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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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Reply by S. Lovejoy, A. Tuck and D. Schertzer

Starting with [Taylor, 1935], the paradigm of isotropic (and scaling!) turbulence was developed initially for laboratory applications, but following [Kolmogorov, 1941], three dimensional isotropic turbulence was progressively applied to the atmosphere. Since the atmosphere is strongly stratified, a single wide scale range model which is both isotropic and scaling is not possible so that theorists had to immediately choose between the two symmetries: isotropy or scale invariance. Following the development of models of two dimensional isotropic turbulence ([Fjortoft, 1953], but especially [Kraich-



nan, 1967] and [Charney, 1971]), the mainstream choice was to first make the convenient assumption of isotropy and to drop wide range scale invariance; below these approaches are collectively referred to as the "IP" ("isotropy primary") paradigm. Starting at the end of the 1970's this has lead to a series of increasingly complex isotropic 2D/isotropic 3D models of atmospheric dynamics which continue to dominate the theoretical landscape. Justifications for IP approaches have focused almost exclusively on the horizontal statistics of the horizontal wind in both numerical models and analyses and from aircraft campaigns, especially the highly cited GASP [Nastrom and Gage, 1983], [Gage and Nastrom, 1986; Nastrom and Gage, 1985] and MOZAIC [Cho and Lindborg, 2001] experiments. Since understanding the anisotropy clearly requires comparisons between horizontal and vertical statistics/structures it is not surprising that this focus has been unfortunate.

Over the same thirty year period that 2D/3D isotropic models were being elaborated, evidence slowly accumulated in favour of the opposite theoretical choice: to drop the isotropy assumption but to retain wide range scaling. The models in the alternative paradigm are scaling but strongly anisotropic with vertical sections of structures becoming increasingly stratified at larger and larger scales albeit in a power law manner; we collectively refer to these as "SP" for "scaling primary" approaches. Early authors explicitly using SP models to explain their observations include ([Van Zandt, 1982], [Schertzer and Lovejoy, 1985], [Schertzer and Lovejoy, 1987], [Fritts et al., 1988], [Tsuda et al., 1989], [Dewan, 1997; Lazarev et al., 1994], [Gardner et al., 1993], [Hostetler and Gardner, 1994]. In addition, many experiments found non- standard vertical scaling exponents thus implicitly supporting the SP position, see the review (and many additional references) in [Lilley et al., 2008]. Today, state-of-the-art lidar vertical sections of passive scalars [Lilley et al., 2004] or satellite vertical radar sections of clouds (the appendix of [Lovejoy et al., 2009d], hereafter referred to as LTSH; see also [Lovejoy et al., 1987]) give direct evidence for the corresponding scaling (power law) stratification of structures. State-of-the-art drop sondes have even been used to show that the IP standard bearer - 3D isotropic Kolmogorov turbulence - apparently doesn't exist in the atmosphere at any scale at least down to 5 m in scale or at any altitude level within the troposphere [Lovejoy et al., 2007]. It has been further argued that this absence of isotropy extends down to much smaller scales on the basis of results in molecular dynamics [Tuck, 2008]. At the same time, massive quantities of high quality satellite data have directly demonstrated the wide range horizontal scaling of the atmospheric forcing (long and short wave radiances; see e.g. [Lovejoy et al., 2009a]) and numerical atmospheric models and reanalyses have been shown to display nearly perfect (scaling) cascade structures over their entire available horizontal ranges [Stolle et al., 2009]. This shows also that the source/sink free "inertial ranges" used in IP models are at best academic idealizations.

The IP/SP opposition is arguably a main contributor to today's lack of scientific consensus about the scale by scale statistical structure of both the atmosphere and of atmospheric models and reanalyses. In order to resolve the deadlock, either the IP camp must show how the findings of wide range vertical and horizontal scaling can be adequately explained through a hierarchy of isotropic models, or the SP camp must explain the key aircraft and numerical model results cited against them as evidence of two (or more) isotropic regimes. In LTSH, we claimed to have found exactly such a reinterpretation, decisively resolving the contradiction in favour of the SP approaches. This claimed resolution has now resulted in a defence of the IP view [Lindborg et al., 2009] (hereafter referred to as LTNCG). Although we welcome this opportunity for debate, we were disappointed that the issues at stake were reduced to the narrow subject of aircraft measurements of the horizontal wind and to our purportedly "clever reinterpretations" so that already by their second paragraph LTNCG avail themselves of a technical point - which we believe to be incorrect - to justify a refusal to "go into any further technical details" i.e. to avoid a substantive discussion.

Before discussing LTNCG's comments further, let us clarify the implications of the SP approaches and the key IP/SP differences. In the anisotropic scaling models, the fluctuations $\Delta f$ over a lag $\Delta x$ of various observables $f$ (such as the horizontal wind) vary



in the horizontal as $\Delta f(\Delta x) = \varphi_h \Delta x^{H_h}$ whereas in the vertical with lag $\Delta z$ they vary as $\Delta f(\Delta z) = \varphi_v \Delta z^{H_v}$ where $H_h$, $H_v$ are horizontal and vertical exponents and $\varphi_h$, $\varphi_v$ are turbulent fluxes which dominate the statistics in the corresponding directions. Ignoring intermittency corrections (which for the spectral exponents are typically of the order 0.1), these correspond to horizontal and vertical spectra of the forms $E_h(k_x) = k_x^{-\beta_h}$, $E_v(k_z) = k_z^{-\beta_v}$ where $E_h$, $E_v$ and $k_x$, $k_z$ are corresponding horizontal and vertical spectra and wavenumbers and the exponents $\beta$ are related to the fluctuation exponents $H$ by $\beta = 1 + 2H$. The isotropic 3D and 2D turbulence models are the special cases where for 3-D isotropic turbulence $H_h = H_v$ and for 2-D isotropic turbulence, $H_v \to \infty$ (indicating either statistical or deterministic homogeneity in the vertical direction so that the fluctuations are independent of $\Delta z$, the structures are purely horizontal). The familiar models for $f = v$ = the horizontal velocity are Kolmogorov's 3D isotropic turbulence model with, $\varphi_h = \varphi_v = \epsilon^{1/3}$, $H_h = H_v = 1/3$ and Charney's geostrophic turbulence model with $\varphi_h = \eta^{1/3}$ and $H_h = 1$ where $\epsilon$, $\eta$ are respectively the energy and potential enstrophy fluxes.

In order to model a wide range of scales, IP approaches involve nontrivial combinations of small scale 3-D isotropic energy flux cascades ($k^{-5/3}$), a medium scale, 2- D isotropic enstrophy flux cascade ($k^{-3}$) followed at still larger scales by a 2D isotropic energy flux cascade (again $k^{-5/3}$; here from smaller to larger scales, the cascade is "indirect"). In comparison, SP approaches involve a regime with wide range scaling but with different dominant horizontal and vertical fluxes and exponents (several different regimes could in principle be used but this seems unnecessary). In the strongly turbulent SP "23/9D model" [Schertzer and Lovejoy, 1985], $\varphi_h = \epsilon^{1/3}$, $H_h = 1/3$ (Kolmogorov in the horizontal) but $\varphi_v = \phi^{1/5}$, $H_v = 3/5$ ([Bolgiano, 1959; Obukhov, 1959] in the vertical, $\phi$ is the buoyancy variance flux) whereas in the weakly turbulent quasi-linear gravity wave models – either the diffusive filtering theory (DFT) [Gardner, 1994] or saturated gravity wave theory (SGW) [Dewan, 1997], $\varphi_h = \epsilon^{1/3}$, $H_h = 1/3$ but $\varphi_v = N$, $H_v = 1$ where $N$ is the Brunt-Väïsälä frequency.

In order to respect anisotropic scaling symmetries, the SP approaches replace the usual Euclidean distance notion of size by a scale function (eqs. 6, 7 of LTSH). The scale function involves a "sphero-scale", $l_s$ where horizontal and vertical fluctuations are equal. The point of LTSH was to show that under rather general conditions - the existence of small but nonzero aircraft slopes ($s$) and small $l_s$ - that for distances $> \Delta x_c$ (from eq. 7 LTSH we find $\Delta x_c = l_s s^{(1/(H_z-1))}$ that the aircraft will spuriously measure the vertical exponent of the horizontal wind, i.e. $\approx$ 2.4 instead of the correct horizontal exponent $\beta_h = 5/3$ (actually, empirically $\beta_v$ increases from $\simeq 2.15 \pm 0.04$ to $\simeq 2.48 \pm 0.03$ from the surface to 12 km, see [Lovejoy et al., 2007], the value 2.4 is an average over several upper tropospheric layers).

While IP approaches involve successions of (at least three) different regimes, with (at least) two different flux sources and (at least) three different flux sinks, SP approaches are parsimonious. Indeed one of their attractive features is that by retaining the horizontal dominance by energy flux $\epsilon$, starting essentially from first principles they correctly predict the typical intensity and lifetimes of planetary scale structures (see [Lovejoy and Schertzer, 2009]). This means that small scale measurements of the solar constant and the efficiency by which the incoming solar energy is converted into mechanical energy - when combined with $\beta_h = 5/3$ to planetary scales – can accurately be extrapolated to planetary scales where they predict fluctuations of $\pm 20m/s$ at antipodes and lifetimes of planetary structures of $\simeq 10^6 s$ ($\simeq$ two weeks).

Much of the LTNCG's commentary consists of interpretations of historic atmospheric analyses and aircraft campaigns starting in the 1960's. However, when reviewing historical data, their context must be taken into account. For example, strong intermittency implies that spectra and other turbulent statistics require massive data sets to ensure adequately sampling, and such data have only recently become available. It was therefore natural for early workers to focus on testing existing theories, hence the systematic emphasis on comparisons with (the then) new theoretical developments in 2D turbulence theory predicting a transition from a $\simeq k^{-5/3}$ to $\simeq k^{-3}$ regime. It wasn't until the



1980's that vertical spectra with $\beta_v \simeq 2.4$ began to appear, and it was only much more recently that the altitudes of aircraft could be estimated accurately enough to make the detailed – and probably decisive - LTSH studies of the coherency and phase relations between aircraft measurements of pressure, altitude and wind. Now that a straightforward explanation of the ubiquitous exponent 2.4 is finally available, we can return to the classics and use our eyes to see, not simply "shoe-horn" the results into a 2D/3D isotropic mould.

As an early example of this $\beta_h = 3$ shoe-horn, consider the cited [Julian et al., 1970] paper which did indeed conclude that $\beta_h$ was in the range 2.7 to 3.1. However this conclusion was made on the basis of "eyeballing" spectra over the range k = 8 to 15 i.e. over less than an octave in scale. Although the temptation to shoe-horn otherwise ambiguous results into the IP paradigm was – and still is – strong, the more careful workers nevertheless were cautious. For example the spectra in the path breaking [Boer and Shepherd, 1983] paper (cited in LTNCH) - which is in actual fact almost exactly $k^{-2.4}$ (see [Lovejoy et al., 2009c]) – was already recognized by its authors as being "too shallow", we return to their comments below.

Prominent in LTCNH commentary is the undoubtedly single most frequently cited study on the horizontal wind spectrum: the GASP experiment [Nastrom and Gage, 1985], [Gage and Nastrom, 1986] which used commercial aircraft to measure wind spectra. GASP flights were divided into short, medium and long range categories; the highly cited spectrum is an ensemble using all three categories combined. In [Lilley et al., 2008] it was already pointed out that this combined spectrum actually only had a very narrow (an octave or so in scale) $\beta_h \simeq 3$ part whereas $\beta_h \simeq 2.4$ was quite accurate over most of the range $k < (100km)^{-1}$. However, an even more striking result is obtained by considering - the presumably more appropriate - long haul flight spectrum (up to 4800 km, [Gage and Nastrom, 1986]). In section 5 of LTSH it was shown that this followed nearly exactly $\beta_h \simeq 2.4$ without any hint of $\beta_h \simeq 3$ all the way down to the low wavenumber limit. Strangely - while failing to notice this from section 5 of the

LTSH paper - to support their case LTNCG mention that [Nastrom and Gage, 1985] had investigated the effect of vertical motions of the aircraft by comparing spectra from flight segments along isobars with segments having large deviations from isobars, finding no significant differences. Since according to the SP approaches, the reason that at large enough scales, aircraft spectra have $\beta_h \simeq 2.4$ is precisely because of their excursions in the vertical: this finding actually gives the SP models strong support! Finally, the use of ECMWF interim reanalyses has enabled us to show that for $k > (5000km)^{-1}$, $\beta_{h,p}$ ("p" for on isobars) and $\beta_v$ differ by less than 0.1 over the lower 5 km of the troposphere - a range over which $\beta_v$ varies from 2.15 to 2.43 (the value 2.4 is only an average [Lovejoy et al., 2009b]).

Similarly, when interpreting their reanalyses, [Strauss and Ditlevsen, 1999] state in their abstract that $\beta_h \simeq 2.5 - 2.6$, a value "significantly different than the classical turbulence theory prediction of -3" but close to our predicted value. Ironically some of the LTNCG authors themselves [Cho and Lindborg, 2001] indulged in some shoe-horning when they forced nearly perfectly $\beta_h = 2.4$ MOZAIC statistics into a $\beta_h = 3$ mould by the use of log "corrections". As pointed out in [Lilley et al., 2008] (and revisited in LTSH), their approach should more properly be called a "logarithmic model with power - law corrections": their logarithmic terms were so strong that their variances become negative just a little beyond the end of the range displayed in their figures (at about 4000 km).

The cited [Skamarock, 2004], [Takayashi et al., 2006] and [Hamilton et al., 2008] papers bring up yet another issue - albeit somewhat tangential to the discussion: whether or not numerical models are even capable of reproducing both $\beta_h = 5/3$ and $\beta_h = 3$ regimes (some apparently are not [Palmer, 2001]). This issue arises because of possible internal contradictions within the IP approach. For example, [Bartello, 1995], and [Ngan et al., 2004] have pointed out the possibility that the small scale isotropic 3D regime could destabilize any large scale 2D isotropic regime. Here, it suffices to note that (as discussed in detail in [Lovejoy et al., 2009b]) that the [Skamarock, 2004],



WRF (regional) model spectra are in fact very close to $k^{-2.4}$, (with tiny $k^{-3}$ ranges) while the earth simulator results in [Takayashi et al., 2006] and [Hamilton et al., 2008] have the poorest fit to (GASP) observations precisely over the range ≈ 400 – 3000 km which their (painstakingly crafted) $k^{-5/3}$ to $k^{-3}$ transition is supposed to explain. In other words, this model "success" may make them less rather than more realistic!

Throughout their commentary, the only attempt by LTNCG to actually refute our analyses is in their penultimate paragraph. Rather than discussing the issue of anisotropic turbulence (which is nowhere even acknowledged), they focus on the isobar/isoheight distinction, strangely summarizing our position as affirming that a "spurious scaling exponent is somehow introduced because the aircraft follow isobars rather than isoheights". Actually, the detailed explanation was given (eq. 7 in LTSH) and most of LTSH consisted of quantitative validation of the precise equation 7. The issue is not one of isobars versus isoheights, but rather the consequences of the fact that $\beta_v \neq \beta_h$. For the LTSH explanation to work, it suffices that the aircraft slopes (whether on isobars or not) are sufficiently large and the sphero-scale ($l_s$) sufficiently small. But by direct measurement, we show that they are indeed of the correct magnitude so that our model quantitatively accounts for all the observations. Their back-of-the-envelope calculations are simply not adequate. To see this, first consider the average trajectory statistics in LTSH the mean aircraft slope .2 $\cdot 10^{-4}$ is typical for isobars (averaging over $\pm 45^o$ latitude, the ECMWF interim reanalysis yields mean isobaric slopes varying from .5 $\cdot 10^{-4}$ to .0 $\cdot 10^{-4}$ for resolutions 0 -$10^4$ km respectively). The mean sphero-scale was found to be 0.1 m (at 167 km resolution the ECMWF interim reanalysis gives $l_s$ = 0.07 m) and our best estimate for $H_z$ was 0.45. Using the admittedly highly simplified model that the aircraft is on a constant slope trajectory, we find a critical transition scale of $\simeq 1000km$ implying that at larger scales, isobars would be expected to show spurious vertical rather than horizontal statistics. This shows that even in the mean, the authors' back-of-the- envelope calculation is inadequate. However, as repeatedly stressed in LTSH, both the slopes s and the value of ls are in fact highly variable so that our figure of 40 km for the ensemble transition scale from $\beta_h = 5/3$ to 2.4 must be seen as a kind of complex nonlinear average involving trajectories which are more fractal than linear over much of their range. A final effect - noted in LTSH but ignored by LTNCG – is that there are good meteorological reasons to expect a correlation between the isobaric slopes and the horizontal wind (the implications of geostrophy plus hydrostatic equilibrium) and these will modify the situation even further. In short, the inter-relation of turbulence and its measurements requires an explicit theory of aircraft motions in atmospheric turbulence. Up until now, the data have been interpreted with naïve isotropic assumptions: LTSH is simply a first attempt to go beyond this using more realistic anisotropic models. Given the large body of data indicating vertical anisotropy, LTNCG's complacent view of the data is simply an act of faith.

To conclude, let us simply cite [Boer and Shepherd, 1983] who could not possibly have been aware that a simple theory could explain their $\simeq k^{-2.4}$ results: "For the purposes of comparison with theory, the spectral slopes obtained from the data are somewhat shallower than the values of -3 suggested by simple theory. It must be emphasized however, the enstrophy containing inertial subrange is not really a prediction for the atmosphere but is a possible solution to the spectral equation in an unforced subrange which may or may not have some correspondence to the situation in the real atmosphere. Consequently, the fact that the spectra obey power laws at all may be considered to be a striking, although by now well known feature of the atmosphere."

References:

See the pdf attachment.

Please also note the Supplement to this comment.

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