Response to Referee #2
"Progress in turbulence detection via GNSS occultation data," Cornman, et al.

A few general comments for both referees:

Even though mentioned in the manuscript, it was not clear that the referees understood the main intent of the paper. That is our fault, and we will make that clearer in both the Abstract and in the Introduction. The main purpose of the paper was to present a parameter estimation methodology and error analysis. The derivation of the frequency spectrum model was secondary, and we thought that it would be helpful for the general reader of GNSS applications to atmospheric science. Finally, the purpose of the limited number of real data case studies was to show that the frequency spectrum model was, at least qualitatively, a reasonable one. It was in support of the parameter estimation and error analysis sections, and was not meant as a stand-alone discussion of the real data, nor an in depth analysis of the cases.

We were also remiss in not being more clear as to what assumptions went into the frequency spectrum model development, and what we were doing that was similar – as well as different – from previous works, (e.g., straight line propagation, weak scattering, moving transmitter, receiver and atmosphere, etc.). We also did a poor job of providing references to the relevant literature. Besides not discussing the model assumptions, we did not do a good job of delineating what items we choose not to include (e.g., deterministic layered phenomena, anisotropy, ionosphere, strong scattering, the radiation pattern of the transmitter and the gain pattern of the receiver, non-straight line propagation due to large-scale deterministic permittivity variations, etc.). We will rectify these matters in the revised manuscript.

We fully agree with both referees that the paper is too long. We will address this issue as follows. (1) We will minimize the sections on the frequency spectrum model development, using more references to the literature as well as being clear about what we're doing that's similar as well as different; (2) We will reduce dramatically the number of figures in the parameter estimation and error analysis sections – as well as the discussions therein.

Specific responses to Referee #2:

General Remarks.

The referee brings up a good point regarding layered structures. As mentioned above, we deliberatively choose not to deal with layered structures. We have studied this, and know that it results in a much more complicated problem, requiring modifications to the first-order Rytov approach. We did not include this topic in this paper, as it really would require probably more than one paper itself to discuss it adequately. It is interesting that the referee brings up this issue, as it was the next logical step in our development. Unfortunately, we ran out of funding before we could do anything except study the existing literature. We hope that the referee will understand that the first step in the analysis was to simplify the problem to isotropic turbulence – without a background permittivity field. Clearly, the next steps would be to introduce more complexity, e.g., anisotropy and/or the combined background/random permittivity field.

The reason that the inclusion of layered media is difficult is because large-scale layers will refract the incoming wave, which then means that the straight-line approximation for the propagation path needs to be modified. The permittivity field is separated into background and random parts. (Note that in this context, "background" refers to all aspects of the deterministic permittivity field.) One could then re-write the original differential equation in a non-Cartesian coordinate system, e.g., a local one which has as its axes the tangent, normal and binormal vectors to the "main" propagation path – for example that described by geometrical optics. These are so-called trajectory coordinates. (See for example: Hill, R.J., 1985: A stochastic parabolic wave equation and field-moment equations for random media having spatial variation of mean refractive index. *J. Acoust. Soc. Am*., 77, 5. Or, Mazur and Felsen, High frequency coherence functions propagated along ray paths in the inhomogeneous background of a weakly random media: I – formulation and evaluation of the second moment, Journal of the Acoustical Society of America, 81(4) 1987.) Another approach is to use a path integral approach to determine the Green's function in a multiple scattering context. (See for example, Mazur, Modeling of high-frequency propagation in inhomogeneous background random media. Journal of the Acoustical Society of America, 111(2) 2002.) Yet another approach is to use so-called Distorted-Wave Born (or Rytov) Approximations (See for example: Beylkin and Oristaglio, Distorted-wave Born and Distorted-wave Rytov Approximations, Optics Communications, Vol. 53, no. 4, 1985 or Devaney, A.J., 1979: The Inverse Problem for Random Sources., *J. Math. Phys*. **20**, 8.). In this method, one assumes that the permittivity field – as above – is separated into background and random parts, however the heuristic model is that the incoming waves are refracted by the background and are then scattered by the random field. The contribution of all such scattered fields is then what is measured at the receiver. That is, it is a single-scattering process, but the electric field at the scatterer is now "distorted" by the background field. Of the three methods, this is probably the most tractable one.

It is important to note that the techniques mentioned above are all based on wave propagation theory. Geometrical optics methods are only used to define the trajectory coordinate system. This is in distinction with the techniques that have been used in the GNSS literature, where one starts with geometrical optics and extends it to include ray multipath effects, i.e., the Fourier Integral Operator (FIO) (and similar) methods. However, the FIO method is really a ray-based method - there is a "coordinate" transformation into a space where the "rays" are not crossing, but it's still rays and geometrical optics after that. From the wave approach, one only considers interaction of wave fronts - without reference to the normals to the wave fronts (i.e., rays). With random medium, it doesn't make sense to talk about rays as physical phenomena ("ray bundles" or transfer of energy via an effective "main ray", perhaps, but not individual physical rays) - since they're all just a jumble. However, this doesn't mean that the wave optics methods are not useful - they are - but one needs to apply them only to the larger-scale refraction properties of the index of refraction field. (In this context, refraction in an inhomogeneous medium can produce crossing rays. Some call that diffraction, but it's a difference without a distinction.) We thought that a combined method could be used as an alternate to the purely wave theory methods discussed above. That is, use as a first guess for the background permittivity field that obtained from geometrical optics, FIO methods, or via the methods presented in the two papers that the referee has mentioned (Pavelyev et al. and Liou and Pavelyev). This background field is then used with the Distorted-wave Rytov Approximation method for the random part.

As mentioned just prior, the referee mentions two papers, and says that, “These papers introduced some progress in the revealing the turbulence contribution in RO signals.” From what we can tell, these papers do not deal with turbulence, but rather larger-scale gradients. Perhaps the referee is pointing out that the residual between the original signal and that determined by the phase acceleration method, would be the turbulence. It wasn’t clear to us. These authors use geometrical optics to derive their relations. However, geometrical optics is an asymptotic theory in that it is assumed that the transmitter wavelength is much smaller than the typical smallest scales (inner scale) of the inhomogeneities ($\lambda_0 \gg \lambda$), and that these scales are also much larger than the path length (*x*) through the inhomogeneities (more precisely, $\lambda_0 \gg \sqrt{\lambda x}$). (See Tatarskii’s 1971 book, Part B – the paragraphs just above Section 45, page 218.) The first condition is also required for the Rytov approximation, but the second one is not. As an example, consider $\lambda = 0.2m$ and $x = 10^5 m$, then assuming that " $\gg$ " means “at least by an order of magnitude,” then $\lambda_0 > 2km$. This is larger than one would typically consider to be turbulent scales – at least in the troposphere and lower stratosphere. Due to the 10^{-6} difference with GNSS-like wavelengths, at optical wavelengths this relationship would certainly hold for a wide range of turbulent scales.

The referee also brings up a good point in how the assumed Gaussian statistics could be affected by layered structures. If there was a universal theory – or at least a good empirical model – for layered structures, it is possible that one could develop a probability distribution for the combined deterministic/random process. The main reason that we chose the two COSMIC that are in the paper was that they had significant amplitude changes over the analysis window. In addition, as mentioned above, the reason for the two case studies was to show that the real data is very similar to the simulated data. For example, one could compare Figures 43 and 45 (COSMIC case 1) with Figures 25 and 27, for frequency power spectra and distribution plots, respectively. The COSMIC case 2 does show differences from the simulated data – but we think that is due to some sort of quality control or signal processing problem than a physical phenomenon. That is, the oscillations in the amplitude after the sharp discontinuity seem suspicious (cf. Figure 48).

(2) We are well aware of strong fluctuation theory. However, we believe that for decimeter wavelengths and propagation path lengths through the turbulence, it is not necessary. As mentioned above, we will be clearer in the aspects of the problem that we are addressing, and the ones that we are not. Strong fluctuations and layered phenomena are two topics that fall into the latter category. As an aside, we did develop a test to try to determine when the weak-scattering assumption breaks down. This uses Eq. (20-46b) in Ishimaru's book. Expand the exponential in a power series. The first two terms correspond to weak scattering, so if one subtracts them from the left-hand side of the equation (calculated empirically), this should be close to zero for weak scattering and non-zero for strong scattering (or data quality problems).

(3) We totally agree that the paper is too long, and will rectify that as mentioned above.

Detailed Comments.

Abstract.

We will change the sentence to reflect that the model is for the fluctuations in the received signal – due to the fluctuations in the media.

Introduction.

We will clean-up the Introduction to discuss solely the RO applications.

See comments above regarding layered structures.

Wave Propagation.

As mentioned above, we will present all the assumptions made in our frequency spectrum model development. We will also describe the issues involved in accounting for layered media – as discussed above. We will also include a figure describing the geometry of the problem. However, for straight-line propagation, it's pretty simple. Recall that we're not taking refraction into account, and so all the more detailed geometry associated with ray paths and bending angles does not appear. We can understand how it may be unclear as to how the location of the turbulence along the line of sight comes out of the derivation. First of all, there is a coordinate transformation, $\xi = x' - x''$ and $\eta = \left(x' + x''\right)/2$, where x' and x'' are the location of two arbitrary scatterers within the turbulence patch. We integrate over ξ, and hence the frequency spectrum is a function of η (and *f*). Next, we invoke the mid-point approximation (cf. the paragraph above eq. (45) in the manuscript). For a small patch, we can consider this the center of the patch, $\eta \to \eta_1$. This is the quantity that we estimate. We are assuming that the turbulence patch is of an arbitrary shape – not necessarily a layer. Although, for single scattering and the parabolic approximation (cf. Eq. (30) in the manuscript), one can assume that it is the scatterers in close proximity to the line of sight that will predominant the field at the receiver.

(3) Parameter Estimation.

We will clarify that it is the frequency spectrum of the log-amplitude fluctuations that we're discussing. We were using the term "amplitude spectrum" as a short hand.

(4) Simulation Studies.

We have discussed this issue above. We reiterate that the referee's comments are right in line with where we wanted to take this research – but unfortunately ran out of funding.

(5) GPS-COSMIC occultation analysis.

We will include more specifics about the occultations that are presented. However, as mentioned above, the purpose of including the real data was to show that the frequency spectrum model and the assumption of exponential statistics therein – simplistic as they are – do in fact compare well to the real data. This was not meant to be a thorough analysis of real data cases – that should be a separate paper in itself.