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.

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

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.

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.

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.

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.

Specific comments:

1. Not good to use an abbreviation in Title

2. Why not plot directly the autocorrelation values in Figure 1? The sign of autocorrelation also has scientific meaning.

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?

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’?

5. Line 215, pay more attention to North America and India as comparing RH in Figure 6.

6. Lines 131-132, half is not enough, especially for a short period. I recommend some 80 or 90%.

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).

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