Manuscript Number : amt–2017–250-RC2
Associate Editor : Dr. Jens Wickert
Manuscript Title : Comparisons of the tropospheric specific humidity from GPS radio occultations with ERA-Interim, NASA MERRA and AIRS data

Dear Referee #2,

We would like to thank you for taking the time to review our manuscript. Your kind words about our work are greatly appreciated, and your comments have now been addressed and implemented in the revised manuscript. We have performed major revisions to accommodate your Comment #13, and we include the results in the revised version.

Minor Comment #1: P2, L38: '... together with the retrieval uncertainty of the SH products from all data sets, we conclude that RO observations are a valuable independent observing system.' What do you mean by 'independent'? RO SH is not independent from weather model data. JPL-RO SH makes use of the temperature from ECMWF. UCAR-RO SH is obtained by variational data assimilation utilizing ECMWF as the background. I suggest to remove the word 'independent'. Also, ECMWF depends on RO, because UCAR-RO bending angles were assimilated.

Answer: Done. We removed the word "independent".

Minor Comment #2: P3, L48: '...Hence, we ought to quantify and understand the degree of agreement of water vapor concentration throughout the vertical extent of the troposphere among different sensors, in order to improve the representation of the Earth's atmospheric humidity content that is key to predicting future climate [Hegerl et all., 2015].' In the present study you consider the altitude range 700-400 hPa (~2–8 km). The troposphere extends from ~0–15 km. In fact, most of the water vapor is contained in the lowest 2 km. In the present study you do not try to quantify and understand the degree of agreement of the water vapor concentration throughout the vertical extent of the troposphere. I suggest to remove the word 'throughout'.

Answer: Done. We removed the word "throughout". **Please, see strikethrough in line 49.**

Minor Comment #3: P4, L83: '...and full diurnal cycle sampling." This is approximately true for COSMIC but not true in general. This depends on the LEO orbits.

Answer: Done. We added the reviewer's comment in the revised manuscript. **Please, see lines 82–83.**

Minor Comment #4: P5, L102: '...Of importance is the fact that we use MERRA, instead of MERRA-2, because MERRA does not assimilate (unlike ERA-Interim), providing an independent data set when comparing the RO SH observations.' This sounds interesting. Does this mean that you expect big differences when you use NERRA-2 instead of MERRA? Would is be a lot of effort for you to add MERRA-2 as well? I recommend to do so. This would be very interesting, because it would show the impact of RO on weather model SH.

Answer: We believe that adding the MERRA-2 SH climatology in our analysis will not show the impact of RO on weather model SH. This is because there have been significant changes on how MERRA-2 handles the Earth's water cycle with respect to MERRA, and these changes have a much more direct contribution to differences in MERRA-2 SH climatology than the addition of RO bending angles. Specifically, *Bosilovich et al.* [2017] state: "*Some of the changes in MERRA-2 have direct effect on the water cycle.*" For detailed explanation of these changes please refer to *Galero et al.* [2016] and *Takacs et al.* [2016]. Thus, we believe that comparisons with MERRA are more informative than comparisons with MERRA-2 for the objectives of our investigations, unless the contributions of all improvements in MERRA-2 are first isolated from the contributions of RO. However, we acknowledge the fact that comparing MERRA-2 and RO could be an interesting task. **We added relevant text to discuss this. Please, see lines 175–180.**

Minor Comment #5: P6, L114: '...We study the tropics and subtropics (±40^{o}, three distinct latitudinal regions) from 700 hPa up to 400 hPa, because this region is key to climate research [IPCC, 2007], but models and observations have large SH differences in the middle and upper troposphere [e.g., Jiang et al., 2012; Tian et al., 2013; Wang and Su, 2013], and we select this pressure range because the RO SH retrievals are most robust.' I can imagine what you mean by 'most robust' but some other interested readers do not know what this means. Please, explain in brief what you mean by 'most robust'. E.g. signal tracking in the lower troposphere is somewhat problematic, the assumption of a spherically layered atmosphere, critical refraction (Ao et al., 2003) etc.

Answer: We included relevant text and removed "most robust" to avoid confusion. **Please, see lines 121–127.**

Minor Comment #6: P7, L144: '...air temperature'. I suggest to remove the word 'air'.

Answer: Done. Please, see strikethrough word in line 153.

Minor Comment #7: P7, L145: Please add (for completeness) the equation that you use to convert water vapor pressure to SH.

Answer: Done. Please, see lines 158–163.

Minor Comment #8: P7, L154: '...air refractivity'. I suggest to remove the word "air" here and in the following.

Answer: Done. Please, see strikethrough line 168.

Minor Comment #9: P9, L188: '...The AIRS physical retrievals use an IR-microwave neural net solution [Blackwell et al., 2008] as the first guess for temperature and water vapor profiles based on MIT's stochastic cloud-clearing and neural network solution described in Khan et al. [2014].' I have very little idea of AIRS retrievals. In short, does the AIRS retrieval at any point make use of data from a climatology or a weather model?

Answer: The short answer is no. The first guess comes from a neural network, which is trained on 60 days of ECMWF during the first year or two of AIRS operations [personal communication with Eric Fetzer]. It does not retrieve water profiles whenever cloud fraction exceeds the 80%, and recently they developed a cloud-clearing algorithm which compares the irradiance of neighboring pixels to infer the water vapor content during clouds.

Minor Comment #10: P9, L192: The section 'Data Sources' can be moved to the Acknowledgments.

Answer: Done. Please, see Acknowledgments.

Minor Comment #11: P10, L207: ' ...GPS-RO air refractivity accuracy of <1.0% at 2.0 km altitude [Schreiner et al., 2007] reduces to ~0.2% above 5.0 km [Kuo et al., 2005].' Schreiner et al., 2007 provides an estimate for the precision and not the accuracy. They measure the degree of the reproducibility of the GPS RO technique. Kuo et al., 2005 provide an estimate for the accuracy. As you focus on the altitude range 2 – 8 km, I suggest to simply write: 'GPS-RO refractivity accuracy is about 1% at an altitude of 2 km and decreasing to about 0.2% at an altitude of 8 km [Kuo et al., 2005].'

Answer: Done. Please, see lines 230–231.

Minor Comment #12: P10, L223: I suggest to remove '...We do not extend our analysis at higher altitudes due to small contribution of water vapor on to the RO observations.' As you already mention in the 'Methodology' section that your focus is 700-400 hPa.

Answer: Done. The sentence has been removed.

Minor Comment #13: P11, L226: '...and the differences between the JPL and the UCAR time series serve as a guidline of an estimate of the SH structural uncertainty.' One of the most interesting points in your study are the differences between JPL SH and UCAR SH. Where do the differences come from? Are those differences due to differences in the raw (=non-optimized) bending angles, the refractivity, or they mainly caused by the difference SH retrieval method? I strongly recommend to add (in an Appendix) a one-to-one comparison (mean and one-sigma) for bending angle and refractivity profiles for the altitude range 0-8 km.

Answer: Done. This is similar to General Comment #3 of Reviewer #1. **See new added Section 3.4.**

The differences in the specific humidity retrievals result from a combination of different things. We have analyzed the refractivity climatologies from both JPL and UCAR at 700 hPa, 600 hPa, 500 hPa, and 400 hPa pressure levels, and have included these results in the main manuscript. We also translate the refractivity differences into specific humidity differences and discuss the discrepancies between JPL and UCAR within these differences. We show these results for the deep tropics. The analysis is exactly the same for the trade winds zones and the subtropics and therefore we have not repeated it.

Minor Comment #14: P12, L240: '...SH time series over the entire observational record for all data sets throughout the vertical extent of the troposphere'. Remove the word 'throughout'.

Answer: Done. Please, see strikethrough in line 343.

Minor Comment #15: P18, L332: '...Overall, this suggests that over less convective regions different data sets tend to agree better, signifying that convection is a limiting factor in properly sensing the amount of water vapor in the atmosphere.' Weather models are known to be less accurate in regions with convection. Do you mean that RO SH is less accurate there as well? For example there is one study by S. Yang and Zou, 2017 showing (positive) RO biases in cloudy conditions.

Answer: Done. Please, see lines 526–528.

Minor Comment #16: P26, L421: Remove 'in the forward operator'.

Answer: Done. Also removed in other places throughout the manuscript.

Comment #17: P28, L467: I suggest to remove the word 'independent'. RO (non-optimized) bending angles are independent, however RO SH is not independent.

Answer: Done. We replaced the word 'independent' with the word 'additional'. **Please, see line 530.**

Panagiotis Vergados

THIS IS THE END OF REVIEWER #2 REPORT .....