Response to the reviewers comments on the paper "Fluctuations of radio occultation signals in sounding the Earth's atmosphere" by V. Kan, M. E. Gorbunov, and V. F. Sofieva

Reviewer #1

The authors should present a justification of the spectrum of internal gravity waves (IGW) that they incorporate. For Kolmogorov turbulence, description of its fluctuations by empirical power law is satisfactory. For gravity waves, the case for its description is more difficult. The "universal spectrum" of gravity waves is valid where gravity waves break, due to either Kelvin-Helmholtz or simple convective instability. Its power density spectrum follows a −3 power law in vertical wavenumber; however, its power density spectrum most assuredly does not follow a −3 power law in horizontal wavenumber as incorporated in this manuscript. The power density spectrum in the horizontal rather follows a form that is characteristic of the original source of the gravity waves. Moreover, IGWs break at different levels depending on the strength of their source: moist convection, orographic, jet stream breakdown. Even though it is probably discussed in the some of the papers they cite, the authors should nevertheless offer some justification for assuming the form of the power spectral density of the IGW in horizontal wavenumber as they did ....

In the Discussion, we added the following text:

For anisotropic inhomogeneities, we employ an empirical model of saturated IGWs (2). Models of this type are widely used for the analysis of stellar and radio scintillations, the angular dependence of the back-scattering of radar signals, the retrieval of model parameters from occultations etc. 1D vertical and horizontal spectra of this model follow the −3 power. However, air-borne observations (e.g., Nastrom and Gage, 1985; Bacmeister et al., 1996) indicate that the horizontal spectra of temperature fluctuations in the troposphere and stratosphere have a power spectrum with a slope close to −5/3 in a wide range of scales from several km to several hundred km (see also the "saturated-cascade" model of Dewan, 1994). In addition, the model (2) has a constant anisotropy. As noticed in section 2.1, observations of stellar occultations with tangential geometry (Kan et. al, 2014), together with the data about the anisotropy of dominant IGWs (e.g. the description of CRISTA experiment in Ern, et al., 2004; GPS occultations in Wang and Alexander, 2010), have revealed that the anisotropy coefficient is not uniform. It increases from about 10–20 for the IGW breaking scale (10–20 m in the vertical direction) to the saturation value of several hundred for dominant IGWs.

The use of the simple model (2) for the problem in question is justified as follows. As shown above, the most important scales for the IGW model (the Fresnel scale and the outer scale), which determine the RO signal fluctuations, equal or exceed the value of about 1 km in the vertical direction. For inhomogeneities with such vertical scales, the anisotropy significantly exceeds the critical value. Therefore, the amplitude and phase fluctuations do not any longer depend on the anisotropy values and reach the saturation level, as if the inhomogeneities were spherically symmetric. This explains why it is possible to use the model with a strong constant anisotropy. Due to this, the RO observation geometry can be assumed effectively vertical, and the amplitude and phase fluctuations depend only on the vertical structure of saturated IGWs (Eqs. (10) and (14)), which is adequately described by model (2). In some cases, for strongly oblique occultation events, the condition of effectively vertical observation geometry may be broken in the lowest few kilometers due to the strong refraction, which decreases the vertical component of the ray immersion velocity.

Following the ideas of Dalaudier and Gurvich (1997), Gurvich and Chunchuzov (2008) developed an empirical 3D model of saturated IGWs with the anisotropy increasing as a function of the vertical scale. The vertical spectrum follows the −3 power law, while the horizontal spectrum can have the −5/3 power law for the corresponding choice of anisotropy parameters. This model is in a good agreement with the known air-borne observations of horizontal spectra of IGWs. Scintillation spectra evaluated on the basis of the variable anisotropy model (Gurvich and Chunchuzov, 2008) are in a good agreement with those evaluated on the basis of the constant anisotropy model (2) for effectively vertical occultations (Kan, 2016).

…and why specifying IGW breaking parameters as a function of height the way they did
To answer this question, we expanded the last paragraph in page 12, lines 3–7 as follows:
It is known that local profiles of atmospheric inhomogeneities exhibit large natural variability. Furthermore, even their average profiles significantly vary depending on latitude, season, orography, regions etc. The turbulence structure characteristic for different observations, even in a free atmosphere, may vary by up to two orders of magnitude (e.g., Gracheva and Gurvich, 1980; Wheelon, 2004). A significant variability is observed for the intensity of saturated IGWs (e.g., Sofieva et al., 2007a; Sofieva et al., 2009), which depends both on the sources producing the waves and on the propagation and breaking conditions. Eq. (3) for the saturated IGW only reflects the most general relation between the structure characteristic and the atmospheric stability. The latitudinal variability of the structure characteristic significantly exceeds that of $\omega_{B.V.}^4$ (Sofieva et al., 2009). However, on the average, the variations of refractivity fluctuations and, therefore, the amplitude fluctuations are determined by the exponential decay of the atmospheric density with altitude. Because our work is aimed at a qualitative distinction of the contribution of turbulence and IGWs to the fluctuations of RO signals, we consider only averaged vertical profiles of the structure characteristic of turbulence and IGWs for the theoretical estimates. Quantitative studies of IGW parameters and wave activity for different latitudes, seasons, and regions in the stratosphere and upper troposphere are planned for the future work. Despite possible inaccuracies in the assumed values of the structure characteristic, the variance estimates obtained in this work, definitely indicate the dominant role of saturated IGWs under the conditions in question.

The log-log plots of power spectra the authors present span only one and a half decades, meaning that there is only the slightest constraint on determination of the power law when significant spread between spectra is present. Such is the case in this manuscript. The authors must distinguish between a -3 power law characteristic of IGWs in the log-log plots and a -5/3 power law characteristic of Kolmogorov turbulence, which can be done easily by including a -5/3 line on the power spectral density plots.
We updated Figures 2 and 4 with the −5/3 asymptotes, corresponding remarks were added the Figure captions.
In the end of Section "Experimental Fluctuation Spectra of Amplitude and Phase", we added the following paragraph:
The atmospheric inhomogeneity models have not only different anisotropy, but also different slope $-\mu$ of the 3D spectra, which determines the diffractive decay $-\mu+2$ in the presented spectra of RO amplitudes and phases. The decay is fast, which aggravates the derivation of accurate estimates. Nevertheless, Figures 2-5 indicate that the diffractive decays of the experimental spectra are in a better agreement with the IGW model, as compared to the turbulence model.

Page 1, line 19: "stimulated"
Page 1, line 20: "Currently, RO sounding…"
Page 2, lines 1-3: "The stability of GPS signals, complemented with its global coverage and high vertical resolution, draws the attention of researchers to the study of inhomogeneities in atmospheric refractivity in addition to the retrieval of mean profiles."
Page 2, line 10: "empirical"
Page 2, line 11: "component described"

Page 2, line 12: "the isotropic component as Kolmogorov turbulence"
Corrected.

Page 2, line 18ff: consider calling it "weak scintillation theory" rather than "weak fluctuation theory" throughout the manuscript.
Many authors (e.g. Ishimaru, A.: Wave Propagation and Scattering in Random Media. Vol 2; Rytov, S. M., Kravtsov, Y. A., and Tatarskii, V.: Principles of Statistical Radiophysics; Gurvich, A. S. in many works) use terms "weak fluctuations", "smooth perturbations", "Rytov approximation", and "weak scintillations" as equivalent ones. To emphasize the equivalence of "weak fluctuation" and "weak scintillation", we modified the corresponding sentence:
"The upper limit was determined by the radiation shot noise, the lower limit was determined by the applicability condition of the Rytov weak fluctuation/scintillation theory."

Page 2, line 21-22: "about 30-35 km where residual ionospheric fluctuations and measurement noise become dominant."
Page, line 23: "In the visible band,..." Throughout the text, call it the "visible" band rather than the "optical" band. "Optics" refers to a kind of signal dynamics that spans most frequency bands, including microwave, infrared, visible, and ultra-violet.
Corrected.

Lines 25-26: "In the radio band, the leading cause of the inhomogeneities is IGWs, whose spectra are characterized by a steep power spectral decrease with increasing wavenumber."
Line 31: "dominate the radio signal…"
Line 32: "The aims of this paper are to clarify the role of the two inhomogeneity types and to evaluate their actual contributions…"
Page 3, line 2: "complicated dynamics of lower-tropospheric…"
Line 3: delete "the basic models and approximations"
Line 4: "screen approximation, the weak fluctuation/scintillation theory, and the approximations entailed. In Section 3 we apply these methods to derive…"
Corrected.

Line 17: "statistical average" should be better defined, most likely as "regional average".
Yes, it should be the regional and seasonal average estimate.

Line 17-18: "Refractivity fluctuations depend…"
This statement refers to the visible band.

Page 4, lines 7-8: "are wavevector parameters corresponding to the outer and inner scales, respectively."
Corrected.

Page 4, line 10ff: The vertical wavenumber spectrum for saturated gravity waves is usually referred to as the "universal spectrum". Be sure to cite the original work: Dewan and Good 1986.
Corrected.

Lines 15-27: The idea of "critical anisotropy" is new to me. To what phenomenon does it refer? Be clearer.
For occultations, the critical value of the anisotropy coefficient $\eta_{cr} = \sqrt{R_e / H_0} \approx 30$ separates moderately anisotropic inhomogeneities with $1 \le \eta < \eta_{cr}$ and strongly anisotropic inhomogeneities with $\eta > \eta_{cr}$. In the former case, the sphericity of atmospheric layers may not

be taken into account, in the latter case, the sphericity results in the saturation of the eikonal and amplitude fluctuations. Gurvich and Brekhovskikh (2001) introduced this characteristic and the corresponding term. We added here a brief remark: "… the concept of the critical anisotropy will be discussed below (see Eqs. (7) and (8)).

Lines 28-30: I'm not sure what this sentence means.

Corrected as: "To obtain the value of the structure characteristic C_W^2 in the radio band, $C_{W,dry}^2$ must be multiplied with the coefficient K^2, which takes humidity into account (Tatarskii, 1971; …)."

Page 5, line 3: "A = 0.033"

Corrected.

Page 6, equations 4, 5ff: Be clear about the "minus-plus" notation and why you use it. It took me a while to figure out.

In using this notation, we follow (Rytov et al., 1989). In our opinion, this not only reduces the number of formulas, but also emphasizes the difference between amplitude and phase fluctuations.

Page 7, equation 7: When the thin screen approximation is itself in the small screen approximation with respect to the Earth's curvature, I wouldn't expect there to be any dependence on the Earth's curvature in any equation. So why does the Earth's radius occur in equation 7? Also, write out $\overline{\Psi}$ explicitly.

The thin screen introduces the same average phase shift and the same phase fluctuations as the atmosphere along the ray. The phase shift is evaluated by means of the integration of the refractivity along the ray. For inhomogeneities with the anisotropy that exceeds the critical value, the sphericity of the atmosphere must be taken into account, which results in the saturation of fluctuations, because different anisotropic inhomogeneities have different orientation with respect to the line of sight, according to their horizontal position. Therefore, the critical anisotropy is an increasing function of the Earth's radius. Formula (7) gives the expression for the phase (eikonal) fluctuations for the case, when the Earth's sphericity and, therefore, the saturation of fluctuations can be neglected. Formula (8) refers to the case, when the Earth's sphericity must be taken into account. A detailed analysis of the thin screen with account of the Earth's sphericity can be found in (Gurvich, 1984; Gurvich and Brekhovskikh, 2001).
The explicit expression for $\overline{\Psi} = \sqrt{2\pi R_e H_0}\,\overline{N}$ is presented after Eq. (6).

Equation 8: Does this math also consider distortion of the Fresnel zones by the differential ray bending by the atmosphere's vertical structure?

The effect of the Fresnel zone compression due to differential regular refraction is approximately taken into account by using the refractive attenuation factor q.

Page 8ff: Be sure to define precisely the angles α, "occultation angle", "obliquity angle". I cannot tell what these angles are.

Now, we uniformly refer to this angle as to the obliquity angle. This angle is defined in the text as follows: "The observation geometry will be determined by the obliquity angle α of the occultation plane, defined as the angle between the immersion direction of the ray perigee and the local vertical in the phase screen."

Page 8, line 14: "or grazing occultation"

Corrected.

Page 11, line 4: "Numerous radiosonde profiles and…"
Corrected.

Page 11, line 8: The value given for L_W is in fact highly variable throughout the global atmosphere. It should have been mentioned somewhere in the introduction that the intention is to qualify RO scintillations as due to turbulence or gravity waves in a gross, global sense.
In the introduction, we added the following remark:
"Our aim is not the quantitative study of RO signal fluctuations, but rather a demonstration of qualitative principal differences between the manifestations of turbulence and IGWs in RO signals."

Page 12, line 3-5: I do not understand this sentence.
We extended this paragraph, as specified above.

Page 13, lines 1-2: "The variances of RO log-amplitude and phase fluctuations…do not contain direct information…" Why can't turbulence be anisotropic at its outer scales? Most of the atmosphere is stably stratified, resisting vertical motion, which means that turbulence would natural seek to extend in the horizontal rather than in the vertical.
It is true that many researchers complement the Kolmogorov turbulence with anisotropic inhomogeneities at scales approaching the outer scale (e.g., Wheelon, 2004 and further references therein). This allows taking into account the underlying surface in the bottom layer or the influence of the stable stratification in the free atmosphere. We chose the simplest and most commonly used models of 3D inhomogeneities, including the isotropic turbulence, because our aim was not the qualitative retrieval of inhomogeneity parameters, but rather a qualitative estimate of the role of different inhomogeneity types in RO signal fluctuations. Introducing the anisotropy into the largest scales of turbulence will not result in radical changes of the fluctuation estimates: amplitude fluctuations are determined by small-scale inhomogeneities, while the estimates of phase fluctuations are aggravated by the strong regular variations of the phase, as discussed in the paper. Our plan for the future work is to perform quantitative evaluation of the RO signal using 3D models of turbulence and IGWs with variable anisotropy.

Line 4-5: delete "to which the anisotropic…"
Corrected.

Line 5: "This information can be extracted from an ensemble of 1D spectra of RO signal fluctuations, when categorized according to frequency or to vertical wavenumber."
We updated this sentence as follows: "This information can be extracted from an ensemble of 1D spectra of RO signal fluctuations measured at different obliquity angles, when categorized according to frequency or to vertical wavenumber."

Line 8: What is the oblique movement velocity? Define.
We defined it as the velocity of the projection of the ray perigee to phase screen plane.

"they" should be "the"
Corrected.

Lines 9-10: "for a highly oblique occultation." Delete "due to the geometrical difference…" to the end of the sentence.
Corrected.

Line 16: What is the inclination angle?
The obliquity angle.

Line 22: Linear trends in what? "Figures 2 and 3"
The mean amplitude profiles were determined by linear fitting.

Line 33: "spectral window with variable width"
Corrected.

Line 33-34: Be clear about f. No need to write "Q-factor", a term more appropriate to prescriptions of oscillatory systems.
We added notation f. Instead of Q-factor, we use the term "quality".

Page 14, line 1: "Figures 2 and 3"
Corrected.

Line 5ff: Be clear about what you mean when you write "isotropy hypothesis", "anisotropy hypothesis". I believe that the isotropy hypothesis is that the scintillations are caused by Kolmogorov turbulence and that the anisotropy hypothesis is that they are caused by breaking internal gravity waves. The text must be clear on this.
Yes, the isotropy hypothesis refers to Kolmogorov turbulence, while the anisotropy hypothesis refers to saturated IGWs. This is clarified in the Figure captions.

Lines 8-9: "frequency. With increasing occultation angle (???), the maxima systematically…"
Occultation angle was replaced by obliquity angle throughout the text.

Line 10: "all the spectra are peaked near wavenumber 1, which represents the first Fresnel zone…"
Corrected.

Lines 17-19: I don't understand this sentence.
We corrected the sentence as follows:
"The variance of amplitude fluctuations weakly depends on the outer scale L_W, if it significantly exceeds the Fresnel scale. Nevertheless, the influence of L_W results in a faster than +1 decrease of the spectrum at low frequencies."

Page 15, line 3: I suspect the "deep oscillations" are a reference to diffraction fringes.
Yes.

Lines 3-4: "The slope of the spectrum at high frequencies agrees…"
Corrected.

Lines 6-7: This sentence needs clarification. What is α, and what does it have to do with anisotropy?
α is the obliquity angle.

Line 10: "they mostly exceed the theoretical..."
Line 11: "RMS values prove the validity…"
Corrected.

Line 14: The definition of "eikonal" should be moved much earlier in the document. Either that, or use term "phase" instead throughout the paper. It is a term much more commonly used in the RO community.

The eikonal is first defined after formula (5), we complemented the definition with the following text:

"The eikonal, or the optical path, characterizes the propagation media, while the phase also depends on wavelength. In the RO terminology, the excess phase (or phase excess) refers to the eikonal of the observed field with the subtraction of the satellite-to-satellite distance. The excess phase, therefore, characterizes the atmospheric effect in the observed eikonal. The excess phase (eikonal) is modeled by the phase screen. Accordingly, in the observation plane we study the fluctuations for both eikonal and phase."

Page 15. Lines 19-20: What are the "first approximation" and the "first term"?

The corrected formulation:

In the first-order approximation of the perturbation method, the eikonal variations are determined by the refractive index variations of the neutral atmosphere (Vorob'ev and Krasil'nikova, 1984).

Page 16, line 8: What is a Hann window? Give a reference.

Hann, or cosine window is defined in (Bendat and Piersol, 1986, p. 13). The reference is added.

Line 9: "Figures 4 and 5..."

Line 14: "These spectra are in fair agreement..."

Page 17, lines 2-3: "1) isotropic Kolmogorov turbulence, and 2) anisotropic saturated IGWs."

Line 4: "phase with empirical 3D..."

Corrected.

Page 18, line 5: What are "small altitudes"? The boundary layer?

Small altitudes are altitudes of a few kilometers. As it follows from eq. (15), for the IGW model $\sigma_\chi^2 \propto q^{3/2}$. The refractive attenuation changes from 1 at large altitudes to approximately 0.15 at 4 km, which partly compensates the increase of the amplitude fluctuation due to larger density at lower altitudes.

We update the text as follows:

"This, together with the strong refractive attenuation at small altitudes, according to (15), significantly reduces the amplitude fluctuations and, therefore, the weak fluctuation condition is met for altitudes down to a few kilometers."

Lines 8-9: "permit a diagnosis of wave activity..."

Line 18: "IGWs are additionally restrained..."

Line 23: Replace "close" with "similar".

Lines 23-24: Remove the sentence. It is obvious.

Corrected.

Line 33: What are "occultation angles"?

Obliquity angles.

Page 19, line 7: Estimates of what?

Estimates of the atmospheric inhomogeneity parameters

Line 8: Begin the sentence with "In the stratosphere and upper troposphere, ..."

These words can be excluded, because the sentence defines the height ranges.

Line 14: "perturbations are sinusoidal."

Sinusoidal form of perturbations is not synonym for their wave nature.

Line 16: What is "higher resolution"? Higher than what?
The sentence mentions "high-resolution radiosonde observations".

Page 20, line 2: "On the other hand, for quick estimates, ... The amplitude variance permits the…
Corrected.

In addition, we corrected some other typos and references.