

## Supplement

Review of paper by M. Gorbunov for AMT: **The influence of the signal-to-noise ratio upon radio occultation inversion quality.**

### Detailed comments

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.
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.
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?
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.
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.
6. COSMIC should be defined and at least one reference provided. Also the source of COSMIC data used in this paper should be given.
7. Eq. 3: Why do the integrals go from 0 to  $\Delta t$  for the  $t_j$  sample? Should it rather be from  $t_j$  to  $t_{j+1}$ ?
8. Eq. 3: Should the limits on the other two integrals be from  $-\infty$  to  $\infty$ ?
9. Eq. 3: The use of symbols  $\xi'$  and  $\xi''$  are unfortunate choices here, since  $\xi$  is also used as symbol for the random noise. As I understand it,  $\xi'$  and  $\xi''$  are equal to  $2\pi\omega'$  and  $2\pi\omega''$ , respectively, but the factor of  $2\pi$  seems to be forgotten in eq.5 where it says  $\exp(i\omega(t'-t''))$ .

I think it should be  $\exp(i2\pi\omega(t-t'))$ , and the result in the second line of eq.5 should therefore be without the factor of  $2\pi$ .

10. Eq. 3-5: Perhaps  $f$  would be a better symbol to use than  $\omega$  in these equations, since is not an angular frequency. Just a suggestion.
11. Line 59 and elsewhere: The noise spectral density is normally written  $C/N_0$  (not  $CN_0$ ). Please change.
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 11-13 (SNR can be expressed in dB, but not in dB-Hz). He does not note that the relative noise level in line 71 equals  $1/\text{SNR}$  (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).
13. Commonly, both the  $C/N_0$  (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  $C/N_0$  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

$$\text{SNR} = \text{SQRT}[(2.0 \times 10^{0.1 C/N_0})/f_s],$$

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  $f_s = 1$ . Thus the values for SNR in Table 1 are lower by a factor of  $\text{SQRT}(50)$  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.

14. Eq. 6: What does  $\langle A_m(t)^2 \rangle$  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.
15. Please provide a reference for eq.7.

### Section 3 Numerical Simulations

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  $\langle A_m(t)^2 \rangle$  obtained when adding noise to COSMIC data? Please clarify.
17. It is unclear how the random noise is applied, in particular because it is unclear what  $\langle A_m(t)^2 \rangle$  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.
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?
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.
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).
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.
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?
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.
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.

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  $C/N_0$  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).
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?
27. Fig. 7 is labelled incorrectly; it is the RMS differences not the mean differences.
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.

Additional references:

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