We would like to thank Chunlüe Zhou for his constructive and detailed review. Below we reply to the issues raised by the referee, where
blue repeats the reviewer's comments,
black is used for our reply,
and green italics is used for modified text and new text added to the manuscript.

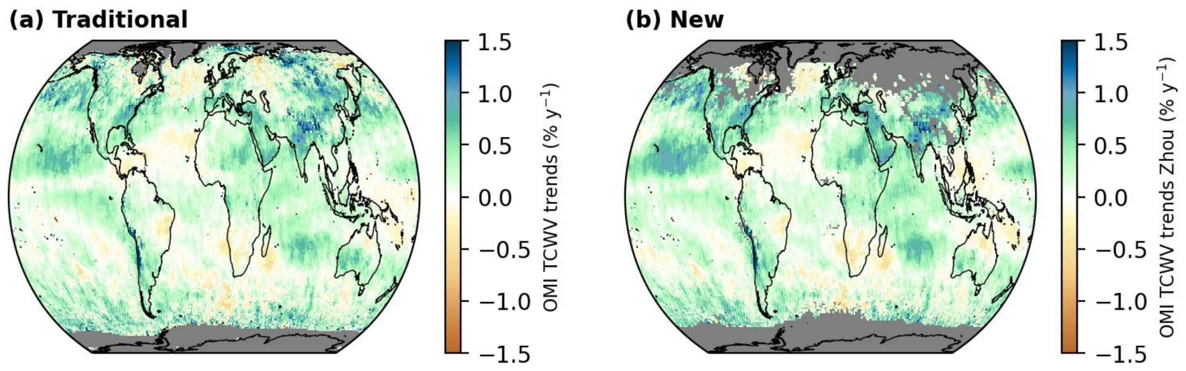
The authors investigated trends in total column water vapor (TCWV) measured by the Ozone Monitoring Instrument (OMI) from 2005 to 2020, and combined air temperature to discuss changes in relative humidity and associated TCWV response to global warming. This is a hot topic in our climate change community, and this study might add some values to the topic. The logic of the manuscript is overall good, but still lacks sufficient discussions with previous studies and also many details, for instances, readers would not know what period of the trend in Figure 3 when they do not read your main text. I think it deserves to be published on ACP after a major revision.
Thanks for pointing out this issue. We added information about the time period of the trend analysis to the title of the respective figures.

Below I made a summary of my main concerns as they pervade the manuscript. Hope they can help improve the quality of manuscript.

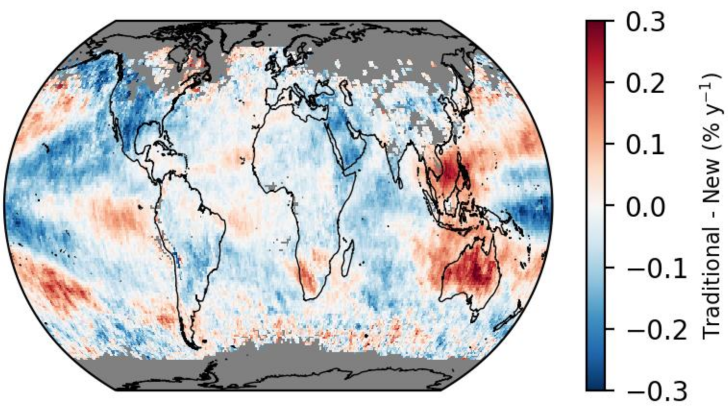
First, the most important question is about possible impact of climate variability on trend estimate. For a short data period (about 15 years), climate variability such as ENSO might dominate the estimated trend. ENSO has diverse impacts on TCWV, so even though the authors removed the ENSO impact by a regression, it is unclear whether that is sufficient. In particular, a regression was done over a short period.
We agree with the referee that it would be desirable to have as long a time series as possible and that 16 years are rather short on climatological scales. However, we would like to emphasise that previous TCWV studies have used similarly short or even shorter time periods. Furthermore, our TCWV data have been obtained from only one instrument and not compiled from different instruments. While this restricts our analysis to the time period of the instrument, one advantage is, for example, that we do not have to consider inter-instrumental offsets.
In the (revised) manuscript we wrote:
[The major advantages of this TCWV data set in comparison to others are that on the one hand the data set provides a consistent time series since it is based on measurements from only one satellite instrument]. Thus, inter-instrumental offsets do not have to be corrected when merging the data time series of the different instruments. [On the other hand, in contrast to other spectral ranges, TCWV retrievals in the visible "blue" spectral range have a similar sensitivity over ocean and land surfaces and thus allow for consistent global analyses.]
Regarding ENSO, the other Referee (Kevin Trenberth) pointed out to us that at least 2 ENSO indices should be considered in the analysis, which we also implemented in the results of the revised version. Thus, we believe that we have not completely eliminated the influence of ENSO on trend, but at least reduced it as much as possible.
In the revised manuscript, we include both: an analysis based on the original data as is, and an analysis including two dominant ENSO indices. The results differ, as expected, but the overall conclusion is not affected.

Second, if directly considering them like Yt = m+ b·Xt +St +Yt-1 +Nt where Yt-1 should include the impacts of ENSO and autocorrelation, what is different from the result of Equation 4? Will be better?

Thank you for this idea! We have tried this "new" approach and the results are shown in the graph below (together with our "traditional" approach). While at first glance the results look quite similar, a closer look reveals a weak dipole structure over the maritime continent in the "new approach" in comparison to the "traditional" results.



This ENSO-like patterns become clearly visible when we take the difference of the trends, and have a comparable structure as in Figure A1, in which we demonstrated the effect of neglecting the influence of ENSO on the TCWV trends.



Even though it might be worthwhile to include ENSO in the analysis of this "new" approach, this would "contradict" its initial idea, we conclude that it is better to stay with our previous approach.

Regarding Equation (4), we would like to point out that it is not a matter of reducing the autocorrelation of Yt, but of the fit residuals or the fit noise Nt. If we did not take the autocorrelation in Nt into account, our significance tests would give us misleading results (see also the beginning of Section 2.2 and Weatherhead et al. (1998)).

Second, about data: The wettest spots locate in India (Fig. 3a vs 3b or all the other figures including Fig. 5), and my main concern is why? Is it related to satellite retrievals? In my recent paper, Zhou et al., (2021), it's found that radiosonde temperature data quality is quite low in India which seriously worsens trend estimate. Is the similar case for OMI TCWV? More relevant reasons will be discussed.

With the inclusion of the TNI and the PMM index in the analysis and the detrending of the teleconnection indices, the trend values have decreased overall, so that in India the values are approximately similar to those over the subtropical ocean. The extraordinarily high values in the Himalayas are probably rather artefacts of the satellite retrieval, as this also depends on surface parameters such as the height above sea level and the albedo. Therefore, similarly high values can also be seen in the Andes region. On the other hand, we have not been able to detect any trends in surface albedo (see also panel (c) in Figure C2).
Thus, we added the following text to the revised manuscript:
We also obtained distinctively high trend values over mountains such as the Himalayas and Andes. However, these high values are likely artefacts due to uncertainties of the satellite retrieval, for example in the input data for the ground elevation. Thus, we decided to filter these artefacts and only show grid cells for which the mean ground elevation is lower than 3000m above mean sea level based on the GMTED2010 elevation data set (Danielson and Gesch, 2011).

Third, about ERA5 and GOME (line 161-164, 167-168): What TCWV products were assimilated in ERA5? Zhou et al., (2018) compared near-surface water vapor pressure trends from the current reanalysis and observation, and some information there can help better show their differences. There are many differences between OMI and GOME, especially in India, North America, Northeast Asia and Europe (Fig. 4). Good to show some statistics about the relationship of OMI and ERA5/GOME? Such as spatial correlation, RMSE? OR show their difference map against OMI? More simple comparisons should be provided rather than only a conclusion.
To our best knowledge, no (satellite) TCWV products are directly assimilated into ERA5, but typically the Brightness Temperature of microwave and infrared instruments and the ZPD of GPS-RO instruments (besides the in-situ measurements of radiosonde data, etc.). See also section 5 in the ERA5 paper by Hersbach et al. (2020).
A detailed analysis of the OMI dataset with comparisons to ERA5 (and also SSM/I and the ESA WV CCI dataset) can be found in Borger et al. (2021a) (doi:10.5194/essd-2021-3, currently under discussion). Therefore, we have refrained from a comparison to ERA5 here.
We reformulated the following text to Section 2.1:
In an extensive validation study, Borger et al. (2021a) showed that the data set is in good overall agreement to other reference data sets such as the satellite data from RSS SSM/I (Mears et al., 2015; Wentz, 2015) or the reanalysis model ERA5 (Hersbach et al., 2019), especially over ocean surface.

Fourth, about TCWS responses to air temperature: The authors estimated a larger response than the theoretical value, i.e., 7%. I think it’s rather reasonable on local or regional scale, because the response on local or regional scale is not only thermodynamic but also dynamical. Zhou et al., (2017) isolated the responses of precipitation to long-term changes and short-term variations in air temperature and showed a much larger response than 7%. More details and discussions can be seen in that paper, and some discussion about possible impacts of short data period, and thermodynamic versus dynamic contributions will be revised into the manuscript.
Many thanks for this hint and the valuable suggestions! However, after reviewer #1's (Kevin Trenberth) assessment, we have decided not to include this section in the revised version.

Finally, about figure: There are several repeated subfigures. I think the authors should keep only % subfigures and remove subfigures for absolute values. Because the latter do not

provide additional information. Is Figure 2a-2b the same as Figure 2a and 2c? It would be better if using blue for wet and red for dry in colorbar.
We have changed the colour bar in all relevant figures from red-blue to brown-blue. Furthermore, when comparing trends between the different datasets, we only focus on relative trends. The absolute trends are made available in the Supplement Section.

Specific comments:
1. Not good to use an abbreviation in Title
Since satellite instruments are mostly known by their acronym (e.g. IASI, SSM/I, MHS, AIRS, MODIS), we would like to stick with the abbreviation in the title.

2. Why not plot directly the autocorrelation values in Figure 1? The sign of autocorrelation also has scientific meaning.
We have adjusted Figure 1 and now also show negative values.

3. Lines 147-149, Figure 2c-2d still show many sparse dots even after applying the FDR test. Could show both results of the Z-test and FDR test?
We have adapted Figure 2 and now show the significant trends in addition to all trends and the FDR trends.

4. Lines 152-153, 'increasing or decreasing H2O absorption' is the same as 'changing atmospheric water vapour content', so change to 'changing saturated water vapour content'?
"changing saturated water vapour content" would imply that the relative humidity would remain constant. Since we find in our work that this is not the case (on local scales), we would therefore stick with "changing atmospheric water content".

5. Line 215, pay more attention to North America and India as comparing RH in Figure 6.
We rewrote the relevant paragraph as follows:
Figure 7 depicts the obtained trends in precipitation as well as the relative RH trends from OMI. Comparing the trend distributions of the monthly mean rain rates to the relative RH trends, negative and positive trends in precipitation and RH match quite well over the tropical and subtropical ocean, especially over the tropical Pacific and the Northern subtropical Atlantic. While over land within the subtropics an acceptable match can be determined in some regions (e.g. South Africa, Brazil), the patterns of the relative RH and rain rate trends no longer match well towards mid and high latitudes (e.g. in North America), likely because in these regions the rain rate is mainly determined by atmospheric dynamics (cyclone or storm tracks) rather than thermodynamics. Furthermore, the distinctive increases of relative RH in the mountainous regions of South America (Andes) and northern India (Himalayas) are likely due to the inadequacies in the OMI TCWV satellite data caused by the complex topography (see also Sect. 3.1.).

6. Lines 131-132, half is not enough, especially for a short period. I recommend some 80 or 90%.
The OMI TCWV retrieval depends on the annual cycle of the solar zenith angle and is only applied over snow/ice-free surfaces, which is why complete coverage is not possible during the winter months, especially at higher latitudes. Thus, for some of these regions, 3-4 months per year are missing. An 80% or 90% filter would therefore remove these regions. Since these regions are particularly interesting for climate studies, we would like to keep them. Instead, we provided a map with temporal coverage in the Supplement/Appendix and

mention at appropriate places in the manuscript (including figure captions) that the values in the high latitudes should be interpreted with caution due to incomplete temporal coverage. We added the following text to Section 3:

At this point, we would like to note that, especially in the high latitudes, complete temporal coverage within the MPIC OMI TCWV data set is not always given. For example, the winter months are often missing because no satellite measurements are available due to the seasonal solar cycle or ice cover. Thus, the trends shown are not representative for the entire year, but only for part of it, and should be interpreted with caution. However, we would still like to present the results, as these regions are of great interest in climate research. A map of the fractional temporal coverage is provided in Fig. S1 in the supplement.

References:

Zhou, C., Wang, J., Dai, A. & Thorne, P. W. A new approach to homogenize global sub-daily radiosonde temperature data from 1958 to 2018. J. Clim. 34, 1163-1183 (2021).

Zhou, C., He, Y. & Wang, K. On the suitability of current atmospheric reanalyses for regional warming studies over China. Atmos. Chem. Phys. 18, 8113-8136, doi:10.5194/acp-2017-966 (2018).

Zhou, C. & Wang, K. Quantifying the sensitivity of precipitation to the long-term warming trend and interannual-decadal variation of surface air temperature over China. J. Clim. 30, 3687-3703, doi:10.1175/jcli-d-16-0515.1 (2017).