.

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Both the replies of Lovejoy and Schertzer contain rather extensive discussions and arguments regarding whether or not atmospheric turbulence is isotropic and whether or not the quasi-geostrophic assumption of Charney is appropriate. We find these discussions and arguments irrelevant and obsolete. Some of the computer simulations we reference are based on the primitive equations that do not include any assumption of isotropy or on scaling or quasi-geostrophy, and yet produce the approximate k^{-3}-spectrum. On the other hand, based on their particular scaling assumptions Lovejoy et al. suggest that the horizontal kinetic energy spectra should scale as $k^{-5/3}$ from very

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small scales to very large planetary scales, which is inconsistent with the simulation results as well as measurements by several different methods. The amount of evidence in favour of an approximate k^{-3} spectrum at synoptic scales is overwhelming. To our knowledge, there is not a single piece of empirical evidence reported in the literature in favour of the hypothesis of Lovejoy et al. that the synoptic energy spectrum should scale as $k^{-5/3}$. Not even the measurements made by Lovejoy et al. themselves support this hypothesis in any way.

Lovejoy et al. suggest that the evidence from one of the several methods of measurement - airborn wind data sampling - can be explained as an artefact of vertical movements of the aircraft. We think that the very straightforward argument presented in the second last paragraph of our comment definitely disproves this. An inaccuracy or uncertainty of 100 m with respect to the vertical position of the aircraft can not explain a supposed wind shift of the order of 10 m/s, for the simple reason that the typical wind shift over a vertical distance of 100 m is less than 1 m/s. In the previous version of our manuscript we referenced Alisse and Sidi (2000) to support this. In the revised manuscript we have added one reference: Lovejoy et al. 2007. From their figure 1 it is very clear that a typical wind variation over a vertical distance of 100 m is less than 1 m/s. Thus, it is quite clear that vertical variations of the order of 100 m of the aircraft trajectory cannot cause any large error in a measured wind difference of the order of 10 m/s. We note that neither Lovejoy nor Schertzer makes any serious attempt to rebut this straightforward argument.

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