Atmos. Chem. Phys. Discuss., 10, C4679–C4685, 2010
www.atmos-chem-phys-discuss.net/10/C4679/2010/

Atmospheric Chemistry and Physics Discussions

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

S. Lovejoy et al.

lovejoy@physics.mcgill.ca

Received and published: 1 July 2010

Our comments are prefaced "Au", Smith's comments "Smith", and the original paper "Reinterpreting aircraft measurements in anisotropic scaling turbulence" (Lovejoy et al. 2009) is hereafter designated LTSH.

Au: This referee has a similar view to Lindborg et al. ("Comment on (aforementioned title)", Lindborg, Tung, Nastrom, Cho and Gage, 2010, hereafter LTNCG): i.e. he seems to be in denial of even the possibility that the atmosphere displays scaling anisotropy, indeed of anything beyond the quasi-2D behaviour permitted by the quasi-geostrophic approximation. Until this key underlying issue is dealt with, we cannot even develop a theory realistic enough to confidently interpret aircraft data: our constant slope model is only a crude attempt. Obviously, the hypothesis of isotropic scaling is only a very spe-

cial case of anisotropic scaling. Our analyses and interpretations are therefore general enough to potentially confirm their theories, whereas their analyses and interpretations will only be valid if the assumption of isotropic scaling is correct.

Smith: "This manuscript follows a long series of papers, comments, interactive discussions, reviews and replies, beginning with LTSH. My overall assessment is that the present manuscript offers nothing new to the debate. Rather, Lovejoy et al. merely repeat arguments already made in their replies to the comment on their paper by LTNCG."

Au: What is new here is a) we offer a succinct summary of the debate, b) we refute Lindborg's repeated back of the envelope calculation using drop sonde data and other new empirical arguments. Since this back of the envelope calculation was the only concrete attempt to refute our analyses, the points made in our new manuscript are scientifically important. In our response to referee 2 (below), we also propose two new highly relevant new analyses. Furthermore, the significance of this rather recent and short debate is that it may help to finally conclude a latent debate that has been around for nearly three decades. Finally, in a full length paper in preparation, we go beyond the classical scale analysis (which over a narrow range of scales attempts to determine which terms of the dynamical equations are dominant) to a scaling analysis of the vorticity equation which confirms that the equations are indeed invariant under anisotropic scale changes.

Smith: "The debate that the present manuscript seeks to continue concerns the interpretation of the observed lateral atmospheric energy spectrum."

Au: The debate is actually about whether atmospheric scaling is isotropic (with two or more ranges) or anisotropic with one wide range. The aircraft data cannot be interpreted without at least implicitly making a specific assumption on this point. The interpretation of the "lateral atmospheric energy spectrum" depends on this assumption.

Smith: “Specifically, the contentious issue is the interpretation of the synoptic-scale spectrum, with an exponent observed to be between -2.4 and -3. Lovejoy et al. claim that the entire horizontal atmospheric kinetic energy spectrum, if measured correctly (which in their view means on levels of constant true altitude), is characterized by an exponent of -5/3.”

Au: We already recalled that the exponent value is just based on the dimensions of energy, not only kinetic energy.

Smith: “The apparent -3 exponent, they claim, is actually -2.4, and this is precisely what they predict based on the assumption that measurement aircraft follow a multifractal path on scales up to 40km, and follow isobars on longer horizontal scales.”

Au: Why not?

Smith: “Moreover, the authors seem to contend that a forward energy cascade characterizes the entire range from planetary scales to micro-scales.”

Au: Going back to 1983, we have questioned the relevance of the usual search for energy sources at rather well defined wavenumbers. Such sources are theoretically not needed for canonical cascades (Schertzer et al., 1997) and this is furthermore supported by observations since both the visible and infra red radiances are scaling from planetary scales down to at least a few kilometers (Lovejoy et al., 2009).

Smith: “Such a radical view requires an alternate theory for the large-scale dynamics of the atmosphere, but figuring out what this theory is takes some digging. It turns out to be an amazingly simplistic argument, which I strongly encourage the journal Editor to read directly: it can be found in the first full paragraph on pg. 32 of Lovejoy & Schertzer, 2010: "Towards a new synthesis for atmospheric dynamics: space-time cascades", Atmos. Res., 96, 1–52 (an electronic copy can be found on Prof. Lovejoy’s webpage here: http://www.physics.mcgill.ca/∼gang/Lovejoy.htm). The essential idea is that the Earth receives about 200 W/m2 from the Sun, and if this is spread evenly throughout

the troposphere, and a 2% conversion to kinetic energy is assumed, then the resulting energy dissipation rate matches values measured in small-scale turbulence. That’s it.”

Au: Unfortunately, the referee did not dig in the right direction! What he mentions bears only on the energetic possibility, not on the dynamics! The latter relies on a 23/9-D turbulent cascade (Schertzer and Lovejoy, 1985b), (Schertzer and Lovejoy, 1985a) based on two fluxes: those of energy and buoyancy variance. But contrary to the classical scheme of energy and enstrophy cascades (advocated by the referee), in the 23/9D model the energy and buoyancy variance do not correspond to two separate ranges with isotropic dynamics, but they are combined together to yield a unique anisotropic cascade. Beyond the obvious economy of our model (and much better agreement with observations of all sorts – especially of the horizontal structure from remotely sensed data and of the vertical structure from sondes), a bonus is that it allows for a first principles estimate of the planetary scale wind fluctuations ($\approx$ 20 m/s) as well as the corresponding time scale (the weather/climate transition scale $\approx$ 10 days). In short, the referee has reduced the bonus to the whole theory!

Smith: “Thus, the theory put forth by Lovejoy et al. not only rejects Charney’s theory of geostrophic turbulence, it also effectively rejects all that any atmospheric dynamicist thinks she or he might have known about how the macroscopic atmosphere or ocean or any rotating, stratified fluid works.”

Au: We are currently finalizing a more theoretical reply to LTNCG on the relationships between the quasi-geostrophic approximation and anisotropic scaling, further to a discussion comment (Schertzer, 2009) that was ignored by LTNCG, whereas we pointed with the help of (Smith, 2004) that quasi-geostrophic turbulence did not at all support one of their claims. Therefore, contrary to LTNCG, we do not intend to mishandle the historical break-through represented by the quasi-geostrophic approximation. A key issue is the incompatibility of the quasi-geostrophic approximation with the observed scaling along the vertical. Therefore, one can hardly argue that our model rejects all we “might have known”! In actual fact, our model is simply a generalization and modernization of the highly successful classical laws of turbulence of Richardson, Kolmogorov, Obuhov and Bolgiano.

Smith: “The authors have so successfully covered their simplistic view of atmospheric dynamics in jargon and obfuscation that no one seems to have realized how ridiculous their claim really is.”

Au: We do not understand why the referee ignores a basic refereeing rule, i.e. to avoid being abusive! Who is the referee to act as a self-appointed guardian of the gate? By implication, the 450 citations for the JGR 1987 paper, and the 379 for the next, and the 10 or so further papers with 100+ citations are authored by people who do not meet the referee's criterion. Surely he needs to examine his position in a probabilistic manner?

Smith: “There are important open issues concerning the turbulent spectrum of the atmosphere, and notably, I believe the authors of Li09 (the Comment on LTSH) disagree amongst themselves, and certainly with this reviewer. Still, no serious theory for the observations (including any of the papers cited by Lovejoy et al. to support their own claims) rejects, either implicitly or explicitly, the incredibly successful and multifaceted existing theory of large-scale atmospheric dynamics.”

Au: In as much as our theory is closer to the models and closer to the observations than the quasi-geostrophic theory espoused by the referee, it cannot be said that we reject “existing theory of large-scale dynamics” rather we consider the series of approximations leading to quasi-geostrophic theory as historically important, but unfortunately with limitations which we discuss in detail elsewhere.

Smith: “The point of view espoused by Lovejoy, Schertzer and Tuck is apparently rooted in a multi-decade attempt to apply multifractal scaling analysis to every nonlinear process in geophysics, not in a deep reconsideration of atmospheric dynamics. Such a contrarian theory for the observations must satisfy a very high burden of proof to qualify for publication in any reputable journal on atmospheric dynamics.”

Au: There is no historical support to the referee's claim. Everything happened in the opposite order: multifractals originated from attempts to model and understand turbulent intermittency and anisotropic scaling arose from attempts to understand atmospheric stratification. Both correspond to major paradigm changes. The authors are therefore not frantically “applying multifractal scaling analysis to every nonlinear process in geophysics”, it is more the referee who attempts to turn his back on 25 years of advances in our understanding of turbulence.

Smith: “In my opinion neither the initial paper, Lo09, nor any of the many recent papers they have published on this subject should have been accepted for publication in atmospheric journals. Certainly, the present comment should be rejected.”

Au: It is a pity that the referee not only did not bring any constructive comments, but is willing to reject all papers pointing out the limitations of an approximation, which in the present case is the quasi-geostrophic one.

References:

S. Lovejoy et al., Atmospheric complexity or scale by scale simplicity? , Geophys. Resear. Lett. 36(2009), pp. L01801, doi:01810.01029/02008GL035863.

D. Schertzer, Interactive comment on “Comment on Reinterpreting aircraft measurements in anisotropic scaling turbulence" by Lovejoy et al. (2009)” by E. Lindborg et al., Atmos. Chem. Phys. Discucss. 9(2009), pp. C8605–C8610.

D. Schertzer and S. Lovejoy, Generalised scale invariance in turbulent phenomena, Physico-Chemical Hydrodynamics Journal 6(1985a), pp. 623-635.

D. Schertzer and S. Lovejoy, The dimension and intermittency of atmospheric dynamics. In: B. Launder, Editor, Turbulent Shear Flow 4, Springer-Verlag (1985b), pp. 7-33.

D. Schertzer, S. Lovejoy, F. Schmitt, Y. Chigirinskaya and D. Marsan, Multifractal cascade dynamics and turbulent intermittency, Fractals 5(1997), pp. 427-471.

K.S. Smith, Comment on: "The k-3 and k-5/3 energy spectrum of atmospheric turbulence: Quasigeostrophic two-level model simulation", J. Atmos. Sci 61(2004), pp. 937-941.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7495, 2010.