Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-66-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessment of GNSS radio occultation refractivity under heavy precipitation" *by* Ramon Padullés et al.

Anonymous Referee #3

Received and published: 18 June 2018

This manuscript investigated the underlying physical cause of a systematic high-bias in retrieved GNSS radio occultation refractivity in the middle troposphere under heavy precipitating situations. Previously people had found similar bias signals but attributed the bias to the scattering caused by raindrop/frozen hydrometers. In this study, the authors interpolated the collocated 3D TRMM-precipitation radar retrieved rain rate to the GPS RO plane and found the difference between simulated SNR and observed ones are not systematically biased. Rather, the authors found some positive relationships between the percentage bias (%) and the collocated specific humidity from three different re-analysis/analyses products. Therefore, the conclusion they made from this work is that the systematic high-bias under heavy precipitation scenes are caused by the corresponded increase in specific humidity.

This idea is novel. The comparisons to multiple observations and reanalysis/analyses products make the conclusion rather solid. The writing is clear and concise. I think this manuscript deserves the final publication in ACP. However, there are some logic caveats I'd like to point out that are either not considered clearly enough when carrying out the data analysis, or not described clear enough to make the readers not confused. There are a few minor glitches that may improve after the revision.

First, with respect to the broad picture of the logic flow: (1) the heavy precipitation scenes are defined by TRMM-PR or IMERG "observations" (by saying observations, I mean retrieved products), but not the precipitation scenes in the reanalysis/analyses products. However, the collocated specific humidity profiles are identified from the reanalysis/analyses. Therefore, when you separate the specific humidity value according to no-rain, light-rain and heavy-rain, it's not necessarily the specific humidity environment for no-rain/light-rain/heavy-rain in the reanalysis/analyses products. I believe this effect is minor as the water vapor field is rather smooth and not as intermittent as the cloud water content field. But I think you need to be clear about this logic difference in the manuscript.

(2) secondly, regarding the collocation and co-incident measurements between GPS-RO and TRMM-PR and IMERG: as precipitation is so transient, especially for heavy precipitation, +/- 15 minutes criterion for co-incident measurements might be too loose. The geo-collocation criterion for TRMM-PR and IMERG was not specified clearly in the context: Do you consider the footprint effect? How do you align the GPS-RO limb-sounding with TRMM-PR type of nadir-viewing instrument?

(3) the simulation of heavy precipitation scenes using the assumed raindrop size and size distribution is more or less questionable to me. As above 5 km melting layer, most of the precipitation-sized particles are frozen hydrometers indeed, and using raindrop assumption throughout the column is not a very good assumption. But once you calculate scattering frozen hydrometers, more free parameters like ice habit, density, etc., will be involved and the uncertainties are huge. I think it is NOT a good idea to COM-

PLETELY exclude the precipitation-sized particle scattering effect out, first because of the aforementioned bullet#1, and also because simulation of scattering effect for any hydrometers, especially when frozen hydrometers are involved, is far from perfect. Actually, since the largest discrepancy occurs at $\sim$ 5 km in Fig. 1 for all three re-analyses/analyses datasets, I suspect strongly that the extremely complicated situation in the melting layer (at $\sim$ 5km in the tropics) would at least have certain effect on that.

(4) Since the authors didn't discuss throughout the manuscript that why they reach different conclusions with previous literatures (e.g., Lin et al., 2010; Yang and Zou, 2012; etc. as mentioned in the manuscript), I'm not sure whether it's because the analysis methodology is different? Data sources are different? Assumptions are different? It worths a paragraph or at least a couple of sentences to discuss the differences in your and previous efforts that eventually lead to the discrepancy in conclusions.

(5) As I mentioned in Comment#3 above, this paper didn't explain the vertical structure of the high-bias shown in Fig. 1. Rather, the last two figures (Fig.5 and 6) and related discussions focus on a single altitude (6 km or 6.5 km) and the reason why this altitude is selected was not specified clearly in the text.

Minor points: Page 8, equation (7): please be consistent with dphi or delta_phi. Figure 3 and Figure 6: Since the collocated sample for heavy precipitation scenes is small, it's important to have the statistical significance level shown on the map. Please consider only color statistically significant grids, or overplot the contoured significance level.

Figure 2: Do you have any speculation why ERA-rain looks worse than ECH-rain? Also, please include the standard deviation envelope for each rain curve.

Figure 4: Can you show a case with negative delay of SNR, together with corresponding TRMM-PR rainrate vertical profile projected on the RO plane for the two cases? I don't get in what situation that SNR delay could be reversed.

Regarding Fig. 5 and Fig. 3: if your hypothesis is correct, you should see smaller specific humidity for light-precipitation scenes (Fig. 3, middle column). Is that the case from the collocated re-analysis/analysis?

Page 11, Line 22: I don't understand this statement. Would you please elaborate?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-66, 2018.