Response to Referee #1
"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 #1:

General Remarks.

We did refer to Tatarskii's 1971 book, Ishimaru's 1997 book, and the paper by Yeh and Liu in the Introduction, but as mentioned above, we were remiss in not including many specific references in the Wave Propagation and Random Media sections. Just because we did not reference certain parts of the literature, does not mean that we have not studied them. We are aware of many references on optical wave propagation, but most of the methods that we use can be found in the abovementioned books (the former of which is mainly focused on optical wavelengths). Furthermore, there are numerous papers that deal with radio wave occultations through turbulent atmospheres – which are equally appropriate to our problem, (e.g., the sequence of papers by Ishimaru and Woo in the 1970s and 1980s.) The use of stellar sources for the study of scintillations differs from our subject. This is because the source can be viewed as fixed in space, whereas in our case, the source is moving. In the GNSS application, this may not be a very significant effect, but we wanted a theory that could be applied to LEO-LEO occultations.

The referee states that our approach to parameter estimation (maximum likelihood) is not new for this application, and references A. Gurvich and I. Chunchuzov, Estimates of characteristic scales in the spectrum of internal waves in the stratosphere obtained from space observations of stellar scintillations, JGR, Vol. 110, 2005. This paper really does not discuss the specific parameter estimation method that they use, but they do refer to an earlier paper by the first author that does: Gurvich and Kan, Structure of air density irregularities in the stratosphere from spacecraft observations of stellar scintillation, Part I. Izvestiya, Atmospheric and Oceanic Physics, vol. 39, no. 3, 2003. (Note that in the Gurvich and Chunchuzov paper, they incorrectly refer to Part II.) The referee states that these authors are using maximum likelihood estimation; however, it is not clear to us that they are. Maximum likelihood methods deal directly with the probability distribution model of the data. As far as we could tell, Gurvich and Kan never discuss the probability distribution, and in fact, their Equation (18) appears to be a weighted least squares objective function as opposed to a likelihood function. This could just be confusion on our part, or a different usage of the term maximum likelihood. It is very important to note, however, that there is nothing wrong with a least squares approach to parameter estimation. In fact, we originally tried a least squares method, but we found that the objective function was too broad and shallow, and hence it was difficult to see a clear minimum.

Introduction.

We will take out the reference to “appropriate theory/inappropriate techniques.” We were referring to the use in the literature of the “scintillation index” S_4 related to a normalized intensity variance of the received signal, or the variance of the phase, σ_S^2.

Wave Propagation.

See above for how we intend to clean up this section. We agree with the reviewer that what is shown in Figure 1 can easily be understood from Eq. (45) in the text, i.e., that the amplitude of the frequency spectrum is directly proportional to the turbulence intensity. We will take this figure out and just mention this fact in the text. Note that if the turbulence was inhomogeneous, our Eq. (43) should be used, and then the proportionality would not necessarily be so simple (not to mention the significant increase in complexity in the parameter estimation).

There are two references mentioned by the referee, A. S. Gurvich, A heuristic model of three-dimensional spectra of temperature inhomogeneities in the stably stratiifed atmosphere, Ann. Geophysicae 15, 856–869 (1997). And, A. S. Gurvich and V. L. Brekhovskikh, Study of the turbulence and inner waves in the stratosphere based on the observations of stellar scintillations from space: a model of scintillation spectra, Waves Random Media 11 (2001) 163–181. The first of these does not cover the topic of wave propagation through random media, the second paper does, but there are some differences with our work. (1) The authors are using a non-moving star as the source; (2) they assume that the turbulence is fixed at the perigee point, and so the location of the turbulence along the line of sight is not a parameter that is not of importance – whereas for our application the location and strength of the turbulence are the key parameters; (3) they are assuming plane waves instead of spherical ones. This is perfectly adequate for stellar sources, but perhaps not so for LEO-LEO or LEO-aircraft occultations. (The GNSS-aircraft occultation problem was the original motivation for our work); (4) they are using the phase screen approximation, which again is perfectly suited to their application, but may not be so for a GNSS-aircraft occultation where the ratio of the path length through the turbulence to the distance from the turbulence to the receiver is not necessarily much less than one.

Parameter estimation.

We did not provide a reference to the fact that the power spectrum of a Gaussian process is an exponential, as we assumed it was common knowledge. On reflection, and as the referee implies, we probably should not be so caviler in that assumption, and we will provide a brief outline and reference. (The sum of the squares of k independent standard Gaussian variables is a Chi-squared distribution with k degrees of freedom. When k is 2, this simplifies to an exponential distribution with mean parameter 2.)

We will clean up the notation in using L to stand for the turbulence length scale as well as for the likelihood function. (Unfortunately, both of these uses are fairly common in their respective fields.)

We will include the discussion provided above regarding the Gurvich and Chunchuzov, and the Gurvich and Kan references regarding prior efforts on parameter estimation.

Simulation Studies.

As mentioned above, we will reduce this section significantly – including many of the figures.

We will correct the typos regarding “likelihood.” (Thanks for pointing that out.)

GPS-COSMIC data analysis.

See our comments above regarding the inclusion of the COSMIC occultations. We will make it clearer why we have included these cases. We feel that a true discussion of the real data belongs in a separate paper – especially seeing as this one is already way to long.