Anonymous Referee #2

The manuscript is interesting and follows in general appropriate logic flow. It requires however some improvements, as detailed below. I recommend minor revision. I encourage otherwise the authors to work to clarify the text, as it is at times difficult to follow.
My main concerns are presently not addressed in the paper, but could be addressed with better justification, explanation, or reference to external material. These are the following two items:
1) The authors show that fluctuations following the structure function presented in Fig i produce a negative bias that is very similar to the one generally known to exist. That structure function is not unreasonable, but the authors do not present a link between known or expected atmospheric properties of turbulence, or temperature and moisture fluctuations, and the $C_N{}^2(z)$ *presented. Why that profile of fluctuations? A later sentence (P4L9) says "refractivity fluctuations can explain and quite well describe the systematic and random error...". The agreement found actually means that some fluctuation profile can be found that reproduces the known bias, although it has not been shown or referenced whether that profile was realistic at all. Beyond, the* $C_N{}^2(z)$ *shown is peaked at the low troposphere, descends near the surface, and also monotonically reduces above the PBL. A realistic* $C_N{}^2(z)$ *may have also other minor peaks and features.*

We agree that these points need clarification in the paper, and some of the current formulations can be misleading for a reader. As we already stated above, in the rebuttal to Reviewer #1 comments, this model is a good structural model that allows finding good candidates for the bias predictors. All the bias estimates are based on the objective characteristics of the signal received. As discussed in more detail, in the paper by Gorbunov, Vorobiev, and Lauritsen (2015), that, with the corresponding choice of the effective profile of $C_N{}^2(z)$, the fluctuation model can reproduce the statistical characteristics of the observed bias. However,

because this model is not directly used in the bias correction procedure, the further discussion of $C_N^{\,2}(z)$ is beyond the scope of this paper. We updated the formulations in the paper along the lines of this discussion. In particular, we added the following remark to the discussion of Figures 4 and 5:
"An important conclusion from these comparisons is that the fluctuation model alone does not explain the patterns observed in the real observations. However, the role of this model is to help finding reasonable predictors. The further bias correction procedure is only based on the predictors that can be readily derived from observations, rather than on the fluctuation model."

2) Although the idea of estimating the expected bias through an atmosphere of given fluctuation properties is interesting, the proposed solution is an empirical regression, where the bias (wrt ECMWF) is reduced. I am concerned about the impact on traceability, since the lower bias is obtained by heuristic fit, rather than by a physical link. Among other concerns, it simply succeeds on reproducing the bias of ECMWF (which may itself be biased) with a large number of predictors. This is the procedure normally applied to, for instance, radiance measurements. Historically, one of the major benefits of radio occultations has been the possibility to use these data without such heuristic bias correction. Otherwise, the number of predictors and adaptive functions being so large, it would have been surprising not to be able to fit the bias. A bias reduction with a very small number of predictors, and more physically based, would be more solid.

Historically, there was a belief that RO data were not biased. However, the further development indicated that these data were indeed biased. Currently, there is no a good physical model that can qualitatively describe the bias. We can only say that the observed bias is a multi-factor phenomenon. In this paper, we are discussing an empirical approach to the estimate of the bias from well-defined predictors derived from objective characteristics of measurements. It is true that still we need some reference, and if we use ECMWF data, we involve the bias immanent to ECMWF. On the other hand, the method itself will stay, if we include some independent bias estimate of ECMWF. We added some remarks along these lines to the Conclusions.

Several minor details follow.
P8L8: "energy density of rays". Please define the meaning here of "energy density".
More precise formulation is "the normalized the energy distribution over rays in the impact parameter space." For more details, see the references (Gorbunov, 2002; Gorbunov, 2004).

P10L11: Given those many predictors, one question that arises is why this set? Why not others, such as season, topography, land/ocean?
Season was tested and found to be a weak predictor. Topography and land/ocean may be worth further investigating, although, as suggested by the new Fig. 8, they are not unambiguous.

P10L18: "limiting the adaptive functions to the reasonable ones" What is the meaning of "reasonable" here?
We agree that the word "reasonable" has no precise definition in the context. We re-phrased this as follows: "… apply some additional constraints in order to reduce the number of adaptive functions."
P14L26: "reasonable profiles of $C_N^{\,2}(z)$*". It has not been justified that these are reasonable. Only that they would reproduce the bias.*
Yes, as already discussed in the previous responses.

Figures 5 and 6: Is it my perception or the procedure is moderately overcorrecting
To some extent, they are. However, The data processing chain with the uncertainty propagation requires the back projection of bending angle bias to the excess phase and amplitude.