We would like to thank Kevin Trenberth for his constructive and detailed review, which helped us to identify several shortcomings in our manuscript. 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.

General comments

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

We changed the projection of all world maps to Robinson-like ("Equal Earth") projection.

2. 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.

We regret that in some places we have not explained our procedure in a precise or comprehensible way and will remedy this in the revised version. Furthermore, we will focus on the statistically significant trends in the revised version.

3. 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.

In fact, we take the effect of ENSO into account in all time series analyses in our manuscript (see also the term E in equation (4) in the discussion paper). In the revised manuscript, we also include an additional ENSO related index (TNI) and the PMM SST index in the trend analysis. In Appendix A in the discussion paper we only show how trends would look like if ENSO was not taken into account. In the revised manuscript we will move these figures into the main text and compare trends accounting and not accounting for any teleconnections along with the following text:
To highlight the influence of teleconnections on the trend results for the OMI TCWV data set, we also perform the trend analysis not accounting for them. The resulting trends and their difference are shown in Figure 3. While overall the spatial distributions of the relative trends (Fig.3a & b) look quite similar, distinct patterns emerge when looking at the trend difference Fig.3c): for instance the typical PMM and ENSO teleconnection patterns are clearly visible (e.g. dipole structure over the maritime continent in the case of ENSO). Consequently, the resulting deviations are particularly strong in the tropical and subtropical Pacific and can reach values as high as the relative trends themselves.
While we agree that local trends are affected by changes in dynamical processes, we do not find that they are not meaningful, as they can also give us an insight, albeit to be judged with caution, into the spatial distribution of changes.
We added the following text at the beginning of Section 3:

Moreover, when investigating climatological trends of TCWV on local scale, these are also influenced by changes in atmospheric dynamics and should therefore be judged with caution. Nevertheless, they can still provide us information about changes of the large-scale TCWV distribution.

Regarding the error bars and uncertainties of the trends, we think that the significant trends provide the clearest picture of changes in TCWV. However, we also believe that the occurrence of non-significant trends is also important information, as in many cases the non-significant trends have values close to zero, showing that the TCWV distribution and magnitude have changed only slightly or not at all on a long time scale. However, for the comparison of trends from different TCWV datasets, we will mainly focus on the significant trends after applying a Z and FDR test.

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?

As mentioned above, we did not communicate clearly enough that we consider ENSO via the ONI index in our analysis. Nevertheless, we would like to thank the referee for pointing out that 2 ENSO indices should be considered in a time series analysis. We have therefore added the TNI index to our fit in addition to the ONI index and evaluated all time series once again. In addition, we tried other indices and found that the PMM SST index leads to a significant reduction in the autocorrelation of the noise in the North Pacific.

In our first analysis we did not take into account that the teleconnection indices are also subject to trends or are detrended over other time periods than ours. Therefore, we have now explicitly detrended each index again for our time period and then used these in the TCWV trend analysis.

In the revised manuscript, we added:

To account for the influence of teleconnections we include several teleconnection indices Ω in the trend analysis. For the case of ENSO, we include the NOAA Oceanic Niño Index (ONI) which according to Wagner et al. (2021) has the strongest impact on the TCWV time series distribution. Moreover, we follow the recommendations from Trenberth and Stepaniak (2001) and include a second ENSO index. In our case we apply the Trans-Niño Index (TNI; Trenberth and Stepaniak, 2001). Furthermore, we investigated the influence of several other teleconnection indices and found that the Pacific Meridional Mode (PMM) sea surface temperature index (Chiang and Vimont, 2004) has a particularly strong influence on the autocorrelation of the noise in the Pacific Ocean. Typically, trends are already removed from teleconnection indices. However, since the time series of the indices cover several decades, the detrending is optimised for this large time period. Accordingly, we have detrended the indices again for our chosen time period (2005-2020).

Overall, the consideration of TNI and PMM index as well as detrending lead to a significant reduction of TCWV trends.

4. 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.

In our significance analysis, we have tried to work as statistically "correct" as possible and avoid overstating our results, and thus also to take into account effects that are often overlooked (e.g. spatial correlation and field significance). Although this limits the amount of significant results, we can be sure that our (trend) results are highly trustworthy.

5. 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.

Following the comments about the land-ocean contrast below, we have added the following text in the section:

The reduction in relative RH over land is likely related to marked land-ocean contrast in warming, (besides various local factors such as changes in vegetation cover) (Simmons et al., 2010; Fasullo 2012): Over ocean, due to the direct link with sea surface temperature, the water vapour content can increase adequately to keep RH constant. Over land, this is usually only possible with a delay due to limited water availability, as water must first be transported there from the ocean. Since the temperature also increases much more over land than over the ocean, the decrease in RH might be due to the lack of an increased water supply from the ocean (Simmons et al., 2010).

6. 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.

We agree that this section is too vague. In the revised manuscript we removed this section.

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.

As mentioned above, this section was removed in the revised manuscript.

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.

As mentioned above, the corresponding section is removed in the revised manuscript. Nevertheless, in Figure 7 we show the relative TCWV response to temperature changes (% / K), which we have determined from the relative TCWV trends (% / year) and the temperature trends (K / year):

TCWV Response = rel. TCWV trend / temperature trend.

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.

As mentioned above, ENSO is taken into account. We followed the suggestion and calculated the TUT trends also on a local scale. Thus, we completely revised this section.

The new section is as follows:

Another key diagnostic of the hydrological cycle is the atmospheric water vapour residence time (WVRT). The WVRT can help to better understand changes in dynamic and thermodynamic processes within a changing climate (Trenberth, 1998; Gimeno et al., 2021): for instance, an increase in WVRT suggests that the length of the atmospheric moisture transport increases, i.e. the distance between moisture sink and source regions (Singh et al., 2016). Several different metrics exist for quantifying the WVRT (van der Ent and Tuinenburg, 2017; Gimeno et al., 2021), bearing in mind that the WVRT distribution or the lifetime distribution (LTD) is exponential on local scale and thus the mean value is strongly influenced by a few high values (van der Ent and Tuinenburg, 2017; Sodemann, 2020). Ideally, one would determine the LTD for each grid cell for each month from backward trajectories and then examine their changes or trends. However, this would be well beyond the scope of this paper.

Thus, for our purpose and for the sake of simplicity we focus on the so-called depletion time constant (DTC) and the turnover time (TUT). The TUT describes the global average mean age of precipitation and can be calculated as the ratio of TCWV to precipitation P:

$$TUT = \frac{\overline{TCWV}}{\overline{P}}$$

where the bar indicates global average. Typically, the TUT varies between values of 8 to 10 days and is expected to increase by 3–6% K^{-1} (Gimeno et al., 2021, and references therein). Analogously, the DTC is defined as the local ratio of TCWV to precipitation (e.g. Trenberth, 1998):

$$DTC = \frac{TCWV}{P}$$

The DTC values might vary substantially from TUT, but the global precipitation weighted average is equal to TUT (Gimeno et al., 2021).

For our investigations of trends in DTC we combine the regridded GPCP data set from Sect. 4 and the OMI TCWV data set and perform the trend analysis scheme from Sect. 2.2 to the monthly DTC values for the time range 2005 to 2020. To ensure numerical stability, we only consider monthly rain rates greater than 0.25mm d^{-1}. As a result, large parts of the subtropical oceans and deserts are excluded from the analysis.

The results of the DTC trend analyses are depicted in Figure 8. On average, we typically obtain mean DTC values between 5-10 days in the areas where rain occurs (Fig. 8a). In the subtropical dry zones, values of around 30 days and well above are found. In terms of absolute DTC trends, the most striking patterns are in the northern subtropical Atlantic with strong increases and in the northern subtropical western Pacific with strong decreases. In comparison, the distribution of relative DTC trends is much spottier, but overall, in addition to the patterns already mentioned, we obtain distinctive increases in DTC on the US west coast, in Europe, Russia and in the eastern Pacific.

For our investigations of trends in TUT we first calculate global averages of the regridded GPCP data set from Sect. 4 and the OMI and ERA5 TCWV data sets between 60°S and 60°N

for each month, then combine the time series of global averages, and finally perform the trend analysis for the TUT time series for the time range from 2005 to 2020.
Altogether, we find an increase in the global TUT for OMI and ERA5 of approximately +0.02d y^{-1} with TUT mean values of around 9.7d for OMI and 8.8d for ERA5. Combining the long-term relative trends in TUT and trends in surface air temperature, we can estimate the sensitivity of TUT to global warming r, i.e. $r = \frac{\Delta TUT}{TUT} / \Delta T$. For the case of OMI and Berkeley Earth, we find a TUT sensitivity of around 8.4%K^{-1} and for ERA5 of around 8.8%K^{-1} which is higher than the results of 3–6% K^{-1} pooled in Gimeno et al. (2021).

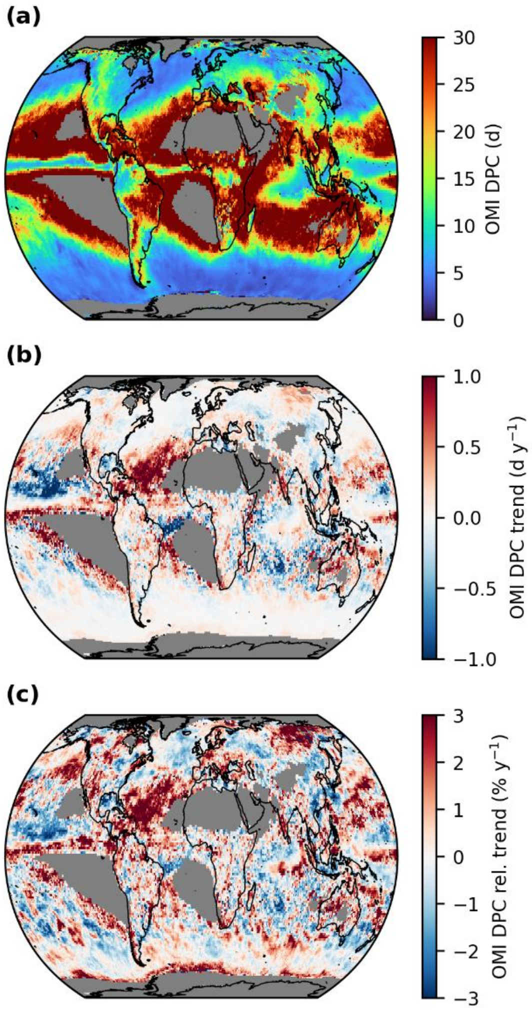

Figure 8. Global distribution of DTC trends for the time range January 2005 to December 2020. Panel (a) depicts the distribution of the mean DTC. Panels (b) and (c) depict the absolute and relative DTC trends. Grid cells for which no valid trend has been calculated are coloured grey.

Some detailed comments

L 22: The equation deals with the saturated water vapour, not just water vapour.
We added “saturated” to the sentence.

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

We added a reference to Wentz (2015).

L 48: the slowdown terminated in 2014.
We added that 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.
We reformulated the sentence and explicitly mentioned that this assumption is only valid close to the surface:
[Typically, it is assumed that relative humidity] close to the surface […]

L 53 also Fasullo 2011; Simmons et al 2010.
We added the suggested references.

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.
Exactly, the word "residuals" is meant to make clear that the autocorrelation used here is not that of TCWV, but of the fit "noise" or the fit "residuals" Nt. In this way, they are determined from the fit, in which the trend, seasonal components and ENSO are also taken into account. We also tested different AR processes (with lag=2, 3, 6, 12) and found only minor differences between the trend results.
In fact, the assumption of stationary variance is a limitation that could possibly be overcome by ARMA/ARIMA processes. However, the transformation of the linear equation system of the fit into this ARMA/ARIMA system is highly non-trivial (especially for unevenly spaced time series) although in the case of ARMA it is possible if the lag-1 and lag-2 coefficients of the autocorrelation function have the same sign (see e.g. Foster and Rahmstorf, 2011). However, this is not always fulfilled in our case.
Regarding the AR-model choice, we added the following text:
One limitation of the AR-model is the assumption of stationarity of the variance. Although this limitation can be overcome by using ARMA (auto-regressive moving average) or ARIMA (auto-regressive integrated moving average) processes, the determination and application of these models (for example in the transformation of the linear equation system of the fit function) is highly non-trivial, especially for the case of unevenly spaced time series. Although an ARMA(1,1) process would be possible in the case that the lag-1 and lag-2 coefficients of the autocorrelation function have the same sign (e.g. Forster and Rahmstorf, 2011), this condition is not always given in our case. Thus, we have decided to stay with the AR(1) process.
[…]
We have also tested other AR-models with lag=2, 3, 6, and 12 and found that the trend results differ only slightly from those using an AR(1) model. The corresponding trend results and the difference to the trends with the AR(1) model can be found in the supplement.

L 116 should refer to the residuals not the total fields?
Yes, this should actually refer to the residuals / "noise". We changed the sentence as follows: *... global distribution of the lag-1 autocorrelation coefficients of the fit residuals or fit noise.*

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%?
We have taken up the idea of Reviewer 2 and now show all trends, significant trends (after Z-test) and "filtered" significant trends (after Z- and FDR-test) in Figure 2 in the paper (and Fig.1 in this review). Overall, a lot of trends are significant at 5%, so we do not think that our criteria are too strict (see Fig. 1c and 1d). Regarding 5° vs. 1° resolution, we found almost no differences in the distribution of significant trends, but obviously some information is lost due to the poorer resolution (compare Fig.1 vs. Fig.2). Therefore, we continue to stick with 1° x 1° resolution.

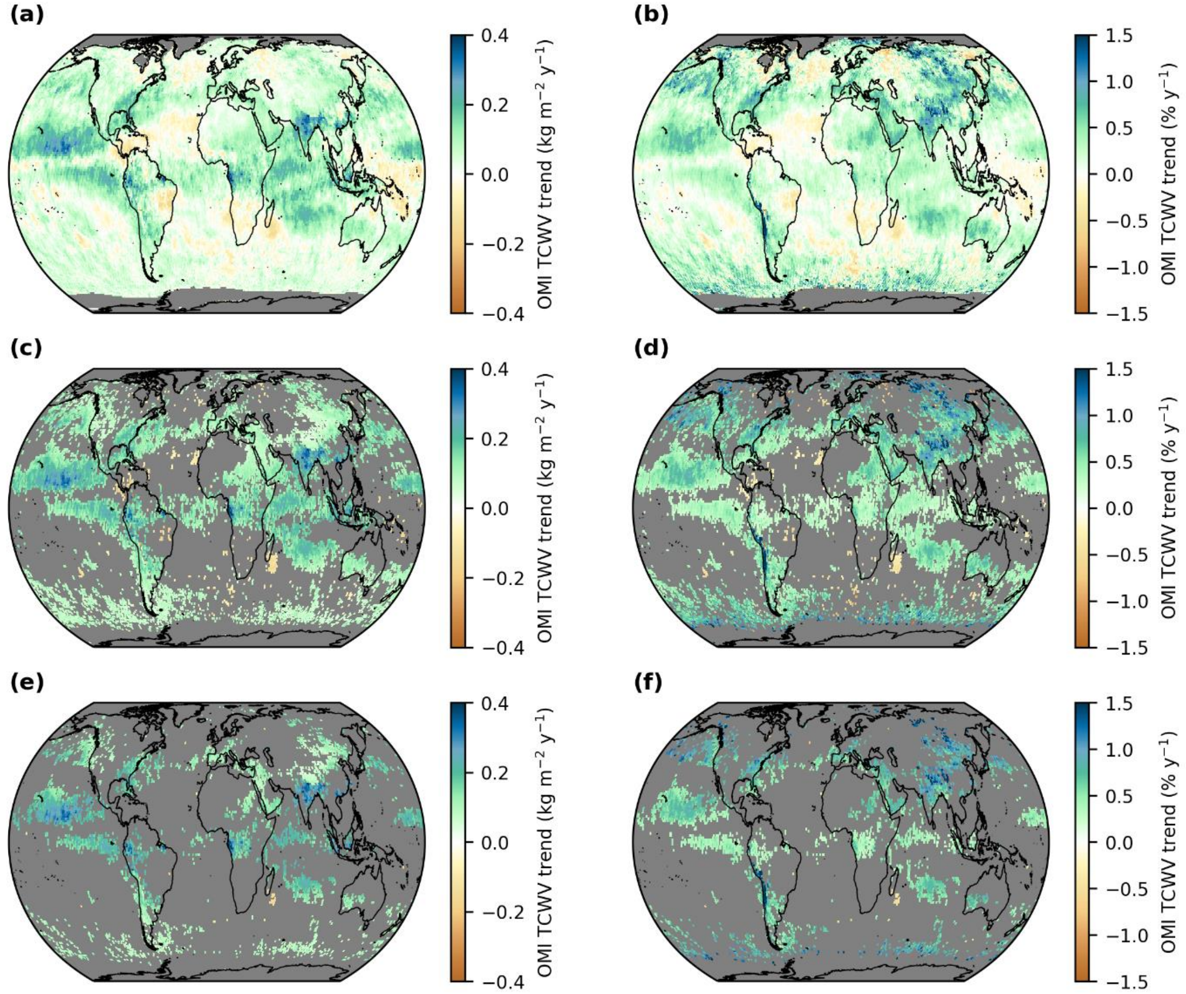

Fig.1: Global distribution of TCWV trends derived from the MPIC OMI TCWV data set. Panels (a) and (b) depict the calculated absolute and relative TCWV trends, respectively. Panels (c) and (d) depict the remaining significant trends after the application of the Z-test. Panels (e) and (f) depicts the remaining significant trends after the application of the Z-test and the FDR test.

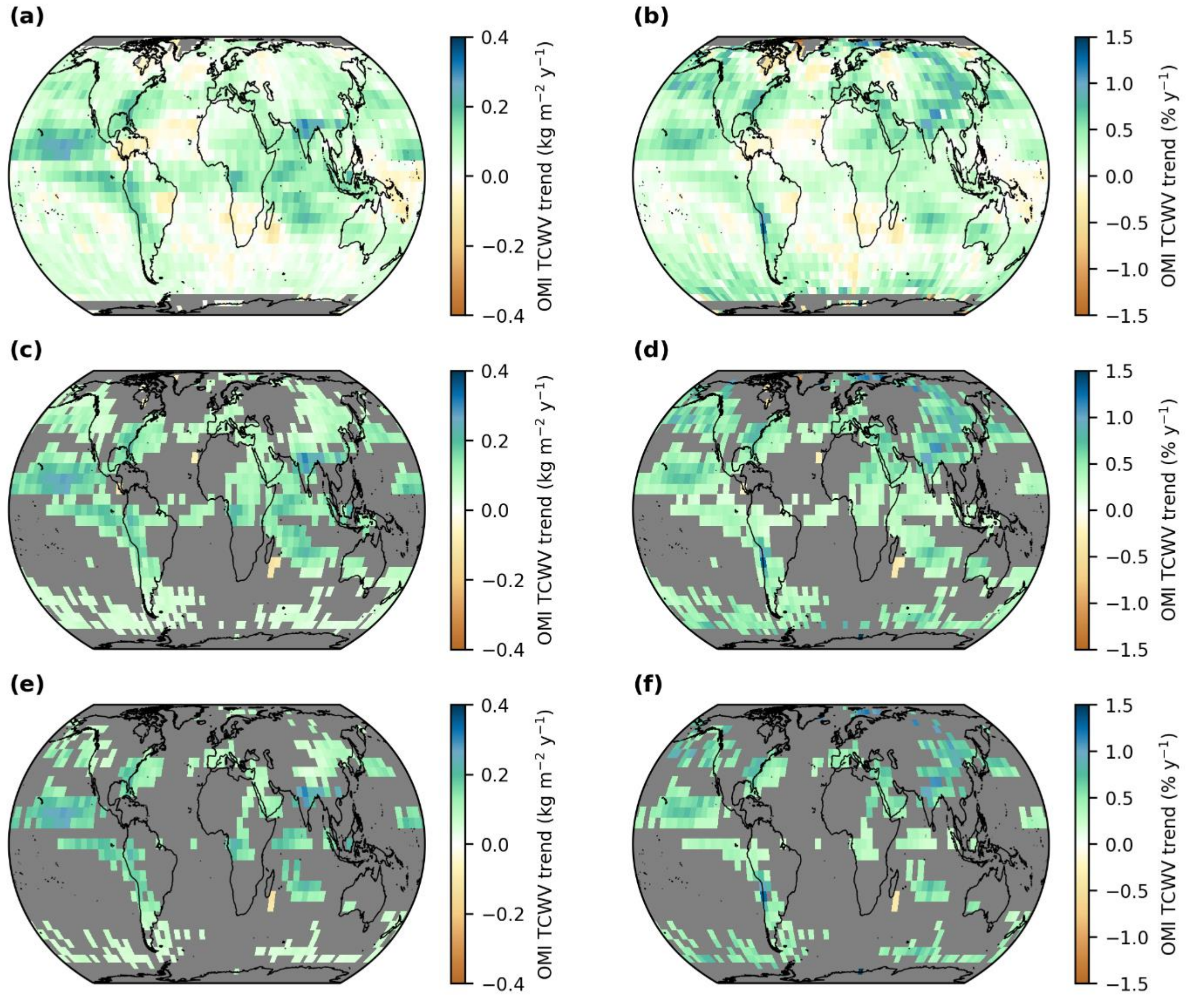

Fig.2: Same as Fig.1, but with 5° x 5° resolution.

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).

We followed your recommendations and included 2 (detrended) ENSO indices (ONI & TNI) as well as the PMM SST index in the new analysis. But since "ENSO is real and changes … with ENSO are also a climate signal", we also include trends on the actual measurements without including ENSO indices in the trend analysis.

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.

As we work as statistically cleanly as possible (and thereby avoid overinterpreting irrelevant or insignificant trends) we consider the remaining significant trends to be real and worth reporting. Actual numbers of course depend on the considered data period, and of course the trend results become more meaningful for longer time periods. Thus, we make use of our new H2O product for OMI, which provides the longest single-instrument time series for this type of measurements. Hence, we are confident that – in spite of all limitations – showing trends is an important information for the reader.

L 177: section 4 should refer to Simmons et al. (2010) and Fasullo (2011). Land vs ocean must be better recognized.
We incorporated the papers by Fasullo (2011) and Simmons et al. (2010) (see comment above).

L 201-214 should account for above studies and also changes in salinity, which better deals with the DDWW aspects: Cheng et al 2020.
We added the following text:
In addition, other studies show that changes in precipitation correlate very well with changes in ocean salinity, suggesting a "fresh gets fresher, salty gets saltier" pattern (Cheng et al., 2020, and references therein).

L 220-220: the discussion related to Fig 6, needs to better account for the changes in SSTs, see Trenberth 2011.
We added the following text:
Trenberth (2011) and Trenberth and Shea (2005) analysed local correlations between precipitation and surface temperature for cold and warm seasons and reported mainly positive correlations over ocean and negative correlations year round over land throughout the tropics. However, over ocean the correlations also depend on whether the (sea) surface temperature is driven by the ocean or by the atmosphere (Trenberth and Shea, 2005). While in some regions of the subtropics we can also find this high correlation in the trend patterns of precipitation and surface temperature (e.g. increase in precipitation in the northern subtropics in the eastern Pacific or decrease in the subtropical Atlantic over ocean; decrease in Brazil or South Africa over land), we cannot find a direct link for the striking negative precipitation trends in the equatorial Pacific. However, it should also be taken into account that a large part of the precipitation trends are not statistically significant.

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?
The corresponding section has been removed in the revised manuscript.

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?

We have revised the TUT section and now distinguish between local trends in the residence time (more precisely the depletion time constant) and trends in the globally averaged residence time (TUT).

In addition, we addressed the uncertainties in the rainfall data sets. According to Prat et al. (2021), however, "For accumulation periods greater than the day (i.e., week, month, years), all SPPs perform satisfyingly, which makes them suitable for long term hydro-logical and hydroclimatological applications." However, it should also be taken into account that Prat et al. (2021) used GPCPv2, whereas we use the latest GPCP version (v3.2).

Although precipitation climate data records (CDRs) allow a global analysis, they are subject to large uncertainties, as satellite and rain-gauge observations do not have good spatio-temporal coverage, weak and short rain events are not well detected or even missed and satellite retrievals can determine the rain rate only indirectly. Thus, deviations of about 50% in the daily rain rate can occur compared to in situ measurements (e.g. Prat et al., 2021). Nevertheless, Prat et al. (2021) show that over accumulation periods of month or years, precipitation CDRs perform satisfactorily. Moreover, Prat et al. (2021) used an older GPCP version (v2) than ours in their evaluation study.

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

We have revised figure D1 and now added the maps of significant trends.

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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.”

Prat, O. P., et al. 2021: Global Evaluation of Gridded Satellite Precipitation Products from the NOAA Climate Data Record Program. J Hydromet. 22, 2291-2310.

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