Dear Marc,

we uploaded our revised manuscript.
The most important change is that we applied (two) established methods for determining the significance of the different teleconnection indices (following the request of reviewer 1).
We also addressed all other comments of the reviewers. In cases where we disagree, or where we assume larger misunderstandings, we gave detailed explanations (see our detailed responses below).

Best regards,

Thomas

Reply to reviewer #1

Reviewer comment are shown in black
Our replies are shown in blue

First of all, we want to thank the reviewer for his/her second review.

In order to account for the request of using established methods for determining the significance of the different teleconnection indices, we performed two different tests: the so-called Walker test and the false discovery rate (FDR) test (see e.g. Wilks, 2006; 2016). The Walker test uses the minimum local p value as the global test statistic. The FDR test compares the p-values of all grid pixels with the distribution of the statistically expected FDR. Both tests deal with the question of "field significance", i.e. whether an index is significant anywhere. Another advantage of both tests is that they are rather robust with respect to spatial correlations of the input data. We also now used a generalized least squares fit accounting for temporal autocorrelation via a first-order autoregressive process. We found that these established statistical procedures (Walker and FDR test) yielded almost identical numbers of significant indices. The results were also very similar to those we derived from our empirical approach (for more details see below).
Thus, we hope to convince the reviewer (a) about the significance of our results and (b) the validity of our empirical approach, which is now complemented by established statistical methods in the revised version of our study.
We also addressed all other comments of the reviewer. In cases where we disagree, or where we assume larger misunderstandings, we gave detailed explanations.

Second review of ACP-2020-565

Recommendation: Reject

General comments: While the authors did make strides to improve the explanation of the methods they employed, and their efforts did make their methods more clear, I obtained additional clarity in their approach and found several glaring concerns that need to be addressed. These concerns fall into three primary areas:

1) The blended use of teleconnections for specific spatial regions as a global product, even though many of these indices were not derived globally when they were created

Several studies investigate the effect of teleconnections on a global scale, e.g. Hsu and Lin (1992) or Hoskins and Ambrizzi (1993). We also see no problem (instead we see it as an interesting application) to fit indices to regions far away from the regions for which they were originally defined. If an index is not important at a given location, the fit coefficient will be small. If, in contrast, the fit coefficient is found to be large, this is an interesting and potentially important hint that the teleconnection index might be of importance even far away from the region for which it was originally defined. Here it should be noted that for ENSO or QBO, influences were indeed found far away from the regions where they were defined.

We would see it as an unjustified pre-selection to restrict the analyses of the individual indices to limited regions. Even if no new discoveries are made, we see at least no negative consequences by performing the fits of all indices on a global scale.

We should maybe also clarify, that If we find an index to be significant, this does not mean that it is significant everywhere on the globe.

2) The use of the ERA model data versus raw satellite data, and the subsequent use of their TCWV approach as effectively a verification measure for the ERA

Here two aspects are important:

a) Why were the model data included at all in the study?

The satellite data are derived for the overpass times and for (mostly) clear sky conditions. Therefore, it was important to test whether the satellite data can be seen as representative for all times and conditions. To answer this question, model data are well suited, because results for specific selections can be compared, e.g. results for all data or only clear sky daytime conditions. We made such comparisons based on ERA-interim data and could conclude that the satellite measurements can be seen as representative for the all-sky conditions.

b) As discussed in Beirle et al., 2018, the long term satellite data set was created with a focus on temporal stability (and consistency between the different sensors). To achieve this aim, for the data analysis a simplified analysis procedure was applied (e.g. by using the oxygen absorption in the red spectral range instead of operational cloud products). It was shown in many studies that especially the standard cloud products of satellite instruments are often affected by instrument degradation and are also not consistent between the different sensors. These problems are overcome by our simplified analysis procedure. This analysis procedure, on the other hand, also has its drawbacks. Especially for individual measurements, the uncertainties can be rather large. Also systematic biases might occur for specific regions, e.g. related to the effect of the surface albedo. These limitations are not important for climate and trend studies, and by comparison with independent data sets, the temporal stability of our global long term satellite data set was found to be very good. However, a direct quantitative comparison to the model data would not make much sense, because of the known systematic biases of our satellite data set.

The main advantage of our satellite data set is that it is very well suited to investigate the temporal variability, e.g. related to teleconnections. Within the scope of this study, the same teleconnection analyses were performed for the satellite and model data. For almost all indices, astonishingly good consistency of the derived spatial patterns and the significance of the different teleconnection indices was found, indicating that the temporal patterns are well covered in both satellite and model data sets.

To make our motivation for the use of the model data in our study more clear, we added the following text in section 2.1 '.The main purpose of using model data is that we want to see if teleconnections are found in a similar way in both satellite and model data sets. In addition, the use of model data also allows to quantify a possible clear-sky bias in the satellite observations, because these observations are made for mainly cloud-free conditions. Therefore we use two data sets:'

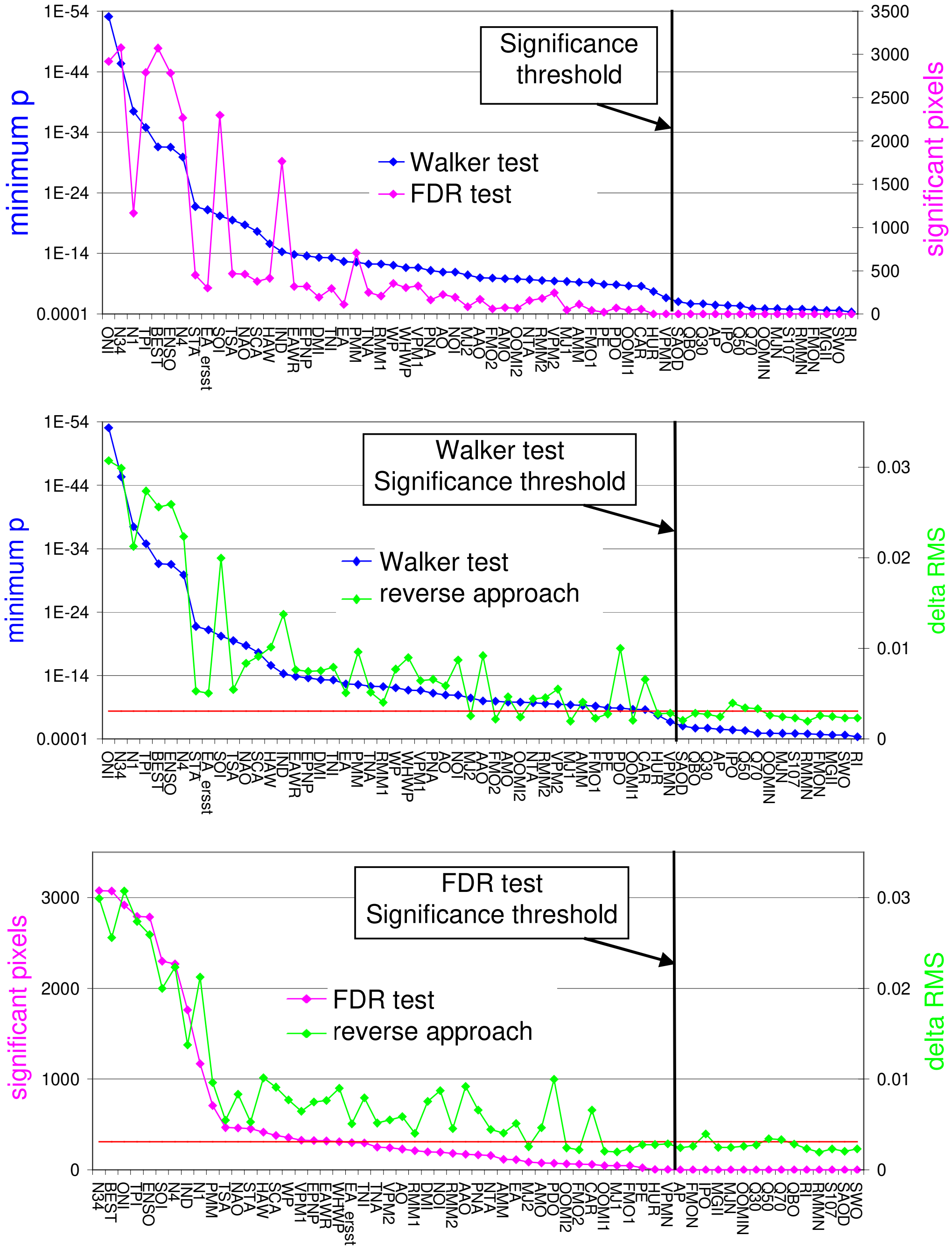
3) Their empirical "reversed index" approach, which effectively is just a basic non-parametric hypothesis test.

As suggested by the reviewer, we now applied established methods for testing the significance of teleconnection indices to our global water vapor data set. For that purpose we chose the so-called Walker test and the false discovery rate (FDR) test (e.g. Wilks, 2006; 2016). We also accounted for temporal autocorrelation effects within the fit method by assuming an AR(1) process of the fit residual (Seabold and Perktold, 2010). From the fit results we derived the local p-values (by a two-tailed t-test) of each fitted index for every grid cell. To account for the effect of test multiplicity, we then applied the Walker and FDR test to the global distribution of p-values for each teleconnection index.

Both tests are well suited for global applications as they are robust to the effects of spatial correlation. Interestingly, especially for the FDR test very similar results were obtained compared to our empirical approach, see figure below. Only a few indices with low frequencies (Q50, Q70, and IPO) which were previously be found to be slightly above the significance level, are now found to be slightly below the significance level. Conversely, some previously non-significant indices with high frequencies are now found to be slightly above the significance threshold. These changes are related to the fact that for the new method we also used a modified fit explicitly considering the temporal correlations of the indices.

The most important finding is that these differences between the FDR test and the old method are only found for indices close to the significance thresholds and don't affect the main findings of the paper. The number of significant indices found for the old and new method differs only by 2 (42 for our empirical method and 44 for the new method) if one takes into account that the OOMI2 and FMO2 as well as the OOMI1 and FMO1 indices are very similar.

Because of the good agreement between our empirical approach and the new standard method, we decided to keep the results of our empirical method in the paper. We added a new section to the paper (section 5.2) which presents and discusses the comparison results between the empirical method and the well established statistical methods from literature.



New Fig. 8 Comparison of the results of the three approaches. Top: results for the Walker test (left axis) and FDR test (right axis). The indices are sorted according to the results of the Walker test. The black vertical line indicates the significance threshold (similar for both tests). Middle: comparison of the results for the Walker test (left axis) and our empirical (reverse) approach (right axis). The indices are sorted according to the results of the Walker test. The black vertical line indicates the significance threshold of the Walker test. The red horizontal line indicates the significance threshold for our empirical approach. Bottom: similar as for the middle panel, but for the FDR test.

The third of these issues impacted the interpretation of all results as it caused the authors to identify results as significant without proper context. Fixing this issue would require the authors to completely redo the analysis with more appropriate methods. As a result, I must still recommend rejection of this manuscript.

For our main reply to this point, please see our previous comment above.
Concerning the interpretation of significance, we want to point out that 'significance' does not mean that an index is significant everywhere on the globe. Our interpretation of significance is that of 'field significance' as described e.g. by Wilks (2006). We added the following clarification in the introduction: 'Here it should be noted that with significance we don't mean that an index is significant everywhere on the globe. We are rather interested in whether an index is significant somewhere on the globe, the so-called 'field significance' (see e.g. Wilks, 2006).'

Major comments:

The authors did not really address my concern with the barotropic conditions in the tropics and why these regions are not used. In my experience deriving teleconnections, including barotropic regions generates large areas of high correlation due to the minimal height gradients in the tropics. Often these large areas wash out teleconnection features that would otherwise be present when using PCA (i.e. the first PC almost always exclusively identifies the tropics instead of a hemispheric teleconnection).

For us, this comment is not completely clear. But probably there is a misunderstanding here: our aim was not to derive teleconnections. We simply tested how strong existing teleconnection indices correspond to the variabilty of the water vapor distribution on a global scale. Such global studies are not unusual, see e.g. Hsu and Lin (1992) or Hoskins and Ambrizzi (1993).

As many teleconnections the authors considered (the NAO, PNA, etc.) were derived without including the tropics or the Southern Hemisphere, it is unclear how employing these teleconnections on a global study even makes sense, or even employing the teleconnections in the tropical latitudes which were not used in deriving the indices. How did you address this issue?

As mentioned in our reply to the first overall point, we see no problem (and instead only advantages) by analysing all indices for all grid points of the global data sets.
If one looks at the derived patterns in Fig. A9, one can clearly see that in the tropics especially for many atmospheric indices, indeed low fit coefficients are obtained. In Fig. A12, it can be seen that after the normalisation, the fit coefficients in the tropics are further strongly reduced. We find these results interesting and relevant. They can only be obtained if the indices are fitted to the data sets globally. In our opinion, there is really no ‚issue' here.

It seems like a strange methodological approach to verify the ERA model representations of TCVW using these teleconnection renderings (lines 87-90). Why not just directly verify the TCVW model data with the observation dataset?
It seems like a very roundabout way to do model verification. Maybe a bigger question is why you are including the ERA data. Why not just use the satellite data directly?

See also our reply to the first general point above. Our intention was not to verify the ERA model. The main aim was to see whether there is a systematic clear sky bias in the satellite observations. From the satellite to model comparison we can conclude that no such bias is found.

The differences do not seem that dramatic and there is no effort to explain why you did this except to compare model data against satellite, which does not help remove the “forecast” confusion in this study.

In our opinion, the “forecast” confusion of the first version of the manuscript should have been clarified in the first revised version. It was never our intention to make forecasts.

The use of the delta RMS quantity is strange. The authors even note (lines 209-210) that this quantity is basically the same as the correlation coefficient between the fit and the teleconnection. This makes sense as essentially you are doing a multivariate linear regression with an extra time term and you are just computing the variance explained by each teleconnection. This should scale almost exactly to just the correlation squared between the predicted value and the teleconnection. Why use this delta RMS instead of something simpler like R2 to quantify the relationship between the teleconnection and the TCVW? Are there studies that employed a similar methodology?

The reason to use the delta RMS is that the results for the individual indices directly quantify the contribution by that index to the variability of the data set. Thus, it quantifies the *relevance* of the different indices, (while significance alone does not imply that a signal is relevant). This is especially important when these contributions are summed up to derive the cumulative effect of all indices (e.g. for the orthogonalised indices).

We found no other studies which used the delta RMS value for the determination of significance. But quantities very similar to the delta RMS were used in several studies to quantify the individual or cumulative contributions of different temporal patterns to the total variance (e.g. Horel, 1981; Trenberth and Paolino, 1981; Barnston and Livezey, 1987). There, the aim was to determine the order of importance of the different time series. This aim is similar to our study.

We also want to note here that we applied our ‚reversed index method’ also to the r^2 values (instead of the delta RMS), and we found exactly the same number of ‚significant’ indices (only two indices with delta RMS values slightly below the threshold were now found to be slightly above the r^2 threshold (and vice versa).

I’m not sure your interpretation of the fit coefficient is correct. Are the time coefficients always the same for all teleconnections? If so, differences in magnitude in the teleconnection could be the reason for the change in the fit coefficient, not the actual amount of fit. You even show an example of this in Fig. A7, where the normalized ENSO index has a range from roughly +/-3 while the WHWP index has a range from +/-5. An almost 100% increase in magnitude in the index would affect the coefficient magnitude dramatically yet not explain any more variability.

Note that the TCWV anomaly was detrended and deseasonalized before the fit, such that the time coefficient is negligible.
In order to make the indices comparable, we have normalized them to their respective standard deviation, which is a common procedure (see e.g. Horel, 1981). The absolute fit coefficient is not of main focus in our study, as the importance of a teleconnection is quantified by the delta RMS, which is not affected by a scaling of the index.

With the volume of indices considered (Fig. A6), many of these have notable longer-term trends (MGII, Sunspots, PDO, AMO, etc.) where you do not even get an entire phase shift in your 20 year study period. How can you say with any certainty that there is a relationship without getting more than a single period of these quantities being in a “high” or “low” phase?

It is true that for some of the considered indices no complete phase is covered by the 20 year study period. Nevertheless, we don’t see this as a reason not to include these indices. Even if not a full period is covered by the 20 year period, it is still interesting to investigate whether the temporal signature is found in the considered global data set. Of course, it is well possible that potential teleconnections caused by effects with very low frequency could not be detected by the 20 year record we are using. We have added a the following text to section 3: ‘Here it should be noted that for some of these indices with low frequencies (e.g. MGII or IPO) no full period is covered by our 20 year-long satellite TCWV data set, which might be a reason why they are not significantly detected’.

I would guess the small RMS values in the tropics are almost entirely a function of the barotropic conditions in the tropical regions (see my first comment above), not related to the impacts of clouds on the observations (as suggested in lines 222-224). Polar regions have similar issues as their conditions tend to be quasi-stationary. This alludes back to my earlier comment regarding the use of the tropics in this study.

We think there is a misunderstanding here. In the paper we wrote with respect to the effect of clouds: ‘In mid-latitudes, systematically higher RMS are found for the satellite observations compared to the model results. This is probably related to the rather large effects of clouds on the satellite observations, which becomes especially important in these regions (clouds lead to less valid observations and larger measurement uncertainties).’.
We did not say that the RMS is in general high in mid-latitudes because of clouds, but that is it higher there for the satellite data set compared to the model data.
We think our description and explanation of the cloud effects on the satellite data set was reasonable and correct. We see no need to change it.

While I technically agree with your statement on line 255, this is the nature of hypothesis testing. Most studies select a level (typically 95% or 99%) and go with it. I think the more important issue is trying to establish significance of these results using a hypothesis testing approach, since these tests are sensitive to sample sizes (e.g. you can get significance with a large sample size that could still have a poor relationship between the variables).

We followed the suggestion to apply standard hypothesis testing approaches, see our comments to the first points above.

Are there citations of other studies that have used the “reversed index” approach you employed in this study?

To our knowledge, we are the first who used this approach.

I have several concerns about it. First, reversing the time series does not ensure this is a completely uncorrelated relationship; random number generation would do a better job of that.

We fully agree with the statement that ‚reversing the time series does not ensure this is a completely uncorrelated relationship’. We obtained exactly the same finding in our study (see Fig. A2).
In order to address the effect of such correlations, we applied the method described in appendix 2: all indices with high correlations to important original indices are removed before the delta RMS threshold is determined. This removal is crucial. If these indices were not removed, the resulting thresholds would indeed be much too high.

As suggested by the reviewer, we also used a large number (100) of random time series to determine the significance threshold. The derived threshold value was slightly smaller (0.0027) than that from the reversed time series (0.0031). This indicates that the threshold from the reversed time series is the more conservative estimate.
We modified the already existing information about the threshold derived from (only few) random time series by the following text: ’We also applied the same method to a set of 100 artificial random time series and obtained a slightly smaller threshold value of 0.0027 indicating that the threshold value obtained from the temporally reversed time series is reasonable.’

As an example, I used a 70 year ONI time series and simply correlated the time series against its reverse and found a correlation of 0.16, which certainly is higher than what a random dataset should yield. This is even clear with several of your indices where the RMS values were fairly large considering this is supposed to emulate a “random” comparison.

We found the similar behavior for several indices, see Fig. A2. Therefore we applied the method described in appendix 2). As mentioned above, this procedure is essential for the application of the reversed index approach. One important advantage of the reversed index approach is that it covers all frequencies of teleconnections.

Second, the black dotted line is based on the mean and standard deviation of the reversed time series RMS 99th percentiles, yet when I look at the plot I see several points that would be “significantly” better than the reversed time series threshold. These points would also drive that mean upward as they are outliers (S107, MG11, etc.). This calls into question the validity of this approach since using a different statistics (e.g. the maximum) would cause almost all of your “significant” points to shift to non-significant.

The high values mentioned by the reviewer belong to exactly the indices with high correlations to important original indices. As mentioned above these indices were removed before the calculation of the thresholds (see appendix 2). This step is crucial for the application of the reversed index approach.

Maybe most importantly, this approach does not really show statistical significance. If you are treating this as a multiple-comparisons problem (which it appears you are), you need a Bonferroni correction on the cutoff threshold to ensure you are not committing type 1 errors (which are basically guaranteed with the 57 comparisons being done here). This would further shift the cutoff threshold upward and make more of your results non-significant. Why are you not using more traditional methods, such as bootstrapping, permutation testing, etc., to quantify this significance?

As stated above, the main aim of the study is not only to strictly determine whether an index is significantly detected or not, but to establish an order of importance. We find that this aim is well achieved by our method. This is also confirmed by the good agreement with the results of the standard hypothesis testing approaches, see above.

The latitudinal results in Fig. 11 make sense to me since most of the teleconnections related back to ENSO. Why are there so many teleconnections near the International Date Line? You never even discussed the longitudinal plot in the paper from what I could tell and that is a more interesting plot to me (and more difficult to explain).

Many thanks for this hint! This is indeed an interesting finding, to which we had a closer look. The location of 4 of the orthogonalised indices found for longitudes between –167° and –180° is located at latitudes between 38 and 71°N.
We searched for similar findings in other studies and found that in several publications, enhanced activity was found in the same area (e.g. Hsu and Lin, 1992; Hoskins and Ambrizzi, 1993; Trenberth et al., 1998). One possible reason for the enhanced activity in this area might be the effect of jet exit regions, which is driven to a large extent by the Earth's topography (Feldstein and Franzke, 2017). We added this information to section 8.

The authors state that an advantage of their empirical approach is that it "avoids problems of existing algorithms for the determination of significance, because no assumptions on the significance level or the measurement uncertainties have to be made." However, by selecting the 99th percentile you have effectively created an α = 0.01 significance level as you are comparing your observation against an the 99th percentile of an empirical distribution. In effect you just did a hypothesis test, just with a slightly different appearance. If it is different than a hypothesis test it needs to be explained more effectively.

We tested whether our results depend on the exact choice of the percentiles. For that purpose we repeated our approach using the 95th and 98th percentiles of the delta RMS and compared the results to the original results (for the 99th percentiles). For all three cases, exactly the same number of ‚significant' indices (40) was found, clearly indicating that our results do not critically depend on the exact choice of the percentile. This can be understood by the fact that for both the original and the reversed indices the same percentiles are used. Thus the effect of different percentiles almost cancels out. This indicates the robustness of our empirical method.
We added the following text in section 5.1: 'Here it should be noted that the exact choice of the percentile is not critical, as the same percentile is applied to both original and reversed indices. We found exactly the same set of significant indices (see below) if we used the 95th percentile or the 98th percentile.'

Minor comments:

Remove all of the uses of the word "like" in the e.g. statements (lines 46-47 and any others in the manuscript).

The 'like' in the 'e.g. statements' were deleted.

Line 98: "In section 2 the global datasets used in this study" is not a complete thought.

corrected

The y-axes in Fig. 7 should be consistent for both indices.

This statement is not completely clear to us, because the same y axis is used for the original and reversed indices. Maybe the reviewer wanted to suggest to use the same y-axes for the three data sets shown in Fig. 3. We changed the y-axis of the middle panel ('ECMWF all data') to be consistent with the bottom panel. We also added a hint in the figure caption that different y axes are used for the satellite and model data.

What do you mean by "reduced number" of data available? How small? What are the differences? (lines 228-229)

We modified the sentence to: 'The RMS for the model results for clear sky conditions is slightly higher than for the model results for all conditions, which is to be expected because of the reduced number of input data for the cloud-filtered data set (about 40% less compared to the non-filtered data set).'
This information is also added to section 2.1.

Would the result regarding zonal winds in the tropics not just be a consequence of the relationship between geopotential height and wind (lines 315-316)?

We added this information to the text:
'Also for the zonal winds, the largest p99 values are found for the polar atmospheric indices, which is probably caused by the strong relationship between geopotential heights and winds.'

Why would a high surface albedo be a systematic measurement bias? Is the satellite instrument the one with the bias or the ERA data? (Lines 341-342).

To make this more clear, we changed 'biases' to 'biases in the satellite data set'.

We added the following information to section 2.1: 'It should be noted that the satellite data set used in this study was optimized with respect to temporal stability, which makes it well-suited for climate studies. However, because of the rather simple analysis approach, for specific situations small systematic biases of the absolute values might occur, e.g. related to the effects of surface albedo or terrain height.'

There is strange comma use and formatting issues in section 7 of this paper.

Here it is not clear, which strange use of commas is meant by the reviewer. However, we want to point out that the use of English language will be checked by the editorial office before final publication.
We removed the gap between the last sentence and the main text in section 7.

Maybe I missed it, but why are there massive data gaps over Siberia and into India in the satellite data (Fig. 10)?

Part of the gaps are caused by data loss due to calibration of the satellite instrument which takes place north of India. Additional gaps are caused above high mountains by the cloud filter applied to the satellite instruments.
We added this information to section 2.1.

Reply to reviewer #2

First of all, we want to thank the reviewer for his/her second review.

Review for acp-2020-565-manuscript-version2:

This manuscript has been greatly improved compared to its previous version. But there are still some minor issues in this paper.
Thus, I recommend acceptance of this manuscript after considering the following minor comments.

We thank the reviewer for the positive assessment.

Line 17: obtained for ---- obtained from

Corrected

Line 184: constant offset b and possible linear trend c ---- constant offset c and possible linear trend b

Corrected

For section 5 and 6: Considering the structure of the paper, I suggest that the author splits these sections into two parts and give a name for each part. For now, there has only 5.1 and 6.1. But it is just a personal writing style.

For section 5, the issue was ‚resolved' by adding the new section 5.2.

For section 6, we added a new subsection for the first part:
‚6.1 Comparison of the results for the TCWV data sets to those for the other data sets'

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