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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Wave-optics uncertainty propagation and regression-based bias model in GNSS radio occultation bending angle retrievals" *by* Michael Gorbunov and Gottfried Kirchengast

**Anonymous Referee #2**

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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 1 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.

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.

Several minor details follow.

P8L8: "energy density of rays". Please define the meaning here of "energy density".

P10L11: Given those many predictors, one question that arises is why this set? Why not others, such as season, topography, land/ocean?

P10L18: "limiting the adaptive functions to the reasonable ones" What is the meaning of "reasonable" here?

P14L26: “reasonable profiles of C2N(z)”. It has not been justified that these are reasonable. Only that they would reproduce the bias.

Figures 5 and 6: Is it my perception or the procedure is moderately overcorrecting

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