Referee #3

1. *Abstract, line 7: Define "fair quality" and give the minimum value of SNR (as commonly used in RO papers) needed to produce this quality at all levels between 0 and 50 km. Does "quality" refer to the SNR or to characteristics such as precision, accuracy, and depth of penetration of the final refractivity and bending angle retrievals? If it refers only to SNR, this should be stated explicitly in the Abstract.*

   I agree that "fair quality" needs a definition. I removed this sentence from the Abstract.

2. *Most operational NWP centers now use bending angles rather than refractivity in their assimilations; yet this paper does not present any bending angle information.*

   The study addresses the quality of inversions, and it is natural to formulate the results of the study in terms of refractivity.

3. *The results should be discussed in relation to other relevant works on SNR, e.g. Kursinski et al. (1997) and Sokolovskiy et al. (2010) Do the magnitudes and vertical distributions of the RMS errors agree with previous results? If not, why not? The effect of noise on the inversions of RO signals with spread spectrum in the troposphere was investigated by Sokolovskiy et al. (2010) by using a similar approach, adding noise to RO signals and inverting. This effect was mentioned by Gorbunov et al. (2015), yet not in this paper. Does this effect explain the large bias in the troposphere obtained at very low SNRs?*

   Gorbunov et al. (2015) discussed the effect of wave propagation in a turbulent medium, which is different from additive noise. But I agree that Sokolovskiy (2010) performed similar numerical simulations with additive uncorrelated noise with a magnitude of 1.7 than the intrinsic noise of COSMIC data. Given the COSMIC C/N_0 of about 55 dB-Hz, the added noise should correspond to C/N_0 of about 50 dB-Hz. In our simulations, this value manifests itself as a weak noise. Sokolovskiy (2010) shows that this may have a significant effect for tropical events, when using an insufficient cut-off height $H_{cut} = -100$ km, but the effect is much smaller for $H_{cut} = -150$ km. In our simulations, the effect of noise with this magnitude is small, and our setup implies $H_{cut} = -250$ km.

4. *Line 17: It is unclear what is meant by this sentence: "SNR, being based on the intrinsic receiver noise figure, has never been treated as the useful signal." The varying SNR during an occultation is used in retrievals to unravel atmospheric multipath, which is indeed useful. Please clarify.*

   The sentence should be re-written as follows: *The intrinsic receiver noise, however, has never been treated as the useful signal.*

5. *Lines 24-26: The reference to the commercial company PlanetiQ and the government- sponsored mission COSMIC-2, which implies that their high SNR is not necessary, is misleading. It is not true that anyone that I know of has ever claimed that high SNR is an "essential" advantage, certainly not Sokolovskiy et al., 2019, where the potential advantages of high SNR are conservatively stated.*

   I meant a discussion about the importance of high SNR at IROWG-2019 in Elsinore. Anyway, this is an important remark. I updated the text along these lines.

6. *COSMIC should be defined and at least one reference provided. Also the source of COSMIC data used in this paper should be given.*

   OK.

7. *Eq. 3: Why do the integrals go from 0 to Δt for the t_j sample? Should it rather be from t_j to t_{j+1}?*

   Strictly speaking, yes. But, because the random processes ξ_m are assumed to be stationary, the origin of time axis can be shifted to t_j. Still, once such a question arises, I replaced t' with $t_j + t'$ and t'' with $t_j + t''$ in Eq. (3). This does not affect Eqs. (4) and (5), because due to stationarity, t_j cancels out.

8. *Eq. 3: Should the limits on the other two integrals be from $-\infty$ to ∞?*

   Integrals over ω' and ω'' without explicitly specified limits are understood as integrals from $-\infty$ to ∞. I added the explicit integration limits in Eqs. (3) and (5).

9. *Eq. 3: The use of symbols ξ' and ξ'' are unfortunate choices here, since ξ is also used as symbol for the random noise. As I understand it, ξ' and ξ'' are equal to $2\pi\omega'$ and $2\pi\omega''$, respectively, but the factor of 2π seems to be forgotten in eq.5 where it says $\exp\left(i\omega(t'-t'')\right)$)). I think it should be $\exp\left(i2\pi\omega(t'-t'')\right)$, and the result in the second line of eq.5 should therefore be without the factor of 2π.*

   This is a typo: ξ' and ξ'' should be ω' and ω''. The formula is corrected.

10. *Eq. 3-5: Perhaps f would be a better symbol to use than w in these equations, since is not an angular frequency. Just a suggestion.*

    As explained above, it should be the angular frequency ω rather than ξ.

11. *Line 59 and elsewhere: The noise spectral density is normally written C/N_0 (not CN_0). Please change.*

    OK.

12. *There are other examples of inaccuracies in sections 1 and 2. For example, the author mentions the SNR in dB-Hz in line 72, while it was previously defined in V/V in lines 1113 (SNR can be expressed in dB, but not in dB-Hz). He does not note that the relative noise level in line 71 equals 1/SNR (as indicated in Table 1). Also, the author does not mention in line 13 that the SNR corresponds to a given sampling band. The latter is important (see next comment).*

    It is a typo. The value expressed in dB-Hz is, of course, C/N_0 .

13. *Commonly, both the C/N0 (per unit bandwidth) and the SNR (in a given bandwidth) are used for describing signal processing in a receiver, which includes a number of integrations and down-samplings. This is not considered in the paper. The noise model is white noise in the sampling band, as stated in line 54, and can be described by the SNR only. There is no value in introducing C/N0 in the context of this paper and the whole section 2 is not needed. But it is important that the SNRs in Table 1 are defined in the sampling band f_S. In this paper the relationship between SNR and C/N_0 is*

    $$SNR = \sqrt{\frac{2 \times 10^{0.1\,C/N_0}}{f_S}}$$

    and thus is a function of f_S. Although f_S is not explicitly specified in the paper (it should be), it can be concluded from eq. 8 and numbers in Table 1 that f_S = 50 Hz (consistent with the COSMIC f_S), and the SNRs in Table 1 are defined in the 50 Hz band, when commonly SNR values are given for f_S = 1 Hz. Thus the values for SNR in Table 1 are lower by a factor of $\sqrt{50}$ or 7.071 compared to the SNR as used in most RO papers, and therefore misleading. For example, 50 Hz SNR = 454 V/V (top line in Table 1) is equivalent to 1 Hz SNR = 3210 V/V.
    It should also be noted in the text or in the table caption if these are SNR's at the top of the atmosphere where the signal is not affected by defocusing and atmospheric multipath, or if they

are something else. Please clarify.

It is true that we do not model the receiver. We added the definition of $f_s = 50$ Hz (which applies to all the current mission). We added a remark that the SNR is evaluated for the signal amplitude at the top of the atmosphere, as discussed below.
It is correct that there are 1-Hz SNR and 50-Hz SNR, as stated in (Sokolovskiy, 2010). We added a column with 1 Hz SNR to the table.

14. *Eq. 6: What does $<A_m(t)^2>$ mean? In the previous equations the brackets mean expectation value of a stochastic variable, but that seems not to be case here. Is it an average over time? Over which time interval? In the troposphere, $A_m(t)$ becomes very small due to defocusing, right? Please clarify.*

    I agree that it was not quite correct to write $\langle A_m^2(t)\rangle$, because $A(t)$ is not a stationary process. It is better to replace it with $A_{0,m}^2$ which is defined as the RO signal amplitude averaged over large impact altitudes, where the defocusing effects are negligible.

15. *Please provide a reference for eq.7.*

    I added references (Kaplan, 1996) and (Beyerle et al., 2006).

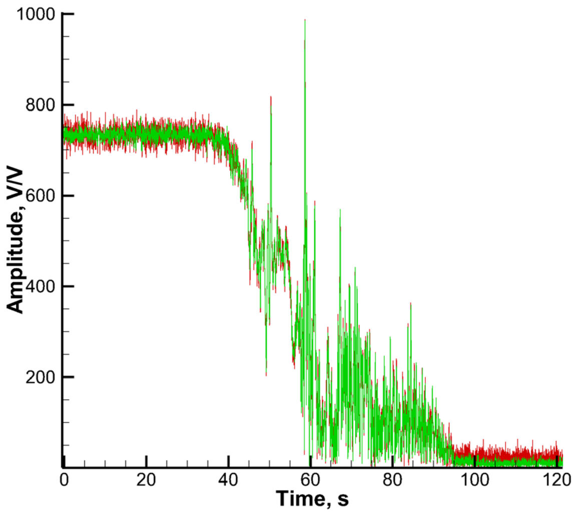
<u>*Section 3 Numerical Simulations*</u>

16. *How are the noise levels on the L2 signal defined when adding noise to COSMIC data? Same C/N_0 as for L1? How is $\langle A_m^2(t)\rangle$ obtained when adding noise to COSMIC data? Please clarify.*

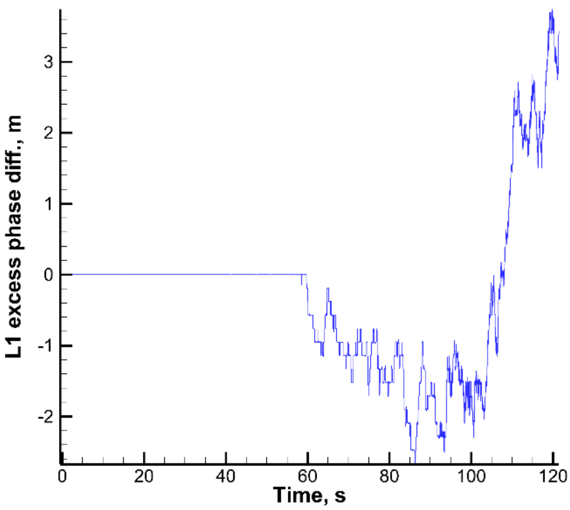
    Noise levels were same for L1 and L2. Upon the suggestion of another reviewer, I replaced $\langle A_m^2(t)\rangle$ with $A_{0,m}^2$, which is evaluated for large impact altitudes, where refractive attenuation is negligible. Corresponding explanations have been added to the text.

17. *It is unclear how the random noise is applied, in particular because it is unclear what $<A_m(t)^2>$ in eq. 8 means. To my understanding, the noise level of the complex signal (LHS of eq.8) should be constant during the whole occultation such that when $A_m(t)$ becomes small in the troposphere, the phase noise goes up. Is that how it was applied? Please clarify.*

    More details on the generation of random noise realizations and their application have been added. Yes, noise level is constant.



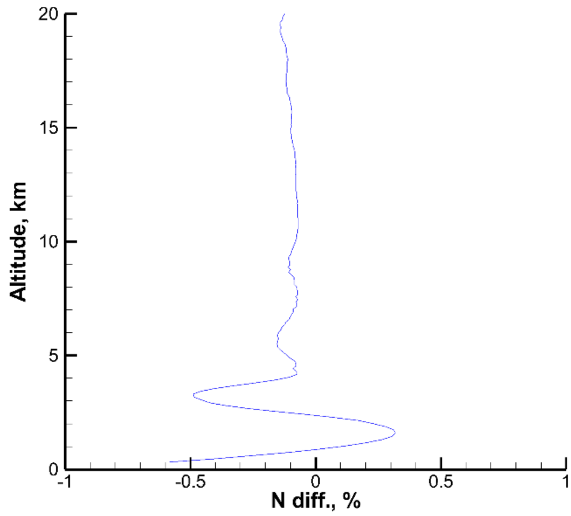
Original (green) and noisy (red) amplitudes.



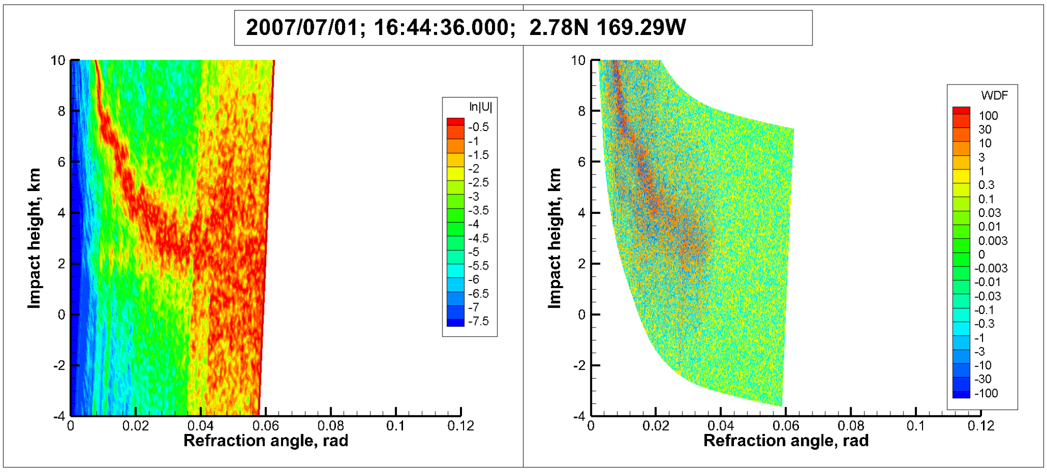
The L1 phase perturbation due to noise.

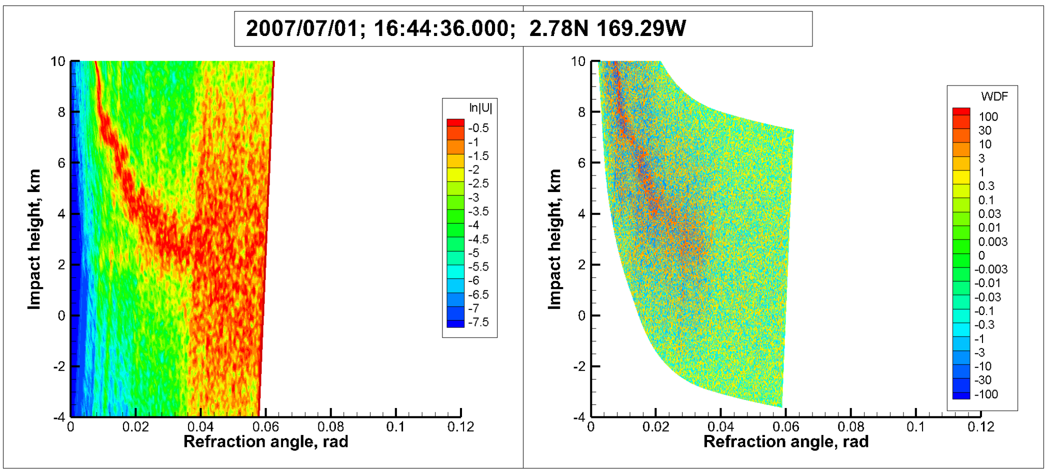
18. *One or more examples of noise realizations as a function of time during occultations would be illustrative in showing the increase of the phase noise in the troposphere, while the SNR (in V/V) becomes small. How does it look in cases of atmospheric multipath?*

Good examples can already be found in (Sokolovskiy, 2010). Still, not for the paper but just for Reviewer, I give the same example as in (Sokolovskiy, 2010): C004.2007.182.16.44.G25.



Retrieved refractivity perturbation due to noise.





Original (upper) and perturbed (lower) spectrogram (left) and Wigner distribution function (right).

19. *Lines 66-67. Please say more about how the ECMWF data are used. There is no mention of how the simulated data are inverted to atmospheric profiles. Is spherical symmetry assumed? Were the data (complex signals; phases; amplitudes; bending angles?) smoothed or filtered differently for the different noise levels? Please provide some information on this, possibly with references.*

    I added more details on ECMWF data and the filtering of RO data. The previous works describing the data processing were cited.

20. *And how do you initialize the refractivity retrievals? As the paper notes, residual ionospheric noise is an important contributor to RO N retrievals. The refractivity RMS values in Fig. 2, which decrease with height above 30 km, seem far too small (less than 0.3% above 10 km, even for the lowest SNR tested). In real data, the refractivity RMS values increase with height above 30-35 km (e.g. O-B statistics; Rennie, 2010 Fig. 2, Kuo et al. (2004, Fig. 10), Schreiner et al., (2020, Figs. 5 and 6).*

    I added an explanation of the initialization. I use the statistical optimization, and the background is the collocated profile from the ECMWF analyses. This explains small errors at large heights. This also explains why they get smaller for stronger noise levels.

21. *The "reference" refractivities in Figs. 1 and 2 need to be defined more precisely. Is it the retrieved N using the ECMWF data with 0 errors added and your forward model? Or is it the local ECMWF N computed from the Smith-Weintraub equation using the local values of ECMWF p, T and e? It appears to be the latter, otherwise the mean and RMS differences from the reference would converge to zero as the SNR increases.*

    The background bending angle profile used in the statistical optimization was evaluated by the Abel transform from the collocated ECMWF profile of refractivity.

22. *It should be noted whether the ECMWF data have any layers of super-refraction (SR) in them, especially in the tropical lower troposphere. They probably do, and if so, how are the RO retrievals obtained in these layers?*

    ECMWF data definitely have super-refraction, and the RO retrievals are obtained using the algorithm that have been described in previous works: bending angle retrieval using combined GO and WO methods, the statistical optimization, the Abel inversion, and the hydrostatic equation integration. In other words, I use the standard data processing chain, which is the same for simulated and artificial data. The question is not quite clear.

23. *Fig. 1: What is the cause of the positive N biases in Fig. 1? Why is there a mean difference of up to ~0.1% in refractivity below 25 km even for the high C/N_0 values? Horizontal gradients? Other reasons? Please discuss in text.*

    Currently, I have no explanation for the 0.1% bias in the simulations. I suspect some purely numerical effects.

24. *Fig. 2: As noted above in Comment #20, the RMS errors (differences between the retrieved and reference refractivities) shown in Fig. 2 are lower than most estimates, even for the maximum error imposed on the SNR. Why do the RMS values decrease with increasing altitude above 25 km; they should increase? And why are the RMS values largest for the largest C/N_0 values? As the C/N_0 (SNR) becomes larger, the RMS differences should become smaller. Why do the RMS values below 10 km reach a positive limit for all C/N_0 values (black profile)? What is the reason for the maxima in RMS values around 25 km at all latitudes? This level is far above the tropopause level in middle and high latitudes. The results here clearly do not make sense. Why would there be a threshold where higher C/N_0 values do not make any difference in the lower troposphere? And why would that threshold be that low? There is no threshold in the simulations with COSMIC data (Fig. 7), although this is not discussed at all. In Fig. 7 the RMS clearly becomes smaller when the SNR becomes larger. This contradicts the results in Fig. 2. It also contradicts the main conclusion*

of the paper (in the Abstract and Conclusions) that there is a threshold. I doubt these results; but in any case, there needs to be a discussion about the mechanism if there really is such a threshold.

The decrease of RMS for smaller C/N_0 above 25 km are explained by the statistical optimization. Regarding the narrow maxima around 25 km, the following can be stated. 25 km is the WO-GO transition height. The maximum is only observed for the strongest noise. This maximum indicates the different sensitivity of WO and GO processing to noise, and WO-GO combination algorithm errors for such noisy signals.
Regarding this statement: *The results here clearly do not make sense.* I performed very simple simulations with the standard and well-established data processing chain, in order to estimate its sensitivity to white noise in the input RO signal. In my view, Reviewer does not clearly formulate what is wrong here. See also the discussion below.
Why would there be a threshold where higher C/N₀ values do not make any difference in the lower troposphere? There is no threshold in the simulations with COSMIC data (Fig. 7), although this is not discussed at all. These statements are not clearly formulated. The influence of the noise indicates a weak dependence from C/N_0, unless it falls below some threshold, which is about 27 dB-Hz, as clearly can be seen in the plots, both for lower troposphere and for stratosphere. And this is discussed at the end of the Section. The similar situation is for COSMIC data, but, because they already have their own noise, the effect of the white noise starts being visible at 37 dB-Hz.

25. *There needs to be more explanation and discussion of Figs. 4 and 5. At present there is no discussion or interpretation (only one sentence referring to these figures). As noted above, the reference ECMWF should be defined; is it the local ECMWF N from the Smith - Weintraub equation or your retrieved N from the ECMWF data before errors were added? What causes the large maximum in biases at between 35 and 40 km (Fig. 4)? What causes the negative biases between 10 and 30 km? What causes the large positive bias in the low SNR experiment (orange profile in Fig. 4)? Why does the RMS for the smallest C/N₀ profile (orange) in Fig. 5 reach a maximum around 5-9 km and then decrease toward zero at lower levels? The maximum values and the levels at which they occur should be given in the Figure captions when the profiles extend beyond the limits of the x-axis (or the x axis extended).*

    We added the definition of the reference ECMWF fields. Discussion of biases about 0.1–0.2% is beyond the scope of this paper. The plausible explanation of the positive bias caused by noise was given by Sokolovskiy (2010). The orange curve goes to zero, where no data pass the QC. The paradoxical decrease of RMS for strong noise is also explained by the reduction of the number of data.

26. *Similar to the previous comment, there needs to be more explanation and interpretation of Figs. 6 and 7. Here the reference profile is the COSMIC profile without errors added. But what COSMIC profiles do you use? Do you use the COSMIC profiles of N reported by the source of your data (e.g. CDAAC or ROM SAF), or the COSMIC profiles of N that you compute from the observed low-level data you start with, either with no noise added or varying noise?*

    These figures show the perturbation of the retrieved refractivity due to noise. The reference refractivity is that retrieved by my data processing chain from the original COSMIC data. We made this statement clearer in the text.

27. *Fig. 7 is labelled incorrectly; it is the RMS differences not the mean differences.*

    This is corrected.

28. *The paper concludes that SNR values above a very small threshold of about 10 V/V are not important in RO retrievals. Yet Schreiner et al. (2020, Fig. 2b) show that the penetration depth of RO increases significantly for SNR increasing from 500 V/V to 2500 V/V. They did not consider SNR less than 500 V/V. Also, high values of SNR are important for detecting super-refraction. These properties are important for RO soundings into the moist lower troposphere. These are additional reasons that it is misleading to conclude that SNR values above 10 V/V are unimportant.*

I agree that it would be misleading to state that any SNR exceeding 10 V/V is equally good under realistic conditions. The conclusion needs more conservative formulations. I added some discussion of (Schreiner, 2020) upon the similar question of Kursinski. (Schreiner, 2020) indicates that GRAS-METOP has the worst penetration, although, according to (Engeln, 2011), GRAS indicates a lower BA noise compared to COSMIC. It would also be expedient to perform a similar study of with COSMIC-2 data in the future.

Additional references:
Ao, C. O., G. A. Hajj, T. K. Meehan, D. Dong, B. A. Iijima, A. J. Mannucci, and E. R. Kursinski (2009), Rising and setting GPS occultations by use of open-loop tracking, J. Geophys. Res., 114, D04101, doi:10.1029/2008JD010483.

Gorbunov, M. E., V. V. Vorob'ev, and K. B. Lauritsen (2015), Fluctuations of refractivity as a systematic error source in radio occultations, Radio Sci., 50, 656-669, doi:10.1002/2014RS005639.

Kuo et al., 2004: Inversion and Error Estimation of GPS Radio Occultation Data. Journal of the Meteorological Society of Japan, Vol. 82, No. 1B, pp. 507--531, 2004 507

Rennie MP. 2010. The impact of GPS radio occultation assimilation at the Met Office. Q. J. R. Meteorol. Soc. 136: 116-131. DOI:10.1002/qj.521

Schreiner, W. S., Weiss, J. P., Anthes, R. A., Braun, J., Chu, V., Fong, J., et al. (2020). COSMIC-2 radio occultation constellation: First results. Geophysical Research Letters, 47, e2019GL086841. https://doi.org/10.1029/2019GL086841

Sokolovskiy, S., C. Rocken, W. Schreiner, and D. Hunt (2010), On the uncertainty of radio occultation inversions in the lower troposphere, J. Geophys. Res., 115, D22111, doi:10.1029/2010JD014058.

Sokolovskiy, S., W. Schreiner, Z. Zeng, D. Hunt, Y.-C. Lin, and Y.-H. Kuo (2014), Observation, analysis, and modeling of deep radio occultation signals: Effects of tropospheric ducts and interfering signals, Radio Sci., 49, doi:10.1002/2014RS005436.

I added most of these references.