

Comment on “Reinterpreting aircraft measurement in anisotropic scaling turbulence” by Lovejoy et al. (2009)

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Abstract. Recently, Lovejoy et al. (2009) argued that the steep $\sim k^{-3}$ atmospheric kinetic energy spectrum at synoptic scales ($\gtrsim 1000$ km) observed by aircraft is a spurious artefact of aircraft following isobars instead of isoheights. Without taking into account the earth’s rotation they hypothesise that the horizontal atmospheric energy spectrum should scale as $k^{-5/3}$ at all scales. We point out that the approximate k^{-3} -spectrum at synoptic scales has been observed by a number of non-aircraft means since the 1960s and that general circulation models and other current models have successfully produced this spectrum. We also argue that the vertical movements of the aircraft are far too small to cause any strong effect on the measured spectrum at synoptic scales.

Aircraft measurements (Nastrom and Gage, 1985) show that the atmospheric kinetic energy wavenumber spectrum has a transition from an approximate k^{-3} -range, at scales from several hundred kilometres to several thousand kilometres (synoptic scales), to an approximate $k^{-5/3}$ -range at scales from about five hundred kilometres down to one kilometre (mesoscales). Structure function analyses from aircraft measurements (e.g. Cho and Lindborg 2001) are also consistent with this result. Recently, Lovejoy et al. (2009) argued that the observed steep spectrum at synoptic scales is an artefact of the vertical motions of the aircraft. According to the authors’ own hypothesis the horizontal kinetic energy spectrum should scale as $k^{-5/3}$ from the very smallest scales of micro turbulence up to the very largest scales of planetary motions

(tens of thousands of km). If this hypothesis were correct, the observation of a steeper spectrum at synoptic scales must somehow be spurious. Lovejoy et al. (2009) thus try to show that the apparent inconsistency between the aircraft measurements and their own hypothesis can be overcome by some clever reinterpretations.

One does not need to go into any further technical details to see that the hypothesis of Lovejoy et al. is unreasonable. It is enough to notice the strong consistency between the aircraft observations and other pieces of evidence reported in the literature that do not have the aircraft movement problem raised by Lovejoy et al. (2009). Wiin-Nielsen (1967) calculated kinetic energy spectra using meteorological data from six different levels in the troposphere and found that the spectra had power law dependence at the four highest levels, with exponents varying from -2.6 to -3.1 . Julian et al. (1970) used four different meteorological data sets to calculate horizontal kinetic energy spectra and consistently found that they could be represented by a power law with exponent between -2.7 and -3.0 in the 1500 – 3500 km wavelength range. Boer and Shepherd (1983) performed a spherical harmonic analysis of global operational analyses of wind data and found that the kinetic energy spectra had a power law dependence at synoptic scales, close to, but somewhat more shallow than k^{-3} . Straus and Ditlevsen (1999) also performed a spherical harmonic analysis of reanalysed meteorological data and observed a power law dependence $\sim k^{-2.7}$. The structure function analysis of Cho and Lindborg (2001) has recently been confirmed by a structure function analysis of global sounding data (Frehlich and Sharman, 2009), showing an excellent agreement between the structure functions calculated from the aircraft data and the sounding data. Modern high resolution simulations using mesoscale fore-



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cast models show an approximate k^{-3} -spectrum at synoptic scales (Skamarock 2004) and so do global general circulation model simulation (e.g. Takahashi et al., 2006; Hamilton et al., 2008).

There is a well known theory of quasi-geostrophic turbulence developed by Charney (1971), predicting a k^{-3} -spectrum at scales which are strongly influenced by planetary rotation. Although Lovejoy et al. (2007) criticized Charney (1971) for using an isothermal lower boundary and for assuming 3-D quasi-isotropy, these limitations have been relaxed in many of the modern models of atmospheric turbulence. Model simulations of anisotropic quasigeostrophic turbulence consistently show a k^{-3} -spectrum at synoptic scales (e.g. Tung and Orlando 2003). At the synoptic scales, the two invariant quantities are potential enstrophy (variance of potential vorticity, including the planetary vorticity) and total energy (kinetic plus potential energy) (Charney 1971), not the buoyancy variance and classical kinetic energy used by Lovejoy et al.. The buoyancy effects are subsumed in the potential energy in the form of potential temperature variance as the additive factor to the classical horizontal kinetic energy. At the synoptic scales, it is the potential enstrophy flux that dominates. Dimensional scaling similar to what has been used by the authors then yields the k^{-3} -spectrum, not the $k^{-5/3}$ -spectrum, which is applicable to much smaller scales. Thus, there are strong theoretical arguments supporting a k^{-3} -spectrum at synoptic scales and there is a great amount of evidence from measurements and simulations showing spectra with power law dependence close to k^{-3} , but slightly more shallow in cases where the measurement is not close to the tropopause or planetary boundary effects are important.

Without justification Lovejoy et al. maintain that scaling laws should be expressed with respect to absolute height. They argue that a spurious scaling exponent is somehow introduced because the aircraft are following isobars rather than isoheights. It is very difficult to believe that isoheights should have any deeper dynamical significance than isobars and it is also difficult to see why the spectra should show such a big difference between the two cases. The isobaric height difference is on the order of 100 m at a horizontal distance of 1000 km. According to the argument of Lovejoy et al. this small difference causes a dramatic shift of about one order of magnitude in the kinetic energy spectrum and the velocity structure function at scales on the order of 1000 km. Such a large shift is highly unlikely, because the typical change of wind over a vertical distance of 100 m is less than 1 m/s (Alisse and Sidi, 2000; Lovejoy et al., 2007), while the typical change of wind over a horizontal distance of 1000 km is larger than 10 m/s (Cho and Lindborg, 2001). At synoptic scales, the difference between an energy spectrum or a second order structure function measured at an isobar and the same quantity measured at a nearby isoheight can therefore be estimated to be 1% or less.

Finally, it should be pointed out that Nastrom and Gage (1985) investigated the possible influence of the

vertical motions of the aircraft by calculating a spectrum using data from 39 selected flight segments that had virtually no deviation from isobaric paths and compared it to the spectrum calculated from a larger data set, including flights with quite large such deviations. No principal difference was observed between the two spectra. The scientific evidence pointing against the hypothesis of Lovejoy et al. (2009) is so strong that it is safe to conclude that the hypothesis is wrong.

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