Changes in the Revision

(1) More details about the retrieval algorithm of MERSI-II PWV product are presented.

(2) The analysis in section 4 is improved and section 4.2 is shortened.

(3) The section 5 is removed.

(4) The discussion is improved.

(5) All figures are replaced.

(6) Some related references are cited.

(7) Some sentences are rewritten.

(8) The English of the paper has been improved.

Responses to the Reviewer's Comments

Thanks for the reviewer to provide very useful comments and suggestions, and please see our responses in the following:

General Comments

(1) There is no detailed information (or reference) given about the retrieval algorithm, possible adaptations specific for MERSI-II data, any assumption made or possible external data used in the generation of the PWV product.

Response: Thank you. The detail of the retrieval algorithm of MERSI-II PWV is presented in the revision (lines 116-156, tracked manuscript).

(2) The paper presents a statistical analysis of collocated data without much physical interpretation.

Response: Good suggestion. More physical interpretations of differences between IGRA and MERSI-II are added in the revision, and there are some possible reasons added.

(3) Actually, the observed differences between the MERSI-II PWV and the radiosonde data are quite small (only a few percent). I would expect that this is within the expected precision/accuracy of the involved products, especially taking into account the large spatial and temporal variability of water vapor. Nevertheless, a lot of effort is made to (statistically) quantify the reasons for these differences, namely distances in space or time or surface altitude, although the findings are essentially as expected (e.g. better agreement for smaller distances). Therefore, this part of the paper can be significantly shortened.

Response: Thank you. In this section, we want to quantify the influence of some factors on the evaluation of MERSI-II PWV and it is important in the application of MERSI-II PWV. However, the factors we selected are too many and the analysis can be shortened. So, we only discussed the spatial distance and the value of IGRA PWV. All analysis and conclusion have been rewritten in the revision.

(4) The conclusions drawn for the Qinghai-Tibet Plateau application are not supported by the corresponding data/analysis. They are mainly based on the comparisons of two stations, which actually show a very similar seasonal variation (the data agree within their 25%/75% percentile ranges). So, how can there be any conclusion about transport be drawn? Of course, water vapor transport is affected by the terrain / surface elevation, but this is no new finding.

Response: Thank you. In this section, we want to get the conclusion that the PWV transport of the Brahmaputra Grand Canyon is important by comparing the PWV

variation of two stations. However, according to the suggestions from the reviewers, and the discussion with co-authors, this section is removed.

(5) The English of the paper needs improvement. In may cases, 'the' is missing in the sentences (also in the title before 'Precipitable'); use of singular/plural is wrong, and wording/formulations are sometimes misleading, e.g., when using in the abstract 'distance discrepancy' instead of 'spatial distance'.

Response: Thank you. The grammar, spelling, punctuation and phrasing of the paper has been improved.

Specific Comments:

(1) Mentioning a 'peak value of the MB' of 0.00 is misleading, especially since in the following underestimations are mentioned. It should be clarified that 'peak value' refers to the histogram analysis.

Response: Thank you. The sentence is rewritten (lines 16-17). The histogram of all MB shows that the MB is concentrated around zero and mostly located within the range from -1.00 cm and 0.50 cm.

(2) In the Introduction only NIR satellite water vapor measurements are addressed. PWV is measured from satellites at various spectral regimes (VIS, NIR, SWIR, TIR, MW, ...); especially the MW measurements have a very long heritage. This should at least be mentioned.

Response: Good suggestion. The information of satellite-based retrieval of PWV is added in the revision (lines 60-62). Over the past few decades, the satellite-based PWV retrieval algorithms are developed with the observations from different sensors, which mainly can be divided into four types according to the spectral region: (1) visible (VIS), (2) near-infrared (NIR), (3) thermal infrared (TIR), and (4) microwave (MW).

(3) 'upward radiance over the view of satellite' – unclear formulation. Probably you mean that the radiance measured by the satellite instrument is affected by water vapor. Please clarify.

Response: Thank you. We actually want to express the influence of water vapor in the satellite measurements and the sentence has been rewritten in the revision (lines 37-38). Furthermore, water vapor can also influence the atmospheric transmittance and the upward radiance measured by the satellite sensor.

(4) l. 30. This is not even a real sentence - please reformulate.

Response: Thank you. We have rewritten this expression in the revision (lines 71-73). For MERIS, the PWV retrieval algorithm employed the ratio of top of atmosphere (TOA) radiance at one water vapor absorption channel (around 900 nm) to the TOA

radiance at the atmospheric window channel such as 885 nm (Bennartz and Fischer, 2001).

(5) Section 2.1 lacks a description (or a proper reference to) the MERSI-II PWV retrieval algorithm.

Response: Thank you. The detail of the retrieval algorithm of MERSI-II PWV is presented in the revision (lines 116-156, tracked manuscript).

(6) The radiosonde product is defined as surface-to-500-hPa PWV - this means that it does not include some percents of the total column. Has this been considered in the comparisons? This may be relevant as observed differences are also in the order of a few percent.

Response: Good suggestion. We consider the dry bias for the radiosonde PWV, and the related citation is added in the revision (lines 168-171). As discussed by Turner et al. (2003), the PWV obtained from radiosonde has an approximate 5% dry bias compared to that derived from the MWR. Therefore, there is an underestimation of PWV evaluation for taking the IGRA-derived PWV as a reference, and the bias found in tropical areas is ~9% (Zhang et al. 2018).

(7) Section 3.1 could possibly be shortened as all statistical indicators except for EE are quite common. However, the definition of EE is not fully clear. In the text, EE is described as a limit in percent whereas eq. 5 results in absolute values. Furthermore, eq. 5 includes an offset of 0.05 which needs to be explained - is this specific for IGRA data and derived from some validation activities? Please clarify.

Response: Thank you. This section is shortened and the EE is deleted in the revision.

(8) 'In processing, only the point that has matching data in each interval is selected for the comparison reliability.' This sentence is unclear, please clarify / reformulate.

Response: Thank you. In processing, only the existing AERONET PWV, which has the matching averaged AERONET PWV in each temporal discrepancy interval, is selected for the determination of the temporal collocation window. This is presented in the revision.

(9) Why is the number N of collocations the same for all time intervals?

Response: Thank you. This can be explained by the response of above comment (8). Only the AERONET PWV is used in the comparison when it has the matching averaged PWV in each temporal interval, that is, if there is no matched PWV in any interval, the data is discarded. Therefore, there is the same number of collocations for all the temporal discrepancy intervals. And This is expressed in the revision (lines 216-219).

(10) l. 179. Please justify the 0.25 cm limit for STD used here.

Response: Thank you. In the processing of satellite data, we hope to eliminated the PWV retrieval with a large variation in the selected 9×9 box. But according to the comments from the reviewers, we are not using this criterion anymore and the data

are recalculated.

(11) 'the closest PWV retrieval ... is selected'. Do you mean spatially closest?

Response: Thank you. Yes, the spatial closest PWV retrieval within 50 km away from the IGRA site is selected. It has been discussed in the revision (lines 247-248).

(12) Instead of Fig. 3, section 4.1 should include a global map of MERSI-II PWV data (e.g. daily or monthly, depending on coverage) on which the global water vapor distribution is discussed.

Response: Good suggestion. We have presented the monthly averaged PWV and the global distribution of the PWV in the revision (lines 262-270).

(13) What do you mean with 'unnormal' and 'intuitive acknowledge'? Do you mean that there is an outlier filter applied here? Is this part of the MERSI-II product (if not, why do you need it)? Is this filter also applied for other analyses in the paper?

Response: Thank you. Actually, there are some outliers in the histogram analysis but they are mostly distributed in the tail of the histogram with a really small sample number. In order to present more detail about the distribution of the MB, only the main range of the MB is shown, and the data is not eliminated. However, in the revision, all MB are presented in the histogram without any filter applied.

(14) Fig. 4 and related text: The histograms given in Fig. 4 are produced using a certain bin width, which should be specified. This bin width should also be considered when specifying values in the text (no. of digits). For example, you cannot derive an MRB value of -2.38% (2 digits accuracy) if the bins are e.g. 1%.

Response: Good suggestion. We have rewritten the expression in the revision. For the histogram of MB, the bin width is 0.05 cm and it is described in the revision.

(15) l. 228–230: To which MERSI-II/MODIS values do you refer here (MRB, STD)? Note that the accuracy of the MODIS product is not the same as an error.

Response: Thank you. The MRB is mentioned here and the part is deleted in the revision.

(16) Fig. 5 and related text: The percentage values given in the text cannot be inferred from Fig. 5, they should be taken from the histograms in Fig. 4. Actually, it seems that the given numbers are not in line with Fig. 4, e.g.:

In general, because the sampling at each station is different, it is unclear how representative Fig. 5 and the conclusions on spatial distribution of deviations etc. are. This needs to be discussed.

Please also discuss some potential (physical) reasons for the spatial distribution of differences in addition to sampling.

Response: Good suggestion. The percentage value can be inferred from Fig. 5 and this is because we divide the bias value into normalized value, that is, each color represents 10% of all sites. It should be considered the influence of the difference of sample number at each station, and we add the discussion in the manuscript (lines

322-325, 347-348). We also describe the potential (physical) reasons for the spatial distribution of differences and the discussion is also presented in the revision.

(17) Section 4.2 could possibly be shortened. Is, for example, Fig. 6 (seasonal results) needed in addition to Table 1 (monthly results)?

As this section is about temporal variations it would be good to show at least one example for a full time series at a station in a plot.

It should also be justified why the results are considered to be significant noting that e.g. differences in MB for different months in Table 1 are small.

Response: Thank you. This section is shortened in the revision, and only the seasonal analysis is presented. Actually, this section is focused on the annual performance of MERSI-II PWV rather than the temporal variation, and the title of this section is rewritten in the revision. Furthermore, the results for different months are removed.

(18) l. 271–272: 'with a high positive MB in winter'. Actually, the MB given in Fig. 6d is negative and small.

Response: Thank you. This sentence is rewritten. For the largest MRB during winter in the Northern Hemisphere, it is may be related to the points with a small PWV value but a high positive MB, because we firstly calculate the MRB for each match-up and then average all MRB values in seasons (lines 368-370).

(19) Section 4.3: As already mentioned in the General Comments, it should be justified why a detailed analysis of influence factors is required at all.

Response: Thank you. We want to quantify the influence of some factors on the evaluation of MERSI-II PWV and it is important in the quantitative application of MERSI-II PWV. In this section, the influence factors, such as the value of IGRA PWV, the spatial distance between footprint of satellite and IGRA station, are all explored in order to quantify their effects on the evaluation of MERSI-II PWV (lines 411-412).

(20) l. 341–343: 'there is no height correction that can be used to eliminate false signals...'. Which height is assumed in the algorithm? Is varying surface elevation considered? This is an example why more information about the algorithm is needed.

Response: Thank you. In the retrieval algorithm, the varying of surface elevation is considered but there is no height correction in the matching between MERSI-II and IGRA. And this part is deleted in the revision.

(21) l. 347–349: How are the 'hazy conditions' identified? 'the influence of haze is hardly corrected completely in the MERSI-II PWV retrieval algorithm'. Does this mean there is a correction or not? If yes, how does it work?

Response: Thank you. There is only the cloud detection in the retrieval with the cloud mask product. Therefore, the hazy with a low optical depth is hardly detected. There is no correction in the MERSI-II PWV retrieval algorithm, and this should be explored in the future. (lines 592-593).

(22) Table 2 and related text: Noting the different sampling of the stations and their

distribution with latitude it should be justified why the presented values are significant.

How much do the results depend on the selected month?

Regarding altitude dependence, what is the vertical sensitivity of the MERSI-II measurements?

Response: Good suggestion. The limitation of MWR-derived CLWC under heavy rains has been indicated in the revision.

(23) In general, it is very difficult to follow the argumentation in section 5.

Response: Thank you. The section 5 is removed in the revision.

(24) l. 388: 'the trends of PWV at the two sites are similar'. There are no trends shown in Fig. 9. Do you mean 'seasonal variation'?

Response: Thank you. The section 5 is removed in the revision.

(25) Fig. 9d and related text: Please confirm that the PWV data shown in the time series are from MERSI-II and not radiosondes.

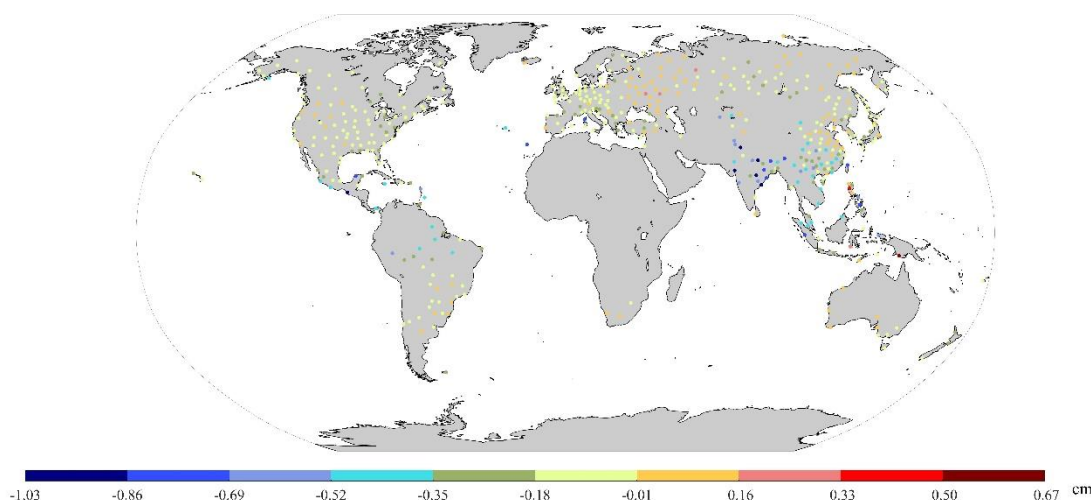Response: Thank you. The section 5 is removed in the revision.

Technical Corrections:

(1) In most cases, too many digits are given for values in the text. Only significant digits (in relation to the uncertainty of the products and the analysis methods) should be given. Two digits should be usually sufficient, in case of percentage values even less.

Response: Good suggestion. The MB and RMSE are with two digits, the MRB is with one digit and the CC value is with three digits in the revision.

(2) Color bars of figures: Figures 1–8 use a discrete color bar which contains quite similar colors at the lower and higher end (light pink, orange) which can be misleading. Furthermore, the steps between colors are in most cases not equidistant, which makes a visual interpretation difficult, especially for the map plots. I suggest to use for all figures a color bar and colors e.g. similar to the ones in Fig. 9, or maybe even a conceptually uniform color map.

Response: Good suggestion. The color bar is changed in all figures. We have tried to plot all figures with equidistant steps; however, the presentation is unclearly because there is a lot of sites with the same color as shown in the below figure.

(3) Fig. 1 (and actually all scatter plots): '=EE:' although explained in the text, this notation (with a '=' in the variable name) is a bit misleading. I suggest to rename this.

Response: Thank you. We have deleted the EE description in the revision.

(4) Fig. 1: Remove symbols on top of panel (a).

Response: Thank you. The symbol is removed.

(5) Figs. 3 and 5 (and possibly Fig. 2): As these show global maps I suggest to remove the labels on the latitude and longitude grid.

Response: Good suggestion. The labels on the latitude and longitude grid is removed in the revision.

(6) Fig. 5: Please describe panels a–d in the caption and label in each figure the quantity (MB, ...) shown.

Response: Thank you. We have added the description of Fig. 5.

(7) Figs. 7 and 8: Suggestion: These figures are not really needed - their results could be placed in a table instead.

Response: Good suggestion. The results are presented in Table 3 in the revision.

(8) l. 334: As I understand, MEAN is not the annual mean value but the mean over all values in April.

Response: Thank you. Yes, MEAN is the mean over all values in April. And this part is removed in the revision.

(9) Table 2: Remove/adapt line breaks in left column / top line.

Response: Thank you. It is improved in the revision.

(10) l. 366–367: 'As we all know, water vapor can significantly affect climate change, radiation balance and hydrological cycle.' This sentence is essentially identical to one

in the introduction and can be deleted.

Response: Thank you. This sentence is deleted in the revision.

(11) Fig. 9: Please include/mark the Brahmaputra River and the Brahmaputra Grand Canyon in the maps. This would help a lot to follow the argumentation.

Response: Thank you. This section is deleted in the revision.