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Comment on acp-2022-149

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Referee comment on "Analysis of global trends of total column water vapour from multiple years of OMI observations" by Christian Borger et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-149-RC1>, 2022

General comments

- On all maps: better to use a Robinson Projection to account for convergence of meridians.

- The goals of this paper are mostly fine. Not so sure about some of the focus on trends when they are not significant! There is a lot of useful information in this paper but also some procedures and results that do not make much sense. Often the description of what was done is not very clear. Many relevant studies have preceded this, and some are referred to. A list of some others that may be of value is appended.

- Our understanding of TCWV is that it varies enormously with weather systems, with seasons, from land to ocean, and from year to year with ENSO. Accordingly, there is

very strong natural variability, especially with phenomena such as ENSO. This is recognized in the appendix, and apparently accounted for? The local trends are often not meaningful because they simply show the phenomenological and related circulation changes. Error bars and uncertainties in trends are not always properly accounted for.

Over the ocean, there is a very strong relationship between SSTs and TCWV, and TCWV and precipitation, especially throughout the Tropics, see Trenberth et al 2005 and Trenberth 2011. It is never fully clear whether results include ENSO or not, or whether it was partly regressed out. It used one index to do the latter, but it is well established that at least 2 indices of ENSO are required to statistically remove ENSO (e.g. Trenberth and Stepaniak 2001). But even then, remnants will remain, and the pattern of trends shown here certainly include ENSO aspects.

Moreover, anything to do with trends should include ENSO because ENSO is part of the climate. Even if ENSO SSTs are not changing, the impacts on precipitation certainly are, and ENSO is the biggest source of droughts around the world. ENSO is part of the system, not external. It would be fine to analyze the ENSO signal separately, but this is not done. It may mean that with ENSO included the interpretation of trends in many places may change?

- The paper finds very little in the way of trends that are significant (Fig 2 c,d) by their tests, but their tests may be overly stringent. Given the usefulness of the dataset, it is perhaps unfortunate they chose to focus on local trends. See below.
- The issues are compounded when they analyse relative humidity involving large assumptions. The findings of changes in rh and links to precipitation are not surprising though (see Trenberth 2011). However, on land, water availability comes into play.
- L 223 on: This is mostly not correct, see Trenberth et al 2003 and Trenberth 2011, to properly account for changes in frequency and intensity, as well as amounts of precipitation. CC relates to saturation specific humidity not actual specific humidity, and one expects big differences between land and ocean. It therefore makes no sense to average globally for this and it matters how this is done. Computing relationships over land and ocean separately and then averaging (area weighting) will give different results than averaging over both land and ocean first. Also over land, it is far from clear that winter (with snow and ice) should be combined with summer.

Moreover, there are important differences between SST and air temperature that greatly affect these results. ERA5 has surface temperatures that would be compatible with the TCWV and surface relative humidity, but this is unlikely when a different temperature analysis (Berkeley) is introduced.

It is not clear what is in Fig. 7. What is the % of? Fig. 7 should be redone. L 243-244 suggests these results are flawed.

L 260-270 and Table 1: This is very unclear, and it makes no sense to compute trends in these quantities in this way. Is ENSO included? It should be. One can compute TUT at various points and examine changes. But Table 1 makes no sense other than to say the result depends on the method.

Some detailed comments

L 22: The equation deals with the saturated water vapour, not just water vapour.

L 35-40: Wentz (2015) gives an excellent analysis of TCWV observations to that point (2015).

L 48: the slowdown terminated in 2014.

L 51: This assumption is only evoked at the surface, it clearly does not apply in the free atmosphere, e.g., where subsidence warms and dries the air.

L 53 also Fasullo 2011; Simmons et al 2010.

L 90 to 114: the accounting for persistence is not quite right or unclear. It seems a reasonable attempt though, but some rewording is warranted.

The formula in l 91 is for an AR1 process only. However, a time series with a trend will feature a strong autocorrelation at lag 1 (and 2 and 3...) In computing the AR1 value one must first remove the trend; or properly account for the higher order AR values (see Trenberth 1984). Is this what the term "residuals" means on l 97 and 107? So, the AR1 value is from N_t ? ENSO also introduces persistence. In addition, the analysis assumes the variance is stationary, but this is not true because of the seasons: very different in wet vs dry seasons.

L 116 should refer to the residuals not the total fields?

The criterion used for significance in Fig. 2c, d was 5% (line 143). It may be too harsh. The latter recognizes the spatial autocorrelation (Fig. B1) and does not take advantage of it. Line 144 and appendix B are likely misleading. L 343-4 should instead take advantage of spatial coherency to area average and remove small scale noise thereby improving signal to noise – e.g., use of 5° instead of 1° squares. Or one could lower the significance level to 10%?

L 137-8: the overall pattern of change is one that surely aliases ENSO to some degree (the coherence of the SPCZ and ITCZ changes), see Fig, A1. Removal of ENSO should use at least 2 indices. However, ENSO is real, and changes in humidity and precipitation with ENSO are also a climate signal (one expects larger values for same index value).

L 156: Given the lack of significant trends in Fig. 2, why is there a focus on trends now?

The comparison between Figs 3 and 4 highlights the dependence on data period.

L 177: section 4 should refer to Simmons et al. (2010) and Fasullo (2011). Land vs ocean must be better recognized.

L 201-214 should account for above studies and also changes in salinity, which better deals with the DDWW aspects: Cheng et al 2020.

L 220-220: the discussion related to Fig 6, needs to better account for the changes in SSTs, see Trenberth 2011.

L 223 on: This is mostly not correct, see Trenberth et al 2003 and Trenberth 2011, to properly account for changes in frequency and intensity, as well as amounts of precipitation. CC relates to saturation specific humidity not actual specific humidity, and one expects big differences between land and ocean because of water availability. It makes little sense to average globally for this. Moreover, there are important differences between SST and air temperature that greatly affect these results. It is not clear what is in Fig. 7? What is the % of? Also over land, it is far from clear that winter (with snow and ice) should be combined with summer. Fig. 7 should perhaps be redone. L 243-244 suggests these results are flawed?

L 245: The residence time is a reasonable concept and relates to the amount vs flux out.

L 260-270: Table 1. What are T trends here: not 0.02: has to be 0.02 per year? Same for all here: per year? There are no error bars on any of these estimates; for instance, global precipitation trends are not significant. The main precipitation fluctuations are associated with ENSO. There also remain uncertainties in precipitation (e.g. Prat et al. 2021) – and several other papers in same issue. It would be better for most readers to see the values of TUT, not the trends. TUT trends in % make no sense. It is also not clear why global temperatures enter this discussion. Suggest the authors focus more on the actual values instead of uncertain trends, although decadal changes may warrant mention?

Similarly in Fig D1, no errors bars are included or areas where trends are not significant indicated.

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Fasullo, J. 2011. A mechanism for land–ocean contrasts in global monsoon trends in a warming climate. *Clim Dyn*. DOI 10.1007/s00382-011-1270-3

Excerpt from abstract

"A feedback mechanism is proposed rooted in the facts that land areas warm disproportionately relative to ocean, and onshore flow is the chief source of monsoonal moisture. Reductions in lower tropospheric relative humidity over land domains are therefore inevitable and these have direct consequences for the monsoonal convective environment including an increase in the lifting condensation level and a shift in the distribution of convection generally towards less frequent and potentially more intense events."

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