Review of "Comparison of total water vapour content in the Arctic derived from GNSS, AIRS, MODIS and SCHIAMACHY" by Alraddawi et al.

General comments

To my opinion, the authors did not respond satisfactory enough to the comments or questions raised by the two reviewers. Therefore, an additional revision of the manuscript is needed by the authors. In particular:

- Q2 of the first reviewer ("Section 4 describes an impact of clouds on satellite TCWV measurements as a source of uncertainties. Is it the only or main factor creating the biases or does there exist other factors like latitudinal dependence? Is it possible to quantify all the disturbing factors"). I agree with this reviewer that the authors focus almost exclusively on the impact of the clouds on the satellite TCWV measurements. This of course belongs to the scientific freedom of the authors, but (i) the authors should argue then more (in the manuscript) why only this effect is investigated, (ii) the authors should provide a "theoretical" explanation of the effect that cloud cover should have on the satellite measurements, based on their respective retrieval method (=suggestion 7 of second reviewer, which was not taken care of **in the manuscript**), and (iii) they should also first describe the other known impacting factors on the satellite TCWV measurements (like IWV dependence, SZA dependence, seasonal dependence: see the reference Vaquero-Martínez, J., Remote Sensing of Environment (2017), http://dx.doi.org/10.1016/j.rse.2017.09.028, already given by the second reviewer). In this context, I also want to raise the question if the differences you find between Thule and Ny Alesund at one hand and Sodankyla on the other hand cannot be partially ascribed to the differences in latitude (10°) and consequently TCWV amount (this is related to your far too vague statement on page 9, lines 21-22: "So there must be a significantly different sensitivity on the measurements to the atmospheric properties over Sodankyla").
- If you decide to focus only on one aspect (cloud impact) to explain the TCWV GNSS-satellite biases, you should be very convincing. For me, at the moment, it is not. The interpretation relies to a large extent on the correlations calculated between 2 time series of maximum 15 points (see e.g. the graphs in Fig. 7-8). I would therefore ask the authors to do the same analysis with another dataset of cloud cover (e.g. MODIS), as suggested by the second reviewer (suggestion 4). This will make your analysis much more consistent and the interpretation (hopefully) much more convincing. Also, please add the 1-sigma or 2-sigma bars to the annual cycle of the AIRS cloud fraction in Fig. 6, this will give the reader a better idea if the annual cycle is significant or not. By the way, adding such 1-sigma or 2-sigma bars to figures 7 and 8 could also shed another light on the interpretation of those figures. You might also want to add a figure, showing a scatter plot between TCWV GNSS-satellite biases and cloud cover (for the monthly values for example), to illustrate visually the retrieved correlation (coefficient).

- I think you could also draw some firmer conclusions, especially about the TCWV uncertainties achieved for the satellite retrievals (see question 4 of the first reviewer). Please give the numbers of the biases for all the treated satellites (not only for AIRS) , which might be compared here with previous intercomparison studies at high-latitude sites.

Specific comments

- Page 2, line 35: please rewrite as “despite the presence of a possible bias in certain specific configuration” and add a reference for this statement here.
- Page 2, line 40: replace with “and found GPS to under-estimate both satellite sensors”.
- Page 3, lines 6-7: replace with “uncertainties, accuracies, and limitations of several global sensors/techniques available, which could help imorving the data analysis methods (Bock, 2012; Guerova et al., 2005, 2016).”
- Page 3, line 8: explain the acronym GLONASS
- Page 3, line 15: replace with “The global validation efforts of the used satellite products have pointed to many factors causing satellite biases in TCWV”.
- Page 3, lines 17-19: replace with “While SCIAMACHY measurements are independent of the initial humidity profile, they are affected by other factors like the albedo estimation for different surfaces (Noël, 2007). MODIS measurements are known to be affected under hazy conditions…
- Page 4, line 10. Explain that what the weights are in T_m (this is the humidity!).
- Page 6, lines 15-18: replace with “Note that SCIAMACHY data solar dependency results in missing data for winter months. Our study takes place from 2003 to 2011 over Sodankyla and Ny-Alesund and from 2004 to 2011 for Thule.”
- Page 7, lines 13-14: please specify what you mean by “longest overpasses”. Does it mean that at Thule for instance, AIRS is measuring TCWV continuously between 06-19 UT (see Table 1) (I’m playing the devil’s advocate here)?
- Page 7, lines 14-16: replace with “AIRS effective cloud fraction used here is computed as the ratio of the number of AIRS cloudy measurements (CF>0.01) to the total number of AIRS measurements per 1° by 1°.
- Page 8, line 26-27: although I do not doubt your data quality of GNSS and SCIAMACHY, another probably reason for the smaller biases in your study compared to ours (I think you should also only refer to the AMT paper, and not the AMTD paper), is the fact that your study deals only with high-latitude sites, hence sites with lower IWV contents, and hence lower IWV biases between different techniques (as outlined in our AMT paper). To be completely fair, you should compare the values site by site (Ny Alesund, Thule, Sodankyla).
- Page 11, line 26 & 29: replace “don’t” by “do not”
- Page 11, line 29: replace impact by impacted
- Page 11, line 31: replace increase by increased
- Page 11, line 35: replace with “Summer SCIAMACHY-GNSS TCWV biases are found to be correlated with could cover at the ….”