

## ***Interactive comment on “Comparisons of the tropospheric specific humidity from GPS radio occultations with ERA–Interim, NASA MERRA and AIRS data” by Panagiotis Vergados et al.***

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Overall this is an interesting and generally well-written paper that should be published in AMT. There have been relatively few papers published comparing water vapor retrievals from radio occultation (RO) observations to other independent observations and with each other (computed using different methods). Thus this is an important contribution.

However, the paper is long and it is a little difficult and tiresome to read because there are three regions ( $\pm 15$  degrees NS, 15–30 NS and 30–40 N/S and these are discussed in great detail with two figures and one table for each region. All of this takes about 16 pages and the reader may get lost. Perhaps the number of regions could be reduced to

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two (0–30 NS and 30–40 N/S? It is not clear to me that the differences between  $\pm 15$  NS and 15–30 NS are important. I become lost in the details of all these comparisons.

Most importantly (and the reason for suggesting a major revision before publication), because a major point of the paper is a comparison of the JPL and UCAR retrievals of specific humidity  $q$  (and these are quite different) it is worth mentioning in the abstract the significant difference between the JPL and UCAR estimation of  $q$  given refractivity  $N$ . JPL uses a “simple” method (using  $T$  from the ECMWF TOGA database in Eq. 1) while UCAR uses a 1DVAR method (using ERA–Interim for the a priori). This difference between these two methods is likely the main reason for the different results, and not a property of RO in general. A reader could conclude from these large differences in specific humidity that RO is not a very good climate observational technique, unlike the results from many other studies. However, this reason should be verified by also comparing the JPL and UCAR refractivities that were used in computing the  $q$ . Or use the same method for computing  $q$  given  $N$  for UCAR as for JPL (i.e. use Eq. 1 for computing the UCAR estimate of  $q$ . This would narrow the differences to differences in refractivity.

Finally, it would be helpful if the authors could say something about what all these differences mean in terms of accuracy of water vapor compared to the estimates of accuracy in  $q$  from other papers. Perhaps this discussion could go in the conclusions.

Specific comments:

1. SH is not a common abbreviation for specific humidity. I suggest using the more common letter “ $q$ .” 2. Line 32–something missing here? “as well as” perhaps? 3. Page 10 lines 206–215. The quoted RO accuracies of 10–20% below 7 km and 0.1 g/kg seem inconsistent. For a typical lower tropospheric  $q$  of 5–10 g/kg, an error of 0.1 g/kg (1–2%) is far better than 10% (1–2%). The JPL quoted accuracies of 0.2–0.4 g/kg in the tropics (2–4% for a typical value of  $q$  of 10 g/kg) are also very high compared to the quoted values of 20% for MERRA and 25% for AIRS. Can the authors comment on these large

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differences? In general, it is very important for this paper to precisely define previous studies of the accuracy of water vapor (specific humidity) estimates from RO. 4. It would be helpful to know why the author's study extends downward only to 700 hPa? Most of the atmospheric water vapor is below 700 hPa. Yes, there is the negative N bias associated with super-refraction and other issues in the lower troposphere, but still it is important to characterize the errors in retrieved  $q$  in this region. 5. The Vergados et al. 2016 paper is in the list of References, but I couldn't find it mentioned in the paper. 6. Lines 285-287 It says that the wet bias in JPL-RO may be due to the warm bias in the ERA-Interim (Eq. 1). But they use ECMWF TOGA analysis for the  $T$  in Eq. 1, not the ERA-Interim (lines 150-151). Please clarify. Similarly, lines 420-422 say the JPL retrieval technique uses "ECMWF" as a-priori temperature information. What ECMWF, TOGA or Interim? 7. Figure 3 is not referred to in text. It looks like it should be in line 291, i.e. "...we estimate the respective SH anomalies (Figure 3)." 8. Lines 372-373-I suggest rewording to "...defines the subtropics where dry air descends from the Hadley cell." 9. Lines 474-475-reword to say "moistest of all data sets" and "driest of all data sets." 10. Lines 490-492-All the pressure levels lie above the PBL not just the 700 hPa level. Do the authors mean that the 700 hPa level is the closest to the PBL?

End of comments

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