E.R. Kursinski Review of
"The influence of the signal-to-noise ratio upon radio occultation inversion quality",
M. Gorbunov 2020

I think this is interesting and useful work and results. However, the work needs some clarifications. The impact of SNR at higher altitudes is inconsistent between the figures. How the SNR affects RO results in the lower troposphere, particularly in terms of random and systematic errors and the minimum altitude to which the accurate profiles extend vs SNR, needs to be clearly shown. Relevant prior work needs to be discussed and how the results here compare to those. The assertions about unimportance of detecting super-refraction either need to be better justified or removed.

Based on the abstract, the paper has two primary conclusions

(1) *Our simulations indicate that the effect of additive white noise has a threshold character: the influence of the noise is very low up to some threshold, but when the threshold is exceeded, the influence increases dramatically*.
(2) *Another conclusion is that, given RO observations of fair quality, the enhancement of the SNR cannot be expected to provide significant improvement in retrieval quality*.

The first conclusion seems reasonable but it needs to be made clear that there is an altitude interval over which that statement is true and the upper and lower bounds of that altitude range depend on the SNR. There are issues with the second conclusion discussed further below.

Key points:

- **Insensitivity to SNR has altitude dependence**: The paper's conclusion about SNR seems generally correct (depending on the answers to some questions below) but it needs to be made clear that this is true over the altitude range where SNR is not the limiting error source and that altitude range depends on the SNR.
- **Problems with Figure 2**: The impact of SNR versus altitude shown in Figure 2 doesn't make sense. At high altitudes, the fractional refractivity errors as a function of SNR should increase exponentially with altitude and look somewhat like the result in Figure 7 as well as Kursinski et al 1997 (hereafter K97). The fact that the results look so odd makes it difficult to trust and use this figure in understanding the rest of the results in the paper.
- **Depth of penetration vs SNR**: An important figure of merit for RO profiling of the atmosphere is how close to the surface are occultation profiles able to accurately profile the atmosphere. This is a function of SNR. Some indication of this is shown in Figures 3 and 8 but only at two altitudes, 5 and 20 km. For instance, in Figure 8, the number of profiles at 5 km drops from about 875 for an SNR of 37 dB-Hz to about 775 for an SNR of 32 dB-Hz. The depth to which RO can accurately profile is an important figure of merit and an area where people expect measurements made with higher SNRs to enable the profiles to extend deeper and closer to the surface. A figure needs to be added that shows the number of occultations vs altitude vs each SNR that was examined. This would enable readers to see whether there is substantial value in higher SNRs for profiling closer to the surface.
- **Errors in the lowermost troposphere**: In Figures 1, 2, 4, 5, 6, and 7 which extend from the surface to 50 km, it is difficult to see the behavior in the lowermost troposphere. An additional figure is needed that shows the behavior in the lower troposphere, say below 4 or 5 km, particularly for the 0-30 deg latitude region, which is typically the most challenging for RO.
- **Threshold explanation**: The SNR threshold described in the manuscript was noted in K97 as the minimum SNR required to detect the GPS signals. I think that the author should note

that and use it is the paper's discussions on the threshold and its variations depending on conditions. (more below)

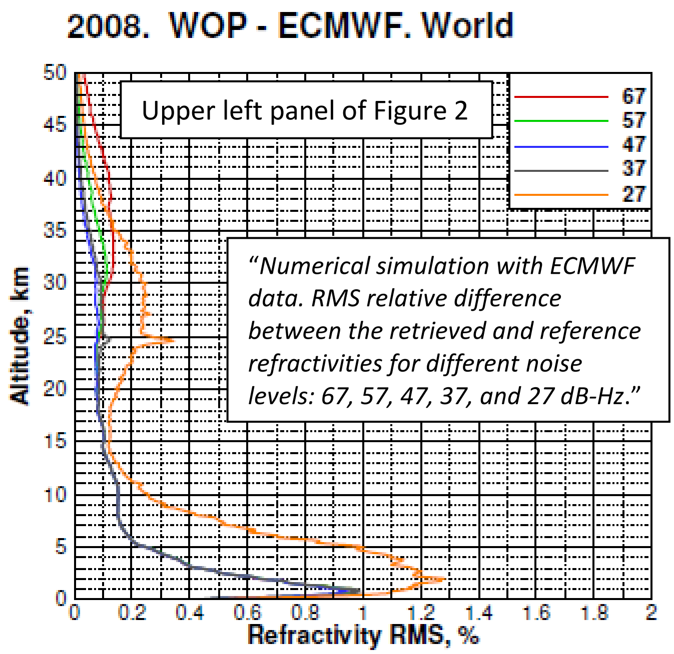
- **Types of errors present in Figures 1 & 2, 4 & 5, and 6 & 7**: These 3 pairs of figures show mean and RMS errors for WOP-ECMWF, COSMIC-ECMWF and COSMIC_N-COSMIC. It would be clarifying and helpful to the reader if the manuscript were to note the different types or sources of error that are present and not present in each of these figure pairs. I note this because the types of error are quite different from one pair to the next, which took me a while to appreciate, and an understanding of those differences is required to interpret the figures. For instance, The COSMIC_N – COSMIC Figures 6 and 7 show the SNR effect but, because they take the COSMIC-derived refractivity profiles as truth, which were presumably derived via the Abel transform, they do not include any horizontal or super-refraction effects. Thus, the addition of thermal noise associated with different SNRs essentially creates no refractivity bias in these results. I presume the RMS errors in WOP-ECWMF Figure 2 below 10 km are due to horizontal structure but, again, I am not sure about what is shown in that Figure.
- **SNR impact in Figure 7 vs K97**: The curves of RMS errors in Figure 7 showing SNR impacts appear to be self-consistent with one another in terms of their scaling with SNR. However, they are about an order of magnitude smaller than K97 for the same SNR. Why is that?
- **SNR-noise amplification**: There is no discussion of the processing steps that amplify the noise associated with finite SNR such as the ionosphere calibration, clock cancellation and vertical resolution.
- **Vertical resolution issues**: There is no discussion of vertical resolution and how it is related to SNR and the amplification of thermal noise when pushing to higher vertical resolution and the advantages of higher SNR to achieve higher vertical resolution and more complete removal of the ionosphere.
- **Figure 3a vs 8a**: The SNR noise in these two figures is quite similar at 20 km altitude but quite different at 5 km. I am guessing this is related to the vertical smoothness of the ECWMF analyses used here vs. the real world structure as captured in COSMIC data. Can the author comment on this? Will the degradation to refractivity due to SNR be still larger in COSMIC-2 data which has higher SNR and can resolve more variations than COSMIC could?
- **Does higher SNR enable higher vertical resolution retrievals**? Certainly experts in the field seem to think so. Does it do so over certain altitude intervals?
- **Super-refraction importance**: The author asserts detecting super-refraction is of limited importance for NWP. As I discuss below, detecting and accounting for super-refraction actually is important for NWP and other applications of RO. More below…
- **RMS vs stdev**: I know the figure say RMS but I want to make sure they are really RMS and not standard deviations. I would also note that I personally prefer figures that show bias and stdev (rather than RMS) because bias and stdev are independent of one another whereas RMS has both the bias and stdev included in it, making it more difficult to interpret (in my opinion).
- In Figures 4, 5, 6 and 7, the orange curves shoot off the right or left hand edge of the figures at 1 to 2 km altitude. Why are they doing that and what does it means?

In reviewing this manuscript, I find it necessary to lay out some context and expectations about what we think we understand about SNR before going through the results of the paper. SNR is important to GNSS RO observations for several reasons including

(1) SNR limits how high RO profiles can extend because the phase jitter associated with finite SNR is one of the factors that determines how small an atmospheric density can be accurately measured by GNSS RO (K97).
(2) There is a SNR threshold to the detectability of the GNSS signals (K97).
(3) This threshold limits how deep into the troposphere GNSS RO occulted signals can be tracked through the effects of defocusing and other perturbations caused by the atmosphere,
(4) A sufficiently high SNR enables detection of super-refraction (Sokolovskiy et al 2014), an effect that causes a fundamental ambiguity between bending angle and index of refraction and results in negative refractivity biases in the lowermost troposphere which have limited the utility of GNSS RO in the lowermost troposphere.

I. Point 1: Impact of SNR at higher altitudes

I.a Somewhat surprisingly, the results in Figures 2 and 5 of the manuscript don't agree with Point 1 above. Specifically,

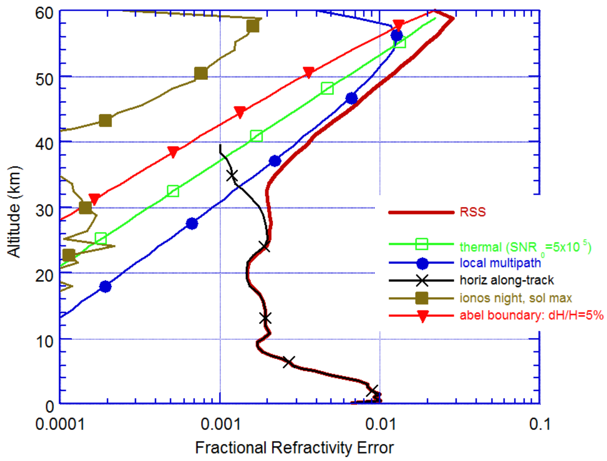


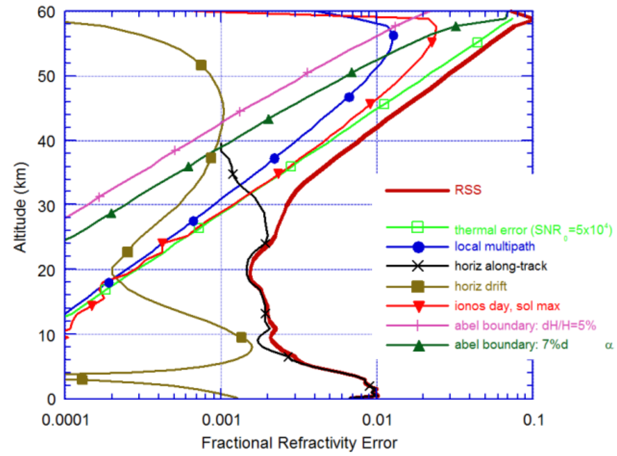
- Figure 2 shows fractional refractivity error results versus altitude. One problem is the figure shows that the fractional errors due to finite SNR go to zero at high altitude. But this is not plausible because bending angle and refractivity, decrease exponentially with increasing altitude while the error due to finite SNR is approximately constant with altitude. Thus, the fractional refractivity errors should increase exponentially with altitude in the middle atmosphere as they do in Figure 7. In order for the fractional errors to *decrease* with increasing altitudes, the refractivity error due to SNR would have to decrease even faster with altitude than the bending angle and refractivity do. More below…
- Also the higher signal to noise ratios (SNRs) have the largest errors at high altitude which also does not make sense.
- In Figure 5, the curves for 4 out of the 5 SNRs sit on top of one another. Lower SNRs should have larger errors at higher altitudes. Certainly, better SNR won't make the results worse. Something is wrong with the Figure or else I don't understand what is being plotted in the figure.

I.2 Previously published SNR dependence of RO noise with altitude:

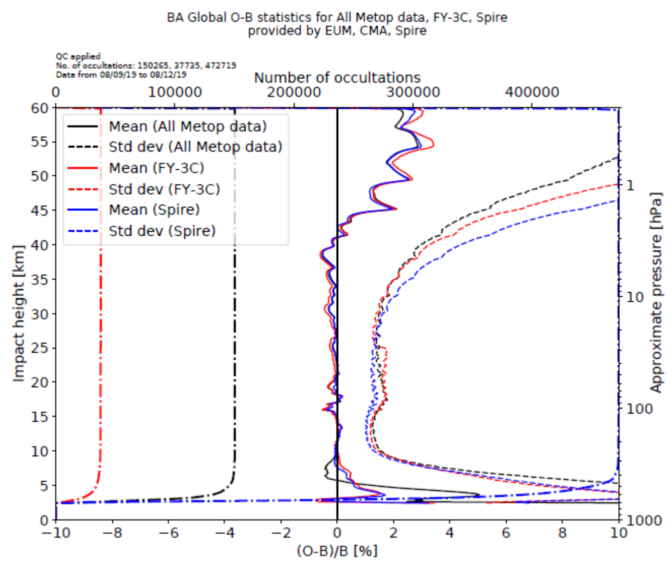
The two figures below from K97 show fractional refractivity error versus altitude which include the contributions of SNR shown in green. The left hand figure shows the fractional refractivity error for a one second voltage SNR of 1000 v/v which is equivalent to 5×10^5 W/W = 57 dB-Hz, which is 20% higher than a typical COSMIC-1 voltage SNR. The right hand figure includes the fractional refractivity error for a one second voltage SNR of ~300 v/v which is equivalent to 5×10^4 W/W = 47 dB-Hz and representative of SNRs from GPS-MET in 1995-1997 and similar to but a bit lower than typical Spire SNRs, based on NOAA's recent assessment of Spire data.

According to the figures below, 1% errors in refractivity corresponding to 57 and 47 dB-Hz are at 53 km and 45 km respectively. Thus, when the SNR is decreased by 10 dB, the curves predict about an 8 km penalty in terms of the altitude where a certain fractional error is reached. 8 km makes sense because the pressure scale height near 50 km is about 7 km and the natural log of (sqrt(10)) = 1.15 and 7km*1.15 = 8 km. Figures 2 and 5 show no such altitude dependence.





I.3 Met Office Report on Spire results: The Met Office report of N. Bowler in March 2020 assessing Spire data indicate that Spire results have larger bending angle errors at altitudes above 30 km than the METOP or FY-3C data (see figure to the right). Given that the three sets of data from METOP, FY-3C and Spire were all taken over the same period, and under the same ionospheric conditions, aren't Spire's larger errors above 30 km altitude due to their lower SNRs? If not, what is causing Spire's errors to be larger at higher altitudes?



I.4 COSMIC data WO below 20km, GO above 20 km:
In deriving atmospheric profiles from the COSMIC measurements, wave optics (WO) were used to derive the portions of the profiles at altitudes below 20 km while geometric optics (GO) were used above 20 km altitude. I thought that the reason for this transition at 20 km was that the WO results were noisier than the GO results at higher altitudes associated with the atmospheric bending signal decreasing exponentially with altitude, while the noise due to limited SNR was approximately constant.
Was this not the case? If not, why was there such a WO-GO transition at 20 km? The results in Figure 2 seem to imply WO should have been used up to at least 50 km (and that lower SNR would have yielded better results than higher SNR).

I.5 Noise amplification in RO processing

Discussions of sensitivity to SNR raise the issue of amplification of the effects of thermal noise that occurs in standard steps in the RO processing. These include (1) estimating and removing the effects of the ionosphere, (2) cancelling noise from the receiver reference oscillator and (3) vertical resolution. These are not discussed in the manuscript and should be. These are certainly important at higher altitudes.

- **Ionosphere correction**: As noted in K97, when the ionosphere correction is applied at full resolution, the thermal error is magnified by a factor of 3. Hajj et al., 2002 noted that smoothing can be done on the ionosphere correction to reduce this amplification at the expense of leaving finer vertical scale residual ionosphere effects in the ionosphere-corrected bending angle profiles. With higher SNR observations, the full resoluteion ionosphere correction could be applied to avoid fine scale ionosphere leaking through the ionosphere correction process while still causing limited errors due to the finite SNR.
- **Noisy reference oscillator**: When the receiver reference oscillator is noisy, single differencing is required to cancel the oscillator noise which increases the thermal phase errors by square root of 2. I would guess that cubesats like Spire's can't afford to carry particularly stable reference oscillators and therefore require single differencing to cancel the noise of their reference oscillator.
- **Vertical resolution**: As discussed in Hajj et al. (2002), the impact of thermal noise on the bending angles and refractivity scales very strongly with vertical resolution, as $\Delta z^{-3/2}$ where Δz is the vertical resolution. Thus, if the vertical resolution is increased by a factor of 2, the noise on the refractivity due to thermal noise increases by a factor of 2.8. Vertical resolution is therefore very important when discussing the effects of finite SNR, at least in the middle atmosphere.

I.6 Vertical resolution

There is no mention of vertical resolution in the manuscript. As just noted, Hajj et al. (2002) described a strong relation between vertical resolution and SNR, at least at higher altitudes, that should be noted in this manuscript. If the Hajj et al results are somehow incorrect, that would be a very important conclusion that the author should note and explain.

I.7 Does higher SNR enable higher vertical resolution?

In discussions with other experts, the higher SNRs systems like COSMIC-2 are expected to enable finer vertical resolution at a given altitude, to the extent that the thermal noise associated with the finite SNR is what is limiting going to higher vertical resolution. Does the author agree or disagree with this?

If lower SNR is not the limiting error, does that mean the vertical resolution can be increased?

If SNR is the limiting error, can vertical resolution be decreased to make SNR not be the limiting error?

I.8 Reduced vertical resolution at altitude to compensate for lower SNR?

Given the odd, high altitude results in Figures 2 and 5 and the strong dependence of errors on vertical resolution noted in Hajj et al. (2002), it struck me that there actually is a way to keep fractional errors from increasing with altitude at higher altitudes and that is to reduce the vertical resolution of the profiles at higher altitudes to compensate for the exponentially decreasing atmospheric bending and refractivity. One could achieve this by reducing the vertical resolution by about a factor of 2 for every scale height increase in altitude. However, I don't see any sign of that being done in this manuscript. As I noted above, I don't see any mention of vertical resolution in the manuscript, which I find surprising given the vertical resolution is one of the fundamental features of GNSS RO for profiling the atmosphere.

I.9 Magnitude of SNR errors in K97 and the present manuscript
The SNR errors in green in the right hand figure of K97 above correspond to a SNR of 47 dB-Hz. The table below summarizes the two estimates at different altitudes. Why don't the SNR errors from K97 and Figure 7 results match for 47 dB-Hz? My guess is that it has in part to do with the K97 including the effects of the ionosphere correction and the clock correction. Different vertical resolutions in the two papers may play a part as well. The author needs to provide some discussion and explanation for this apparent discrepancy.

Altitude (km)	K97	Fig 7 of this manuscript
45	1%	0.12%
35	0.25%	0.06%

II. Point 2: SNR threshold
In the manuscript's conclusion, it states "*The simulations indicated that the effect of additive white noise has very much a threshold effect. For values of C/N0 > 27-32 dB-Hz, the effect of the noise is very low and does not influence the retrieval quality significantly. For WOP data, the noise threshold is independent from height, while for COSMIC data the lower-tropospheric data indicate stronger, noise-like fluctuations and, therefore, a higher value of the threshold C/N0 = 32 dB-Hz. For weaker SNRs, e.g., when C/N0 . 17 dB-Hz, the data become unusable*."

Section 2.4 of K97 noted a fundamental SNR threshold exists, stating that "In order to acquire an occulted signal, the signal-to-noise intensity ratio (SNR) at the receiver must exceed a critical value of about 10. If SNR falls below this value, phase lock and the signal are lost." The point is that below this intensity SNR, the signal becomes essentially undetectable.

COSMIC occultation data was acquired with a 50 Hz bandwidth which was chosen to be wide enough to capture most of the errors and uncertainty of the open loop atmospheric Doppler model and the spread in signal frequencies due to atmospheric multipath. (It also fortuitously matched the GPS navigation message bit rate, enabling a relatively easy transition from the JPL ground based receiver design to the flight version). The minimum signal to noise intensity ratio of 10 in a 50 Hz noise bandwidth translates to an intensity SNR of 500 in a 1 Hz which is 27 dB-Hz, consistent with the author's findings via simulations. I think the author should note that the threshold he found in simulations is associated with the fundamental SNR noted in K97 needed of the signal to be detectable above the noise.

I also note that in order to recover a higher percentage of the signals deep in the occultations, the noise bandwidth on COSMIC-2 has been increased to 100 Hz. In order to maintain the critical signal-to-noise intensity ratio of 10 means that the SNR threshold will increase from 27 dB-Hz to 30 dB-Hz for the COSMIC-2 measurements. This can be reduced if subsequent filtering is done on the data to narrow the noise bandwidth at the risk of losing the signal if and when it falls outside that narrower bandwidth.

I also note that I find it odd that the high altitude results in Figures 2 and 5 can be implausible while the author's results in terms of a threshold SNR appear consistent with previously defined expectations. I do not know how to reconcile this.

III. Point 3: Higher SNR improves the ability to profile closer to the surface

The figure below from Kursinski and Hajj (2000) shows a histogram of the lowest altitude of profiles acquired by GPS MET in 1995 with 1 second voltage SNRs of roughly 300 v/v which are equivalent to 46.5 dB-Hz and similar to but a bit lower than the SNRs of the Spire cubesats, according to the NOAA report on Spire's data. The left hand side shows closed loop results. The RHS shows results obtained using a simple version of open loop tracking called "fly-wheeling".

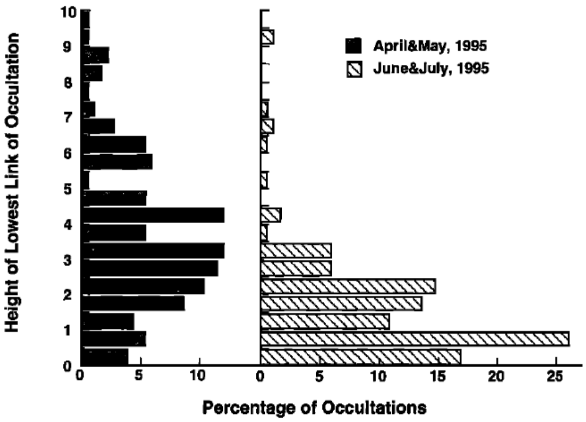


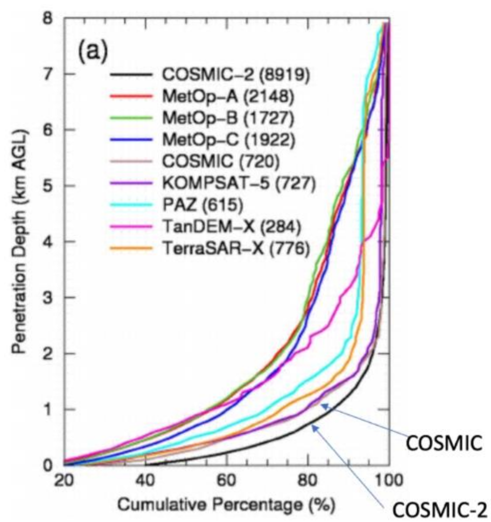
Figure 2. Comparison of histograms of the minimum altitude of occultation profiles for two periods of the GPS-MET mission. (left) Data from April 24 and 25 and May 4 and 5, 1995. (right) Data from June 21, 22, 23, and 27, 1995. Occultations during the second period probed systematically deeper into the atmosphere because of GPS-MET receiver software uploaded during this period modified to significantly improve signal tracking in the troposphere (see text for details).

These relatively low SNR profiles extend rather deep, with approximately 50% reaching to within 1.5 km of the surface and 17% (1 in 6) of the profiles extending to within 500 meters of the surface. Thus, rather deep tracking can be and indeed has been achieved with relatively low SNRs.

In looking at the right side of the Met Office figure above, it appears that about 50% of the Spire occultation profiles reached to about 1.1 km altitude which is a bit better than the GPS-MET results, as one would expect given that Spire's voltage SNRs are 20% higher than GPS-MET and the open loop model in the Spire receivers is presumably better than the simple fly-wheeling model used in summer 1995.

Importance of profiling to the surface

Having said that, users of RO, including those who predict weather and air quality and those who use that information in decision making, presumably desire RO profiles that extend to the surface 100% of the time across the globe. The lowest levels of atmosphere are in fact quite important given that we live on the surface and therefore care about the weather and air quality at the surface. Furthermore, most of the water vapor in the atmosphere is near the surface (perhaps 50% typically resides in the lowest km) and that water vapor provides much of the energy that fuels severe weather. Thus, forecasting severe weather requires knowledge of near surface water vapor which is extremely hard to resolve from orbit.



Schreiner et al., GRL, 2020

RO is one of the few, and perhaps the only one at the moment, satellite remote sensing system that can provide this lowermost troposphere information. Doesn't profiling the atmosphere closer to the surface routinely with RO measurements require higher SNRs to track the occulted GNSS signals closer to the surface? The figure shown from Schreiner et al. (2020) certainly implies so and COSMIC-2 is achieving a significantly higher percentages of deeper profiling at low latitudes where it is hardest to do.

Importance of SNR vs tropospheric structure

Figures 3 and 8 are quite useful for understanding the relative contributions of the different sources of error at two levels in the atmosphere.

Figures 3a and 8a are shown below. The manuscript states that Figure 3a shows the RMS differences between the reference and retrieved refractivities at heights of z = 5 km (red) and 20 km (green) as function of noise level on the x-axis. “W” is the reference WOP data without noise; “WN” is the WOP data with superimposed noise; “E” is the reference ECMWF data;

Figure 8a shows RMS difference between the reference and retrieved refractivities at the same two heights as function of noise level. C is reference COSMIC data without superimposing additional noise; CN is COSMIC data with superimposed additional noise; E – reference ECMWF data; “Ndata” is the number of data for both figures.

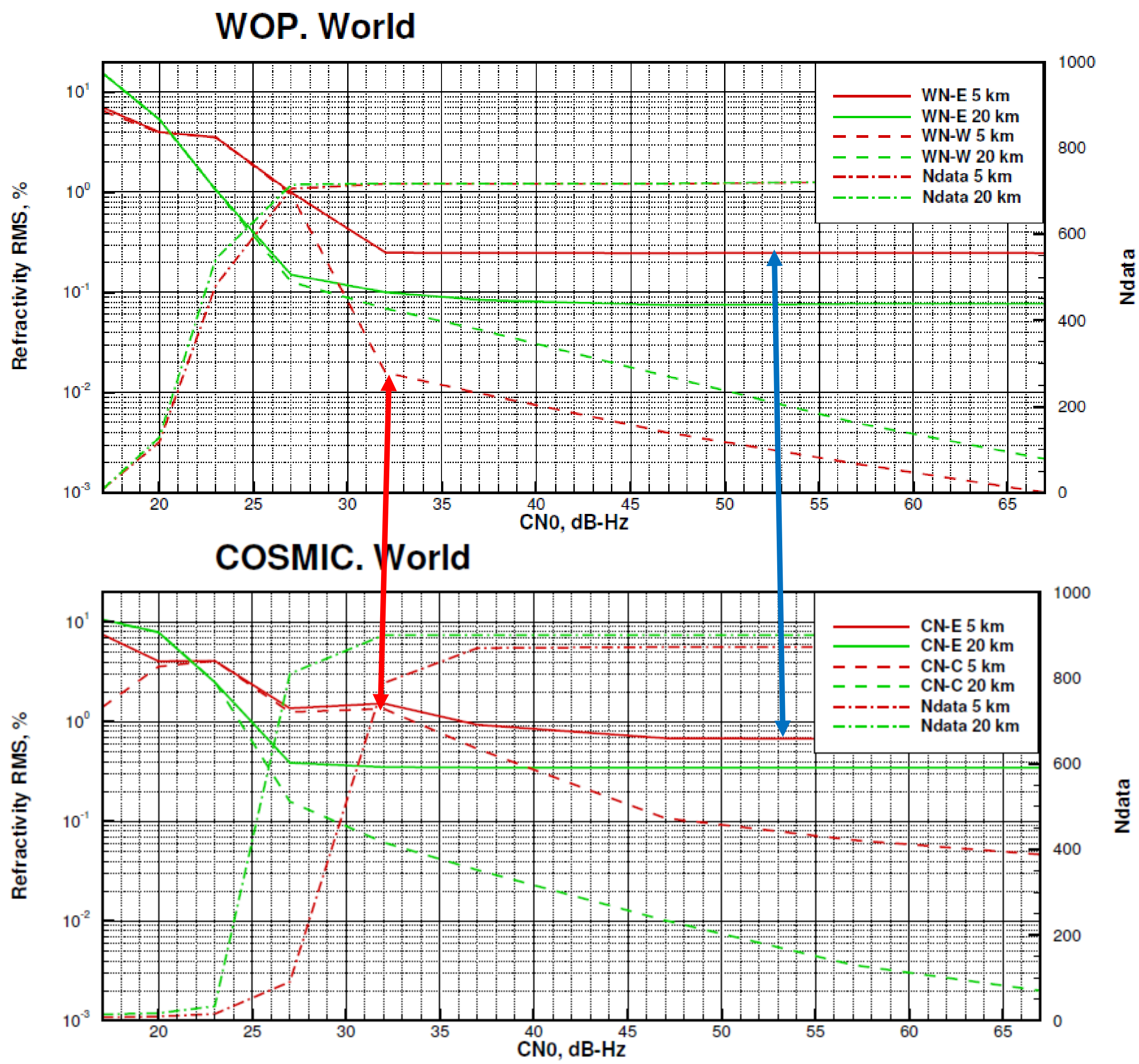
The WN-E curve at 5 km altitude shows a RMS refractivity of 2.5% for SNRs > 32 dB-Hz, while the CN-E curve at 5 km shows a RMS refractivity of 7% for SNRs > 47 dB-Hz (see the blue arrow below). I presume the difference between the RMS of those two refractivity differences reflects the fact that the COSMIC data is capturing more real world refractivity variations than is captured in the ECWMF analyses.

Now the SNR impact in these two figures (indicated by the dashed lines) is quite similar at 20 km but quite different at 5 km. For an SNR of 32 dB-Hz, the fractional refractivity error at 5 km, due to that SNR, increased from 0.016% (WN-W) to 1.6% (CN-C), a factor of 100 (see the red arrow below). For 42 dB-Hz, the fractional refractivity at 5 km increased from 0.006% to 0.25%, a factor of 40.

Is this increase due to enhanced vertical structure and defocusing captured in the COSMIC results but not present in the ECMWF analyses? Is the increase refractivity error in the COSMIC products the result of the occulted signals being weaker and therefore more difficult to track/detect?

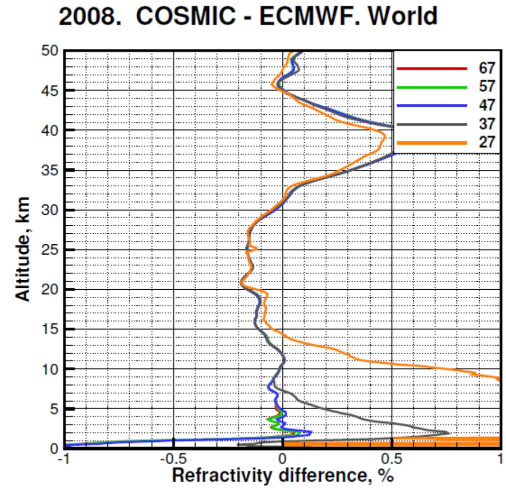
Will this enhanced impact of SNR associated with more atmospheric structure be still larger with COSMIC-2 given that its higher SNRs may resolve more structure than that resolved by COSMIC-1?

Do the results shown in Figures 3, 5, 7 and 8 depend on the vertical resolution selected in the processing?



Another question here is the COSMIC results already include the noise associated with an 800 v/v SNR which is 55 dB-Hz. Therefore, adding noise equivalent to SNRs higher than 55 dB-Hz may not have much effect. Is that effect evident here?

Lowest altitude vs SNR



The panel to the right from Figure 4 shows the bias between the COSMIC results and ECMWF in the simulated retrievals for the entire World. A negative bias is apparent in the lowermost troposphere for the 4 highest SNRs. The biases are presumably larger in the 0-30 latitude band but the orange lines associated with the 27 dB-Hz results in that Figure 4 panel make it difficult to see what is happening in the lowest 2 km.

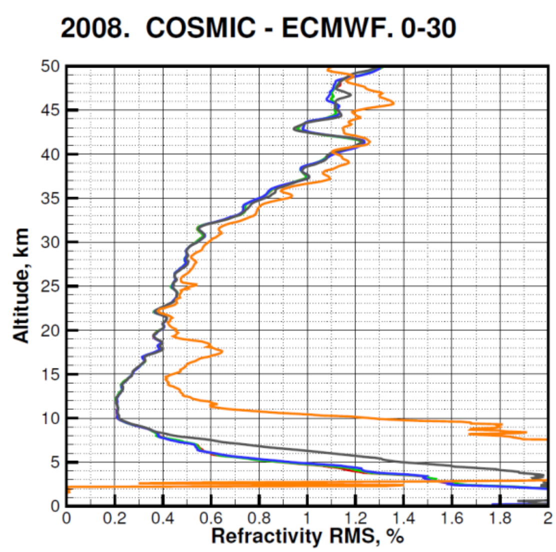
Interestingly, the results for SNRs of 47, 57 and 67 dB-Hz all fall on top of one another below 2 km indicating that the bias does not change at SNRs of 47 dB-Hz and higher. Does that mean that the depth of penetration is not changing with SNR, and that all of these retrieved profiles are reaching the surface, independent of SNR once it is at least 47 dB-Hz?

Lowest altitude of profiles versus SNR

An important figure of merit of RO profiling is the percentage of profiles that extend down to each altitude level. This is shown for instance in the Met Office figure above. The manuscript needs to include a figure showing how the percentage of profiles reaching each altitude in the lower troposphere depends on SNR.

If the lowest altitude of the profiles does depend on SNR (as one would expect), then the author needs to be very clear about what he means by the phrase, "retrieval quality", when he states, "*given RO observations of fair quality, the enhancement of the SNR cannot be expected to provide significant improvement in retrieval quality.*"

Figure showing just the lowermost troposphere results



As noted in the COSMIC-ECMWF figures, the orange curve associated with 27 dB-Hz makes it difficult to see what is happening at the bottom. This is also true in Figure 5 where the scale needs to be expanded so the reader can see the RMS uncertainties in the lowest 4 km.

The bottom 5 km or so of Figures 4 and 5 needs to be blown up to enable the reader to see what is happening in the bottom few km.

Also, why does the orange line go to zero at 2 km? Does that mean there are no profiles extending below 2 km for the 27 dB-Hz SNR?

IV. Point 4. The need for and relevance of very high SNR to routinely detect super-refraction

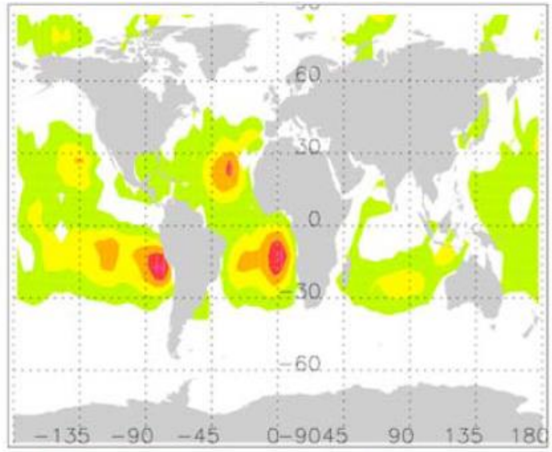
Super-refraction or ducting is important because it causes a fundamental ambiguity between bending angle and refractivity that leads to negative biases in the retrieved refractivity profiles at and below the height of super-refraction (Xie et al., 2006). It is associated with a very large negative vertical refractivity gradient typically at the top of the PBL that causes the radius of curvature of the ray path to become smaller than the radius of the Earth.

With regard to super-refraction, the manuscript states "*Interesting examples of deep occultations in the presence of pronounced humidity layers are discussed by Sokolovskiy et al. (2014). For measuring such events, it is important to have a high SNR because signals observed deep below the planet limb, correspond to large and sharp spikes in the bending angle profile and are, therefore, weak. Such events may be statistically insignificant for numerical weather prediction (NWP) purposes, still, they are interesting for the study of the planetary boundary layer (PBL) (Sokolovskiy et al., 2006).*"

In the conclusion, the manuscript makes a similar point, stating "*The influence of the noise level on the processing of deep, PBL RO signals revealing intensive humidity stratification and other features more appropriate for research and less consequential for NWP applications, requires an additional study*".

The author's description of why SNR is important for detecting super-refraction is ok. However, he does not note the fundamental ambiguity and negative bias issues associated with super-refraction.

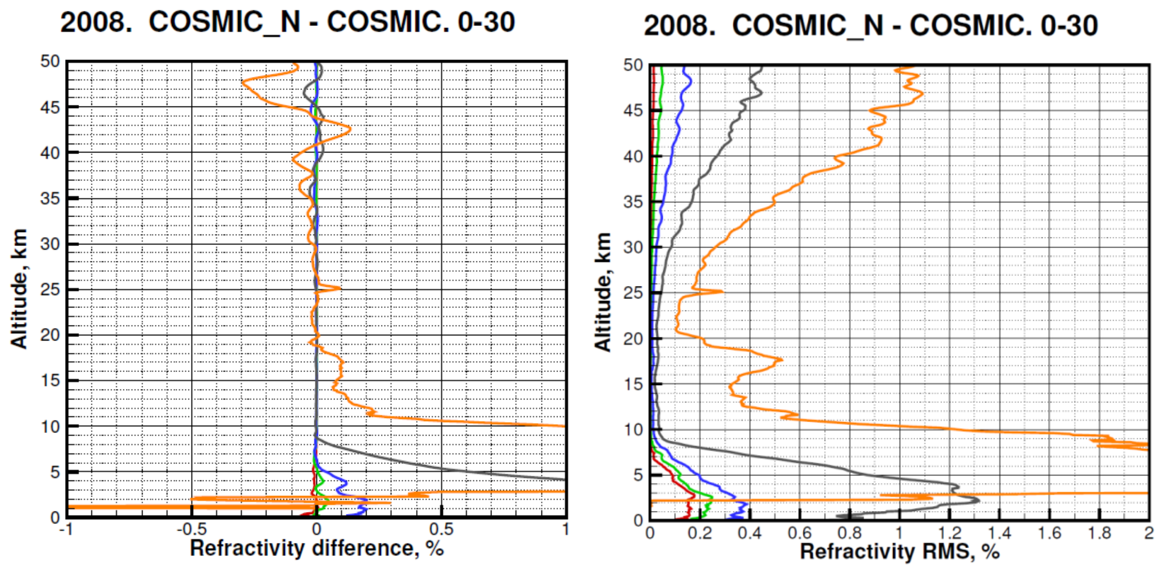
He also implies super-refraction is statistically insignificant for NWP. However, as shown the figure to the right from Xie et al. (2010), the regions where super-refraction occur span more that 50% of the globe. Super-refraction results derived from COSMIC-2 very high SNR measurements confirm this and extend the results over land where super-refraction is also routinely detected.



Monthly mean fractional refractivity difference (N-bias) between COSMIC RO and ECMWF analysis, i.e., N-bias = (N_RO − N_ECMWF)/N_ECMWF × 100, in [%]. (Xie et al., 2010)

Furthermore, the results in Figure 4 clearly show a negative bias in the lowermost troposphere (<2 km) between 60S and 60N latitude, which is 87% of the globe. This bias is likely due to super-refraction, particularly given that no such bias is evident in Figure 6 which is a comparison of COSMIC refractivity profiles with and without extra SNR noise. The COSMIC refractivity profiles used in Figures 6 and 7 presumably do not contain super-refraction because they were derived via an Abel transform which gives the minimum refractivity solution of a family of solutions in the presence of super-refraction (Xie et al. 2006) which leads to the negative refractivity bias. When the COSMIC results are used as "truth" as in Figures 6 and 7, there won't be any super-refraction related bias.

Using COSMIC 1D profiles as "truth" which also do not include any errors due horizontal structure, is quite useful because it makes the effect of the SNR more evident at high and low altitudes in Figures 6 and 7, shown below for the 0-30 degree latitude range.



Returning to NWP and super-refraction, the very high SNRs needed to detect super-refraction are important because without an ability to detect super-refraction, NWP systems do not know whether an individual refractivity profile is negatively biased because of super-refraction. As a result, NWP systems have had to be cautious when assimilating RO data in the lowermost troposphere because underestimated refractivity will cause water vapor to be underestimated which will cause severe weather to be under-forecasted. Thus, it is not only cases where super-refraction exists but also the cases where super-refraction *might* occur that must be avoided.

With these points as background, my question to the author is, how can an effect that (1) affects the ability of RO to profile the lower troposphere and PBL, (2) where most of the atmospheric water resides, (3) that occurs over more than 50% of the globe and (4) shows up in the bias results in Figure 4 be described as "*statistically insignificant for NWP*"?

I also note that the 2018 US National Academy of Sciences Decadal Survey identified the PBL as a key focus area for improving understanding and forecasting, and extending the duration of forecasting of weather and air quality, hydrology and climate. Because of its importance, the word, PBL, was mentioned 126 times in the Decadal Survey. In this context, GNSS RO was identified as being of particular interest because of its unique combination of very high vertical resolution, cloud penetration and insensitivity to the surface conditions that give it the potential to routinely profile the PBL across the globe. RO's potential is quite relevant to Tables 6.1 of Section 6: Global Hydrological Cycles and Water Resources, Table 6.3 of Section 7: Weather and Air Quality Minutes to Subseasonal and Table 9.1 of Section 9: Climate Variability and Change Seasonal to Centennial.

The manuscript's assertions about statistical insignificance seem intended to give the impression that efforts to create more advanced GNSS RO measurement capabilities developed to further extend RO profiling unique capabilities in the lowermost troposphere and associated applications were largely of esoteric science interest and not of much relevance to NWP applications and provide little advantage over the more limited capabilities associated with 3U cubesats. The Decadal Survey seems to disagree with this assertion.

Thus, with regard to the manuscript's assertions as to statistical insignificance of the ability to detect super-refraction, the author either needs to spend more effort justifying those assertions of limited relevance or remove them.

CONCLUSION

The manuscript states that "*We conclude that an RO mission exceeding the SNR thresholds noted here will provide good data, and **any improvement in the SNR cannot be expected to significantly improve the resulting statistics**.*" Also *"the enhancement of the SNR cannot be expected to provide significant improvement in retrieval quality".*

Again, a key aspect of retrieval quality is how close to the surface can the occultations profile down to, with high accuracy and without significant bias. The dependence of performance or retrieval quality on SNR includes the bias, the standard deviation of the differences and the upper and lower altitudes over which the occultation profiles can extend with high accuracy.

I agree with the statement about good data and that improvements in SNR will not significantly improve the profile performance **over the altitude range where SNR is not a limiting error source**. According to K97 and the arguments made above, the altitude range over which SNR is not a limiting error source **depends on the SNR**. The altitude dependence of the SNR impact in the K97 results does not match the results in Figures 2 or 5 or 7 and that discrepancy needs to be resolved.

The lower bound of the altitude range over which the statement above is true also depends on SNR (for example the Schreiner et al 2020 figure shown above). The manuscript should include a figure or figures that shows the dependence of the lowest altitude of the profiles on SNR. If the author believes the lowest altitude does not depend on SNR, then he must be able to explain the figure for instance from Schreiner et al (2020).

I note again that getting the lowermost troposphere measured correctly across the globe is critical to forecasting weather and air quality, hydrological applications and climate.

"*This also indicates that RO missions based on smaller satellite platforms such as CubeSats and without high gain antennas will not inherently suffer poor retrieval quality if their minimum SNR exceeds these thresholds*." This again is true over the altitude range over which SNR is not a limiting error, which depends on SNR (and perhaps vertical resolution).

Comments on wording in the Introduction

The introduction states "When proposing new RO missions, like COSMIC-2 (launched and now beginning an operational phase) (Sokolovskiy et al., 2019) or the high-gain instrument **proposed** by PlanetiQ (Kursinski, 2019), an improved SNR is treated as an **essential** advantage and is expected to result in an improved retrieval quality in the troposphere."

Regarding the high gain PlanetiQ instrument, I would replace "proposed" with "developed". The first instrument has been sitting in Cape Canaveral since mid-March awaiting launch, but delayed due to COVID.

While the author has chosen the word, "essential", I would perhaps choose a somewhat weaker adjective such as "significant". One can clearly make RO observations at lower SNRs than those being achieved by COSMIC-2 and those planned to be achieved by PlanetiQ, such as has been done on COSMIC and METOP and even GPS-MET as just mentioned above. COSMIC had significant impact on NWP leading the way to following missions like COSMIC-2 and Spire. Clearly one can live with poorer weather forecasts than those that could be achieved with better measurements and thus one can argue that the word, essential, is an overstatement. However, there are clear advantages to higher SNR particularly in the lowermost troposphere and PBL which are what the COSMIC-2 and PlanetiQ designs are focused on adding.

Also in this regard, Kursinski (2019) stated that PlanetiQ's RO system has been designed to deliver COSMIC-2 performance (>2000 v/v) which should enable routine profiling to surface for NWP & climate (hopefully). It quotes Sokolovskiy et al., 2014. It does not state these improvements are essential but does imply that they should be quite beneficial for weather and climate.

References

The author should use references to provide context for what we know, or at least think we know, about the effects of finite SNR on GNSS RO and what is new in the work presented here.

References mentioned in this review

Bowler, Neill E., "An assessment of GNSS radio occultation data produced by Spire", The Met Office, Weather science technical report 640, March 23, 2020

Hajj, George A., et al. "A technical description of atmospheric sounding by GPS occultation." *Journal of Atmospheric and Solar-Terrestrial Physics* 64.4 (2002): 451-469.

Kursinski, E. R., et al. "Observing Earth's atmosphere with radio occultation measurements using the Global Positioning System." *Journal of Geophysical Research: Atmospheres* 102.D19 (1997): 23429-23465.

Schreiner, William S., et al. "COSMIC-2 Radio Occultation Constellation: First Results." *Geophysical Research Letters* 47.4 (2020): e2019GL086841.

Xie, Feiqin, et al. "An approach for retrieving marine boundary layer refractivity from GPS occultation data in the presence of superrefraction." *Journal of Atmospheric and Oceanic Technology* 23.12 (2006): 1629-1644.

Xie, F., et al. "Super-refraction effects on GPS radio occultation refractivity in marine boundary layers." *Geophysical research letters* 37.11 (2010).

Additional minor comments

Line 84 "preformed" should be "performed".

What are ξ' and ξ'' in eqn (3)?

In the caption to Figure 7, I believe the word "Mean" should be replaced by "RMS".