Review

Title: Fluctuations of radio occultation signals in sounding the Earth's atmosphere

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The authors have done a good job in addressing the concerns I raised in my first review. I have two more significant concerns along with several grammatical corrections I missed in the first pass.

My first concern is that I believe the authors misapply the concept of the outer scale in the prescriptions of the internal gravity wave (IGW) and isotropic turbulence power spectral densities. This concern may not actually originate with the authors but with a fairly long list of authors that preceded them in the field of random fluctuations in nonhomogeneous media. Specifically, the outer scale L approximates the correlation length scale of the random atmospheric fluctuations, and it is *not* the vertical depth of the layer in which the fluctuations occur. The trouble in this paper enters in the prescriptions for the outer scale of the IGW spectrum: the authors take 4 km as an upper limit on the outer scale, but depth is more reminiscent of the depth of a breaking gravity wave layer than the correlation length of the breaking gravity waves. One can demonstrate this by estimating the temperature fluctuations associated with breaking gravity waves. Following the same notation as in the paper,

$$\sigma_T \approx \frac{\omega_B^2 T}{g} L .$$

Using values typical of the stratosphere and L = 4 km, the standard deviation of temperature is approximately 40 K, an absurdly large value and far greater than anything ever seen in the stratosphere. More typical numbers are 1–2 K. This will impact the prescription of the IGW spectral density throughout the paper. I do not expect it to affect the central conclusion of the paper, however.

My second concern is that the authors mistakenly prescribe the outer (L) and inner scales (l) of the Kolmogorov turbulence independently. Based on mixing length theory and the intuition that the turnover time of the largest eddies in the turbulence are the inverse of the Brunt frequency ω_B, the ratio of the outer to the inner scales of turbulence is given by

$$\left(\frac{L}{l}\right) \approx \left(\frac{\varepsilon_K}{\nu_a \omega_B^2}\right)^{3/4} \approx Re^{3/4}$$

with *Re* the Reynolds number of the turbulence. The quantities ε_K and ν_a are the energy dissipation rate of the turbulence and the kinematic molecular viscosity of the atmosphere. (It is likely that the Reynolds number should be normalized by a large number, ~2000, because that is the critical threshold for the onset of turbulence.) This, too, should affect all of the calculations of the spectral density of the turbulence. Again, I do not expect it to affect the central conclusion of the paper.

Grammar

Page 2, line 19: "determined by radiation shot noise"

Page 3, line 7: "not a quantitative study"

Line 10: “~~the~~ weak fluctuation theory”

Line 13: “relative contributions to RO signal fluctuations of isotropic”

Line 19: “approximation of ~~the~~ weak fluctuation”

Line 23: Is the “regional and seasonal statistical average” the relevant average? If so, then ν will contain massive contributions from planetary waves and synoptic disturbances that are neither gravity waves nor Kolmogorov turbulence.

Page 4, line 1: “…is locally homogeneous embedded in a spherically symmetric background.”

Line 7: “…the anisotropy coefficient defined as…”

Line 10: “The function ϕ determines…”

Line 16: “…scales of the IGW model…”

Lines 16-17: “We will show that RO signal fluctuations are determined primarily by the Fresnel scale ρ_F and the outer scale L.”

Line 18: Spell out “kilometers”.

Line 21: “…result in saturation of the eikonal…”

Line 28 (equation 3): Shouldn’t the structure constant depend on the outer scale as well?

Lines 29-30: “gravitational acceleration”

Everywhere: Instead of “sphericity”, call it the “along-track curvature of the Earth”. There is no need for the Earth to be a sphere in this geometry when only relying upon cylindrical symmetry in the vicinity of the occultation.

Page 5, lines 3-4: “the exponent of a purely power-law spectrum must lie between 3 and 5”

Lines 7-8: “refractivity fluctuations and the spectrum (2)…”

Line 15: “several hundred kilometers.”

Line 16: “~~and equals~~ about 3000 km.”

Lines 25-26: “regular variation of refraction with altitude. Only when evaluating the phase shift (eikonal) is it necessary to take the Earth’s curvature into consideration.”

Line 27: “…are considered weak if their variance…”

Line 34: “…especially in the tropics…”

Page 6, lines 4-5: High velocity compared to what? I’m pretty sure the descent velocity is large compared to the atmospheric motions associated with the refractivity inhomogeneities.

Section 3.1: Be a bit more careful in your definitions of y and z.

Page 7, line 7 (equation 6): Isn’t the eikonal defined to within an additive constant? If so, how can this equation be correct? Is there a way to write this in terms of the defocusing factor instead?

Page 8, line 5: “…such a steep 3D spectrum ($\mu = 5$) are determined…”

Line 12: “…eikonal fluctuation spectrum…”

Page 9, line 4: “described”

Line 11: “…inner scale in Eq. (2)…”

Equations 15, 16: These look like integral equations but without the integral.

Page 10, line 10: “$A = 0.033$”

Page 11, line 22: “…with a scale height of 2.5 km.”

Line 24: As above; the associated temperature fluctuations would be ~40 K. I would guess that the actual outer scale is ~100 m and the breaking gravity waves occur in a 4-km layer.

Line 30: What is "dry optical turbulence"?

Line 31: An outer scale of 1 km for Kolmogorov is associated with an energy dissipation rate integrated in the vertical of 800 W m^{-2}. That's an enormous amount, far greater than the energy available to the atmosphere, especially when this is considered as globally representative. I've assumed that the turnover time of the largest eddies is $\omega_B^{-1} \sim$ 50s.

Page 12, line 11: "…and can be attributed to…"

Line 25: "…and region."

Line 27: "Significant variability is observed…"

Page 14, line 16: "The noise spectrum was estimated from…"

Figures 2 and 3 captions: Panels A and B do not *refer* to the isotropy hypothesis and the anisotropy hypothesis. The only difference is the horizontal coordinate ("abscissa"). I believe what you meant to say is something like, "If the refractivity inhomogeneities were isotropic, then the spectral density curves of figures 2 and 3 would be maximized at the same oblique wavenumber (panel A); if instead they are anisotropic, then the curves would be maximized at the same vertical wavenumber (panel B)." It's the strongest argument in the paper.

Page 16, line 26: "…excess phase as the eikonal."

Line 34: "…tenths of a percent…"

Page 17, lines 18-19: The difficulty arises not from the steep slopes but from the lack of range in the spectral interval.

Page 19, line 25: "…open new pathways in the development…"

Page 20, lines 3,4: "degrees", not "degree".

Line 5: "…indicate strong anisotropy…"

Line 10: "…at low altitudes…"

Line 13: "visible occultations."

Line 22: "visible", not "optics"

Line 29: "The use of a mean eikonal obtained by sliding averages with…"

Line 35: "…have nearly the same periods."

Page 21, line 1: "In the tropical lower troposphere…"

Line 13: "…in the framework of the thin phase screen and weak fluctuation approximations…"

Lines 18-19: "This was demonstrated previously by Steiner et al. (2001), who, for the stratosphere, in the altitude range 15–30 km, …"