Atmos. Chem. Phys. Discuss., 9, C7631–C7632, 2009
www.atmos-chem-phys-discuss.net/9/C7631/2009/

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 #2

Received and published: 30 November 2009

Scientists have invoked a number of theories to explain the character of the energetics and the kinetic energy spectrum of the atmosphere at large horizontal scales ever since measurements have become available.

While the 3D turbulence theory of Kolmogorov appears to be valid for fully 3D atmospheric flow (length scales of order meters to perhaps a kilometer or so), there is no consensus concerning a theoretical explanation of the the spectrum for mesoscale flow (order ten to several hundred kilometers). There is some consensus concerning spectrum of the synoptic scale - it is generally believed to be characterized as an enstrophy cascade, essentially dominated by 2D rotational-flow dynamics, possessing a horizon-

tal wavenumber k**(-3) behavior. The meso- and smaller-scales possess a spectrum with k**(-5/3) behavior.

Lindborg et al is commenting on a paper by Lovejoy et al that challenges the consensus view that the synoptic-scale energetics are characterized as an enstrophy cascade. This disagreement extends past the theoretical characterization of the spectrum - Lovejoy et al argue that the k**(-3) aspect of the spectrum is the result of problems with the analyses of aircraft observations that have been used to illustrate this behavior.

The Lindborg et al comment does not attempt to deconstruct the theory and supporting evidence offered by Lovejoy et al except to argue that the vertical variations in the horizontal winds are not sufficient to change the general character of the analyzed observational spectra in the synoptic scale. Lindborg et al also make three other points.

First, that many of the theoretical limitations to other existing theories supporting the k**(-3) character have been superseded by theories that bypass these limitations. Second, simulations also show the k**(-3) behavior, and the analyses of the model results should not have the problems that Lovejoy et al associate with the observational analyses. Third, that other observational analyses (i.e. not aircraft based, e.g. radiosondes) show the the k**(-3) character. The theory and arguments on both sides of this disagreement are complex. While it would be ideal to have a complete deconstruction of the Lovejoy et al arguments in the Lindborg et al comment, it is, in my view, sufficient for publication that Lindborg et al have pointed out that there is much more than just the observational analyses of the aircraft data that support the existing consensus of the k**(-3) character of kinetic energy spectrum for the synoptic scales, and that the Lovejoy et al theory and analyses need to explain this other evidence if it is to gain traction in the community. Both Lovejoy et al and Lindborg et al provide the necessary background and references for readers to judge for themselves the validity and correctness of the arguments and evidence presented by both.

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