Review for Pham et al., "*Estimating the refractivity bias of Formosat-7/COSMIC-II GNSS Radio Occultation in the planetary boundary layer*"

Summary

The refractivity retrieval bias in the planetary boundary layer has long been recognized and is an important issue that impede the GNSS radio occultation data assimilation in the lower troposphere. This paper developed the polynomial regression model to estimate the region-dependent biases with the assumption that the refractivity biases (refer to REFB in the paper) are related to the local spectral width (LSW) and the thermodynamic information including the temperature and specific humidity (T &q). An individual regression model with LSW and T & Q were derived. In addition, the minimum variance estimation (MVE) was applied to combine all three parameters into the regression model. The authors demonstrated that the MVE regression model reproduce the refractivity biases better than the individual models. The authors also claimed that the model can help correct the refractivity bias in PBL and thus improve the quality control of the GNSS RO and increase the values of RO observations in the lower troposphere.

Overall, this paper is generally well written, and the regression model estimation effort offer some improved understanding of the refractivity biases in the lower troposphere. However, some statement regarding the refractivity biases in the PBL were not accurate. The relatively poor performance of the LSW regression model over ducting region is not a totally surprise, as LSW might not ducting-induced biases is primarily caused by the Abel inversion (i.e., a singularity problem in retrieval algorithm). However, the physical explanation of the better relation between REFB over ducting region with the T&Q was not clear and need better explanation.

Moreover, it needs to be cautious that the 1D-Var retrieved temperature and humidity retrieval embed the uncertainty from model a-priori information. Therefore, it is susceptive that the use of such data in the MVE regression model could introduce solid positive impact to the data assimilation.

I would recommend "major revision" for the paper. The major comments along with some detailed technical comments are listed below:

Major comments:

1. The planetary boundary layer has been raised in numerous place in the paper, including the paper title. However, there are no clear definition of PBL, instead of a fixed height of 1.5 km as the indication for PBL. Note that the PBL height has various definition and can change significantly from a few hundred meters up to 3-4 km. It might be too much simplification for use one fixed height of 1.5 km to indicate the PBL.
2. The authors need to clearly stating that the Local spectral width, Temperature (T) and specific humidity (Q) used in this paper are the "average" below 1.5 km?

a. L267: Temperate and specific humidity averaged below 1.5 km for land and ocean (Figure 6)
    i. Do the authors considering the regional difference in penetration depth of RO profiles (Fig. 1)?
    ii. How to deal with the potential negative bias in T/Q resulting from the early termination of the RO profiles, i.e., missing the lowest part of profile below 1.5 km where temperature and moisture values are likely larger closer to the surface.
    iii. Do the authors process all RO data or only the ones penetrating certain threshold (1.5 km??)? Sensitivity test might be needed, for example, regenerate Fig. 5 by requiring RO profiles to penetrate below 500m above mean-sea-level. Or simply calculate the average between 500m and 1.5 km.

3. I am not totally convinced that the temperature and specific humidity patten correlated to the refractivity bias.
    a. The seemingly high correlcation between temperature/specific-humidity (T/Q) and the refractivity biases over the ducting regions is not a surprise, as those regions are mostly over the ocean upwelling region with cool sea surface temperature and relatively low moisture in PBL (below 1-2 km). However, the T/Q relationship to the REFB might not hold over high latitude.

L67-70:

- The authors give the impression that the "strong refractivity gradient" are the primary problem or RO measurement uncertainty. The authors claimed that the strong vertical refractivity gradient will introduce multipath and resulting complex bending angle and complexity of RO uncertainty. Also the L70: "strong refractivity" (I assume it should be strong refractivity gradient) causes a negative refractivity bias (N-REFB) (Rocken et al., 1997)
- However, the negative bias in lower tropposphere in earlier generation RO missions (e.g., Rocken et al., 1997) were mainly due to the close-loop tracking issue in early generation of RO receivers, along with the geometric optics retrieval algorithm which can't handle multipath well.
- The implementation of open-loop (start from SAC-C and COSMIC and thereafter) largely resolve the bias due to the tracking issue. In addition, the introduction of the holographic retrieval method (e.g., CT, FSI, PM) largely solve the multipath issue resulting from the "strong" refractivity gradient.
- Therefore, other than the "ducting", the refractivity biases can't be primarily attribute to the "strong" refractivity gradients.
- The authors need to rewrite the session to make it clear.

L71-72:

- The authors claimed that the ducting cause phase/SNR changes and thus introducing bending angle errors and additional refractivity errors. **<u>The statement is NOT correct</u>**. The primary reason of ducting inducted refractivity bias is due to the singularity problem in the Abel retrieval algorithm, which resulting in a non-unique inversion problem. Even the receiver tracking is perfect, the bending angle do not have any errors, the refractivity biases will remain in the presence of ducting. The Abel retrieval always selects the minimum refractivity solution and thus large negative refractivity biases (Sokolovskiy 2001 etc.). Of course, the sharp gradient in the present of ducting could potentially adding complexity onto RO signal phase/amplitude and might introduce extra errors in RO bending angle and refractivity retrievals but is likely secondary.

- The paragraph needs to be rewritten.
- Similarly, in L153-154: the negatively biased refractivity is due to the singularity problem in Abel inversion, and thus the non-unique inversion problem.
- Same

L126-130

- Detailed description of the ERA5 data is missing. The spatial (grid size, vertical level #), temporal resolution of the data need to be described.
- In addition, how the collocation between the RO and ERA5 need to be clearly described, including the threshold for time and space differences.

L146-147:

- I don't think the statement is correct: "*However, in the presence of a large vertical gradient, refractivity is nonspherically symmetric, and noise appears because of multiple rays (Sokolovskiy 2010).*"
    - The large vertical gradient surely can introduce multipath but doesn't introduce the nonspherically symmetric atmosphere, unless there is horizontal inhomogeneity.
    - Rewritten sentence is needed.

L155-156:

- "Under certain conditions, extreme SR occurs, and the signal is trapped … called the atmospheric duct"
    - The authors seem to be confused of the difference between Super Refraction (SR) and ducting. A good explanation can be found in Lopez (2009). However, the earlier publications make the two terms interchangeable. For example, the SR introduced in Sokolovskiy 2003, Xie et al., 2006 and many others are all referring to the "ducting" condition, i.e., $dN/dz < -157$ N-unit/km.
    - There is no such thing of the "extreme SR" becoming ducting.
    - Also, Sokolovskiy (2003) shows that the RO signal entering the ducting layer from above will not be trapped. Only when the RO signal is from below the ducting layer could be trapped.

L221-222: Figure 2b:

- Should overplot the percentage of profiles as a function of height in reference to the maximum number of profiles or the total number at 10 km.
- Although the authors claimed the number of profiles should still be sufficient for statistical evaluation, it is better to screen out the low sample regions (as indicated in Fig. 1), such as those grids with fewer than 30 profiles.

- Moreover, more stringent criteria should be explored, such as requiring the total number of profiles penetrating below 500 m above MSL within a grid to be more than at least 30 or more.

L315-318:

- It is not surprised to see weak relationship between ducting induced N-biases to the LSW, as the ducting induced N-bias is mainly caused by the Abel retrieval, but not too much due to the signal tracking or multipath (after implementation of open-loop tracking and radio-holographic retrieval method). The low temperature and low humidity (below 1.5 km) seem to be a good indicator for large N-bias over ducting region. However, such relationship could be serious fault over high latitude (not included in this study), for example, the polar region mostly with low temperature and moisture, but will not expect to see large N-biases but the opposite.
- The authors need to discuss the serious limitation of using T/Q for N-biases estimation away from subtropical oceans.
  - L331-332: "*This finding also confirms that the N-REFBs below 1.5 km are highly related to the thermodynamic conditions and that the TQ estimation successfully reflects the impact of the air-sea interaction on the RO refractivity.*"
  - I don't think the "impact of air-sea interaction" has anything to do with the N-bias assessment here.
  - Also, such assessment of T/Q relation to N-bias WON'T be able to apply to higher latitude, and maybe even over land.
  - The limitation of use T/Q for global application need to be discussed.

L368:

- Figure 11 shows two selected regions (A & B). Why Area B shows the improvement in MVE than T/Q, which seems not consistent with Figure 10f, where large RMSE remained in Area B for MVE. Please explain.
- L620-625: Figure 11 and 12 can be combined into a three-panel plot for better presentation

Technical comments:

T & Q in the paper should be ***tilted*** as they are variables

L26: Change sentence:

- "… receive radio signals, which are emitted from GNSS transmitters and tend to bend" → "receive radio signals from GNSS transmitter, which tend to bend"

L30: "reflectivity" → refractivity; "reflect the changes → measure the change (to avoid the confusion

L52: No need to add "atmospheric boundary layer (ABL)", should simply use PBL

L55: Should clarify "the penetration rate of RO profile is limited to extremely moist conditions …". Is the RO sounding penetrating deeper in more moist condition?

L62-67: "strong refractivity gradient" has been used many times in this paragraph, But what does the "strong" means? The authors state that under "strong refractivity gradients, "In such conditions, the assumptions and approximation in the retrieval algorithm can result in large uncertainties in the RO data." The refractivity gradient itself do not introduce the uncertainty. What assumptions and approximation are you referring to?

L89: such as that → such that

L90: Please clarify what the "reflected bending" means, does that mean "grazing" signal bending measurement?

L124: below the 1.5 km-height of sea level… → below the 1.5 km above the mean sea level…

L30: Parathesis needed for the difference (REF_FS7 - REF_EC)

L181-182:

- Please explain why and how to "independent fitting is performed five times, by replacing the testing data with another 20% of the data". So does the 20% testing data used in deriving the regression model? Will that defeat the purpose of using the "testing data" to verify the regression model?

L184: 5° x 3° → 5°longitude x 3° latitude?

L195: product → processing

L199: Equation (7): any explanation why only the normalized Q (specific humidity) was used in the higher order term, but not the temperature (T)? Should there be another z_i^2 term representing the T?

L209-213: Equation (10):

- What is the meaning of the vector of ones "1". Why the
- What is u_j? explanation needed.

L224: Figure 3

- L588: "during the study period" should simply state the study period, e.g., from *** to **

L234: "The increased LSW just above the boundary layers could be caused by common inversion layers in the troposphere of some oceans."

- As discussed in the major comment earlier, there is not any direct definition and calculation of PBL height is this study. In addition, regions especially over tropics might

not have clearly defined PBL due to convection. Would be more appropriate to simply using the height (e.g., 2 -3 km).
- Lager LSW due to “inversion layers”, is this a pure guess or with certain supportive evidence? If so, the reference needs to be listed.

L244: “The LSW over ocean below 4 km increases faster over the ocean, and the second peak value at the PBL top is much larger”
- Again, the “PBL top” should be replaced with 2 km.

L254: “thermodynamic structure in the PBL” → “thermodynamic structure in the lower troposphere.

L267: “temperature and specific humidity averaged below 1.5 km”
- Discussion on the sensitive of the result due to the penetration issue should be added.

L270: “Instead, N-REFB appears at the PBL top”
- Again, please not use PBL top, but using the height, e.g., ~ 2km

L286: Figure 7
- It is not clear why ONLY the relationship between REFB and LSW over the southern hemisphere (austral summer) were discussed.
- The relationship over norther hemisphere should also be discussed. Same for Figure. 8
- L604: Can’t see the orange open circle (Land data over 1.5 km). could consider using a slightly larger open circle to avoid overlapping by the solid circles.
    - Figure caption: “N-biases” should be replaced with REFB to be consistent with manuscript.
- What about the relationship in the northern hemisphere?
    - With the weaker N-bias during the evaluation period, will the relationship hold up? If not, please explain.
    - Note there are seasonal variations in N-bias, the northern hemisphere is expected to see higher N-biases in NH summer (JJA) season, will the authors expect to see clearer relationship in the NH as compare to the Fig. 7?

L293: N-biases → REFB

L294: Figure 8
- L297: The manuscript only discussed Fig. 8a, but not the Fig. 8b. Discussion of both are required, and the difference should be clearly stated.
- L609: No labels: (a) (b) in Fig. 8. Also Figure caption should be self-explanatory. The manuscript also did not clearly tell the difference. Caption should clearly indicate the difference between the two panels, one for ocean and the other over land, right?
- The relationship is hard to be seen clearly.
- Similar to Fig. 7, the relationship over norther hemisphere should also be presented and discussed.

L318: rewrite “inversion layers on top of the surface cold atmosphere”? Do you mean on top of cool sea surface?

L351: Figure 10

- “Almost all the large RMSEs in the LSW or TQ estimation are removed by the MVE method (Fig. 10c and 10f)”
    - It is hard to see the significant improvement from MVE results (10 e,f) as compared to TQ-based approach (10 c,d). Actually, a slight degradation can be seen.
    - Need to explain why the training data set show the large improvement of T/Q over ducting region is SE Pacific/Atlantic in Fig. 10-c,e (e.g., SE Pacific [30S,90W]), but was not the case for the testing data set (Fig. 10-d,f)
    - L355:

L366: EQ < Lat < 10°N → 0° < Lat <10°N

L383: 3 N → 3 N-unit

L386: Please remove the PBL in “positive bias above the PBL altitudes of 3 to 5 km.”

L423: “… temperature and moisture from the ERA5 reanalysis may have their own bias

L612: Box A & B should include the [lat, lon] information.