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Atmospheric Chemistry and Physics Discussions



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

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This manuscript follows a long series of papers, comments, interactive discussions, reviews and replies, beginning with "Reinterpreting aircraft measurements in anisotropic scaling turbulence" (Lovejoy et al. 2009 – hereafter Lo09). 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 Lindborg et al. ("Comment on [aforementioned title]", Lindborg, Tung, Nastrom, Cho and Gage, 2010 — herafter Li10).

The debate that the present manuscript seeks to continue concerns the interpretation of the observed lateral atmospheric energy spectrum. 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 — 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. Moreover, the authors seem to contend that a forward energy cascade characterizes the entire range from planetary-scales to micro-scales.

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/m$^2$ 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.

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. 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.

There are important open issues concerning the turbulent spectrum of the atmosphere — and notably, I believe the authors of Li09 (the Comment on Lo09) disagree amongst



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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. 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.

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.

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