Comments on A global analysis of the fractal properties of clouds revealing anisotropy of turbulence across scales

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Overall comments:

As a preface, I recognize that I am a invested protagonist in the science reported here, so please take these comments as helpful suggestions, not in the spirit of anonymous referee comments.

This paper is a welcome update on a key question of atmospheric dynamics: over what ranges are they scaling? The key finding is that observations of cloud radiances over a huge range of horizontal scales are indeed scaling. This vindicates Richardson's wide range scaling hypothesis updated as confirmed by Lovejoy's 1982 area-perimeter analysis (and numerous spectral and other analyses since). Wide range scaling is incompatible with the still prevalent 2D isotropic/3D isotropic paradigm that necessarily involves a "dimensional transition" somewhere in the mesoscale. The question is which symmetry is dominant: the scale symmetry or the rotational symmetry? Richardson believed it was scaling. Following the isotropic 2D Kraichnan 1968 model, and Charney's 1971 quasi-geostrophic variant, the atmospheric community has largely considered isotropy to be the dominant symmetry, thus implying an elusive dimensional transition/scale break somewhere near the mesoscale. This paper contradicts the latter hypothesis but supports the former. It would be worth bringing this out in the introduction, it will enhance the significance of the work.

My main issue with the paper is that it is monofractal – both in the theoretical model as well as in the data analysis. This aspect with respect to both area-perimeter relations as well as Korcak laws was considered in some detail in several appendices to [*Lovejoy and Schertzer*, 1991]:

http://www.physics.mcgill.ca/~gang/eprints/eprintLovejoy/neweprint/NVAGlovejoyall.pdf

The main conclusion relevant to this paper is that the interpretation of the area-perimeter (A-P) exponent, is quite different when monofractal models (such as fractional Brownian motion), or when multifractal models are used. In the former case (assumed here), the cloud regions exceeding a threshold are assumed to be non fractal (they are assumed to be true (2D) areas, with dimension = 2) so that the usual interpretation of the A-P exponent = D/2 is valid. However if clouds are multifractal, then for any exceedance threshold that define "clouds", the A-P exponent is the ratio D(P)/D(A) of the perimeter dimension D(P) to the fractal dimension of the exceedance set D(A). Since D(P) and D(A) will both decrease with the brightness threshold that defines the sets, the ratio may be quite stable over a range of thresholds, potentially explaining the robustness of the A-P exponent.

The authors may want to reflect and comment on this?

Minor comments (These are in the attachment).
(The line numbers are with respect to the second version of the manuscript).

I have few additional minor comments that the authors could address:

Line 55. "The multifractal nature of clouds and their apparent size and type dependence of D seem to contradict the argument that cloud geometries are scale invariant."

This is a nonsequitor: by definition, multifractals are scale invariant. What did you mean to say?

Line 60: "Indeed, the topic of whether or how scale invariance applies to atmospheric structures has been the topic of decades of debate (Lovejoy and Schertzer, 2018)."

In the turbulence community, scale invariance itself is a mainstay for all the theories, the question is the type and range(s) of the scaling: the standard 2D isotropic / 3D isotropic turbulence model with dimensional transition somewhere in the meso-scale versus a single wide range but anisotropic scaling regime (the 23/9D) model. The debate is about the type of scaling: anisotropic or isotropic, the limits of the scaling regime(s) and the values of the scaling exponents.

Note: there is no Lovejoy and Scherzter 2018, you seem to be referring to Lovejoy and Schertzer 2013; please change this throughout the text.

Line 72: Eq. 2 needs an absolute value sign around the difference. In addition, H is only the usual Hurst exponent in the nonintermittent (Gaussian) case. In equation, the H is inspired by Hurst, but is not the same. Also, if fluctuations are defined by other wavelets (i.e. not the differences as indicated), then H can in principle take any real value, the range $-1<H<0$ being particularly important in the macroweather regime.

Line 83: The law eq. 3 ignores intermittency, it is at best an average law. Statistics of other orders will presumably define a hierarchy of (multifractal) exponents. Your mention of the dimensionality is in fact a reference to the 2D isotropic/ 3D isotropic versus 23/9D debate.

Line 94: The correct reference for the spurious nature of the scale breaks in aircraft data is [*Lovejoy et al.*, 2009].

Line 100: The expression "intermediate turbulence regime" is unfortunate since readers will likely think this is a regime intermediate in *spatial scales* whereas I understood (only later in the text) that you meant intermediate in the value of the dimension (i.e. 3>23/9>2). The key point to make here is that rather than 2 isotropic regimes separated somewhere in the meso-scale, a single (much wider scale range) anisotropic regime was proposed.

Line 104: The 23/9D model proposes that the volume of NONfractal structures scales as $L^{23/9}$. 23/9 is an upper bound on the dimension of the (sparse) fractal structures (i.e. rather than the usual upper bound of D =3). In the 23/9D model, only structures with D<23/9 are fractal.
Also, the exponent is Hz, not H so that it is NOT a Hurst exponent. In the equation "D = 2.55 = 2+H" , H is in fact a RATIO of exponents H_z $=H_{hor}/H_{vertical}$. I'm puzzled because later in the paper, this fact is acknowledged. In terms of the spectral exponents B, the relationship is H_z = $(B_{vertical}-1)/(B_{horizontal}-1)$ (this is true for both monofractal and multifractal variants of the 23/9D model).
I could also note that the relationship $B = 1+2H$ is only valid for the Gaussian (nonintermittent, nonmultifractal) case (this should be stated), otherwise the are intermittency corrections that are (inconsistently) invoked later (line 111).

Line 110: Eq. 4 applies to the fractal dimension of the geometric set of points on the graph (x,B(x)) where x is the position in a 1-D cloud transect), B is the brightness of 1-D transects through monofractal cloud such as a fractional Brownian motion (fBm) cloud with structure function exponent H. In this case, the fractal dimension of the set of "zero-crossing points" (the intersection of the line B=T = constant with the cloud brightness B(x) is D = 1-H for any threshold T. That is why fBm is a monofractal function. If this fBm model is extended to two dimensional space B(x,y) then the codimension is still H, so that the dimension of the zero-crossing sets (the perimeter set) is independent of the brightness threshold.

Line 150: The nondimensionalization is not only a question of convenience. If the process is multifractal, the key scale is the outer scale and the dissipation plays the role of small cut-off. At any intermediate scale (between the smallest dissipation scale and the outer scale, only the outer scale intervenes, not the inner scale.

Line 184: The intermittency correction arises because turbulence is multifractal, not monofractal.

Line 188: The quantity 3-D is the fractal codimension of a fractal set embedded in a three dimensional space. In (multifractal) turbulence, the codimension is in fact a function (not a unique value) that depends on the threshold used to define the fractal set. At best this equation is useable for a Gaussian model.

Line 354: "Because stratification is only observable in vertical velocity perturbations".
I don't understand: the role of vertical velocity is not clear, and the data on vertical velocities is inadequate. However, the fact of scale dependent stratification and the key H_z parameter (the ratio of horizontal and vertical scaling exponents) has been estimated in several fields:
Temperature, potential temperature, humidity, horizontal velocity, lidar reflectivity (aerosols), radar reflectivity (clouds). This is reviewed and summarized in ch. 6 of [*Lovejoy and Schertzer*, 2013], see in particular, table 6.5.

Eq. 15: The Richardson law is nearly equivalent to the Kolmogorov law. In the 23/9D model, the standard Kolmogorov law holds in the horizontal (but not vertical), and therefore, we expect the Richardson 4/3 law to hold in the horizontal (but not vertical). Using your eq. 8, we expect the vertical exponent to be 1+3/5 = 8/5 rather than the horizontal value 4/3 (the value 14/9 is not justified). Here you imply the existence of an isotropic Richardson law that would certainly contradict the highly anisotropic 23/9D model.

Line 353: maybe stress that these exponents correspond to the horizontal velocity component with the subscript only indicating the direction of the separation.

References:

Lovejoy, S., and Schertzer, D., Multifractal analysis techniques and the rain and clouds fields from 10^{-3} to 10^{6}m, in *Non-linear variability in geophysics: Scaling and Fractals*, edited by D. Schertzer and S. Lovejoy, pp. 111-144, Kluwer, 1991.
Lovejoy, S., and Schertzer, D., *The Weather and Climate: Emergent Laws and Multifractal Cascades*, 496 pp., Cambridge University Press, 2013.
Lovejoy, S., Tuck, A. F., Schertzer, D., and Hovde, S. J., Reinterpreting aircraft measurements in anisotropic scaling turbulence, *Atmos. Chem. and Phys.* , *9*, 1-19, 2009.