Response to Review report for “OMI Total Column Water Vapour Version 4: Validation and Applications”

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General comments

In this manuscript, the version 4 TCWV retrieval from OMI is validated against ground-based GPS TCWV retrievals over land and SSMIS satellite microwave retrievals over land. Differences of the version 4 retrieval with previous versions have been described, although a detailed analysis of the improvement with respect to the previous version is still lacking. I will point out some specific examples where such an additional comparison might be included in the manuscript. Also the interpretation of some of the findings for the OMI TCWV differences with TCWV from GPS or SSMIS is lacking, see again below in my specific comments. Thereafter, 3 well-chosen examples show the importance of having a global TCWV dataset, here from OMI. These are nice demonstrations of the TCWV product, but the authors might argue more what the added value of in particular OMI TCWV (and version 4) is for those applications, compared to other satellite retrievals or reanalyzes.

Thank you very much for the thorough and constructive review. We have improved the manuscript following your suggestions. The example applications are intended to test and show that there is value in the OMI TCWV dataset, and therefore, the data can contribute to the overall understanding of water vapor. Comparisons with other satellite datasets or reanalyzes for the added value of OMI TCWV are left for future work.

Please find our detailed response below.

Specific comments

- Page 1, line 10: I would write out “OMI” already in the abstract, as well as WRF (on line 28).

We have now written them out.

- Page 2-3, lines 58 –60: to me, it is strange to already mention a result of the analysis in the introduction of the manuscript. I would drop this sentence.

The sentence has been deleted.

- Page 3, lines 72-73: here again, you already mention a result of this study in the introduction. Reformulate please.

The sentence has been deleted.

- Page 3, line 80: data filtering criteria are recommended

“is” has been changed to “are”.

- Page 4, lines 96-100: rather strange formulation. I would start the sentence with “In the non-linear least square fitting, we consider…” And also, please reformulate “In addition to water vapour” to a more specific formulation as e.g. “the use of spectroscopic water vapour dataset”.

The sentence has been rephrased following the suggestion.

- Page 4, lines 100-108: to a reader that is not entirely in the satellite data retrieval field, it might seem ought that you start the discussion here with what version 4 is not using (common mode) in the fitting. Perhaps describe first how the fitting is done with version 4 and then describe the disadvantages of the common mode.

The elements considered in the Version 4.0 nonlinear least square fitting are explained in the previous sentence. The intention of this sentence is to point out the difference with previous versions. For readers who are unfamiliar with common mode, we have added the reference González Abad et al. (2015).

□ Page 4, lines 109-110: as it turned out that the choice of the water vapour reference spectrum really matters for the comparison between the version 3 and 4 TCWV retrievals (later in the manuscript), you might comment on why you use an "older" water vapour reference spectrum in version 4 than in version 3.

We have added a couple of sentences to explain the rationale. It is primarily driven by the validation results. In addition, through personal communication with the HITRAN group at the Smithsonian Astrophysical Observatory, we have recently learned that HITRAN 2016 has some issues with water vapor in the blue wavelength range and that spectroscopic improvements are being made for the next HITRAN release.

□ Page 6, lines 134-139: is the compromise for the wavelength interval as retrieval window for version 4, chosen for a particular orbit number and geographical area, also tested/valid for other orbits and other areas? Please comment.

We have changed "we use OMI Orbit number 10426 … as an example to …" to "we randomly selected OMI orbit number 10426 to…". We tested the result with Orbit 10423 (which cut across the Pacific near the dateline). The patterns exhibited by the variables are similar, though the values for SCD and SCD uncertainties are slightly higher, as Orbit 10423 is over the ocean.

□ Page 6, lines 140-145 and Fig 2.: I really do not understand what is represented in Fig 2. Is this the overall median SCD of the entire dataset or also for the same orbit and geographical area as in Fig. 1? Please specify.

Following the other reviewer's suggestion, we have combined the original Figure 1 and Figure 2 into one figure. In the figure caption, we have added "for OMI Orbit number 10426".

□ Page 8, lines 184-185: from which dataset do you obtain the "mean elevation within the corresponding 0.25°×0.25° grid square"?

The dataset was downloaded from www.temis.nl/data/topo/dem2grid.html in December 2015. The ultimate data source is USGS. A comment about this has been added.

□ Page 9, lines 203-204: "because the fitting includes many other interference molecules whose reference spectra may also contain errors within the retrieval window" □ are version 3 and version 4 not using the same reference spectra for those molecules? So the errors in those reference spectra should then give the same effect in both version 3 and 4, no?

This sentence has been deleted. Version 3 does not include the Vibrational Raman Scattering of air, but Version 4 does. We have recently found, through personal communication with the HITRAN group, that the HITRAN 2016 water vapor spectrum in the blue wavelength range is adversely affected by a line broadening issue. It is therefore not surprising that HITRAN 2008 can lead to lower bias than HITRAN 2016.

□ Page9, lines 211-212: "This indicates a positive bias of OMI against GPS for small TCWV and a negative bias for large TCWV"

The sentence has been changed following the advice.

 Page 11, lines 235-236: what might be the reason for the rapid increase of r from f=0.05 to f=0.15? The other parameters are changing more smoothly between the different f ranges (as well as the r for the other f ranges).

Firstly, the error in cloud top pressure decreases with cloud fraction (Veefkind et al., 2016). As a result, f = 0.05 corresponds to the largest uncertainty in cloud top pressure and the error will propagate to OMI TCWV through AMF, leading to smaller correlation coefficient. Secondly, this is related to the effective dynamical range of TCWV. There is a larger fraction of data pairs with TCWV > 40 mm for f = 0.15 than for f = 0.05. A larger dynamical range generally favors a larger correlation coefficient. The explanation has been added.

 Page 13, lines 267-268: "suggesting that OMI cloudy TCWV is larger than OMI clear TCWV in general". Come up with an explanation here.

We have added a sentence to explain. Basically, other things being equal, cloud formation indicates water vapor saturation and therefore higher TCWV than that under clear-sky condition.

 Page 13, lines 273-274: "In most cases, higher cloud fraction thresholds correspond to larger σ values." Give an explanation here.

This is consistent with the larger dynamical range (due to a larger fraction of data with high TCWV) for larger cloud fraction thresholds. The relative scatter, however, shows little dependence on cloud fraction threshold. A comment about this has been added.

 In Section 3.2, you do not compare the version 3 OMI –SSMI TCWV retrievals with the version 4 OMI – SSMI TCWV retrievals. As you did it for GPS (over land), we lack the information of the version 4 behaviour w.r.t. version 3 over the oceans.

We have added the information. Essentially, Version 3 OMI TCWV has significantly larger bias than Version 4.

 Page 16, lines 348-351: this part belongs to the section describing the sensitivity of the OMI-GPS TCWV differences, and not here.

We mis-typed OMI-SSMIS as OMI-GPS. The error has been corrected. Thanks for catching it.

 In contrast, I would add a paragraph at the end of section 3 in which you mention the overall conclusions of the OMI TCWV validation with both GPS and SSMIS (e.g. best agreement in the 10-20/30 mm range, worse for smaller & higher TCWV ranges + reasons) and some conclusions on the improvement of version 4 over version 3.

The overall conclusions from the comparisons are summarized in the "Summary and Conclusion" section.

 Page 17, Fig 7a: indicate the July 2010 and July 2015 epochs on the time series of the ENSO index.

We have drawn dashed vertical lines to indicate the epochs in the plot.

 Page 17, lines 368-373: mentioning Level 3 and Level 2 for creating the different climatologies is confusing to me. Basically, you first construct the long-term (2005-2015) July TCWV monthly mean map (climatology). Then you create the July 2010 monthly mean map, and the July 2015 monthly mean map and you calculate the differences of those monthly means with the long-term July climatology, right? Shouldn't you use exactly the same dataset (Level 2 or Level 3) for those monthly mean maps?

The procedure described above is indeed what we used for the figure. Averaging the monthly Level 3 July data is an alternative way of composing the July climatology. It does not make any noticeable difference for the purpose of this figure.

- Page 17, lines 374-377: personally, I would prefer not to use the verbs “increases” and “deceases” when comparing a monthly mean of a specific month with the long-term monthly mean (=anomalies), but rather reserve those verbs in describing trends in time series. I would rather use “is elevated/higher w.r.t. “

We have changed to “higher/lower”.

- Page 18, line 381: if you give a possible reason for the differences in details, then you should also specify what those “differences in details” are.

We have deleted this part, as it is not essential for this paper. Readers who are interested in the details can compare with Shi et al. (2018).

- Page 20, line 412: write out NARR.

It has been written out.

- Page 20, line 418-419: Describing Figure 9, you write that “TCWV is generally lower in the run without evapotranspiration”. This is true, except in the lower boundaries of the box. Where does it come from?

The higher TCWV in the No ET run near the southern boundary reflects the non-linear water vapor transport from the Gulf region. Note, turning off evapotranspiration not only affects the water vapor flux from the surface, but also influences other meteorological variables, such as temperature and winds. Thus, there is a difference in the water vapor flux across the domain boundary. A comment has been added in the paper.

- Page 21, lines 439-448: You use a very detailed description of the AR event of 6-7 Nov 2006, based on datasets that are not used/shown here. Could you not describe the event shorter – process-wise – and refer to the frequently cited Neiman et al. 2008 paper for more details?

We have shortened the description and combined the original Section 4.3.1 and 4.3.2 into one subsection.

- Page 22, lines 465-466: “is consistent with the dark stripe in the upper tropospheric water vapor image obtained by GOES-11” show similarities to the formation processes, not to datasets or observations not shown here.

We have deleted this part and pointed out that the feature is associated with the same extra-tropical cyclone as the AR is.

- Page 24, Figure 11: please add in the figure caption that the grey color coding means no data available.

We have added in the figure caption “Gray color indicates area with no SSM/I data”.

- Page 25, line 523-524: specify the “error” in the simulated AR structure (i.e. too strong southern filament of TCWV).

We have specified the error according to the suggestion.