Dear Editor,

We appreciate the prompt review and would like to thank the three Reviewers' perceptive and helpful comments and suggestions on our manuscript entitled "Observed Trends of Clouds and Precipitation (1983–2009): Implications for Their Cause(s)", Author(s): Xiang Zhong et al., MS No.: acp-2020-577, MS type: Research article. We have carefully considered all comments and suggestions and carried out major revisions as suggested. We believe that the revisions have resulted in a significantly improvement of the paper. Listed below are point-by-point responses to all comments and suggestions of the three reviewers (Reviewer's points in black, our responses in blue).

Anonymous Referee #2

The main focus of this paper is establishing the role of global warming, AMO, and PDO in the spatial pattern of global cloud and precipitation trends (based on global satellite records). Cloud cover and precipitation trends from Chinese meteorological stations are also examined. Unfortunately, I find a number of major flaws in this paper and do not believe that it meets the quality for publication in ACP at this time: 1) There is a lot of overlap with recent papers that have performed similar analyses, and I struggle to see how this paper provides a substantial new contribution to the peer reviewed literature. Figure 1a is nearly identical to Figure 1a in Norris et al. (2016), the PDO/AMO analysis is similar to that in Chen et al. (2019), and Adler et al. (2017) already examine contributions of the PDO and AMO to global precipitation trends. Adler, R.F., Gu, G., Sapiano, M. et al. Global Precipitation: Means, Variations and Trends During the Satellite Era (1979–2014). Surv Geophys 38, 679–699 (2017). https://doi.org/10.1007/s10712-017-9416-4 2)

We agree with the criticism that there are already numerous studies on our subject of study. However, as stated in our introduction, there is hardly any agreement on the quantitative roles of global warming, AMO, and PDO in the spatial pattern of global cloud and precipitation trends. Moreover, there are very few studies utilizing both cloud and precipitation data sets. Last but not the least, with a lot of help of all three referees' comments, we believe that in the revised manuscript we have made "a substantial new contribution" in the conclusion below: Further analysis of the widening of the Hadley and Walker circulations (Figures 2a-2h, see Response to referee 3) shows that the trend in global temperature, rather than those of AMO and PDO, is the primary contributor to the observed linear trends of total cloud cover and precipitation in 1983–2009. The underlying mechanism driving this widening is proposed to be the moisture–convection–latent heat feedback cycle under global temperature conditions.

How reliable are the trends in the satellite data products? While the authors use the corrected data set of Norris and Evan (2015) to account for some of these issues in the ISCCP data, no mention is made of the reliability of the trends in the GPCP precipitation data set (line 91). Also, no discussion is provided of the role that potential instrumentation/reporting method changes may play in the trends from the Chinese meteorological stations.

This point is well taken. In our study, the reliability of data products is mainly concerned with the precision rather than the absolute accuracy of the data. So comparison of different instruments are usually used to evaluate the reliability of the trends in the ISCCP data or GPCP precipitation data set. For example, Xie et al. (2003) found that good agreement is observed between the pentad GPCP and the gauge-based dataset of Shi et al. (2001) over the combined space–time domain. The correlation is 0.776, 0.660, and 0.688, respectively, for the total value, anomaly, and intraseasonal components of the pentad precipitation. These results imply the reliability of the GPCP pentad data is on the order of 70%, or uncertainty of 30%. For the ISCCP data set Norris and Even (2015) found that the root-mean-square difference between ISCCP and PATMOS-x grid box trends decreases from 2.0% (the amount per decade for the original data) to 0.9% (the amount per decade for the fully corrected data). Disagreement between ISCCP and PATMOS-x cloud trends may be due to differing satellite instruments and methods of cloud retrieval or remaining artifacts in the datasets.

We have made extensive comparisons of the ISCCP data and the GPCP precipitation data with corresponding data at the surface stations in China. In many cases, correlations of better than 0.7 were observed, particularly for precipitation data. Therefore, we believe that the correlation results of 0.7 or better are reliable in this study.

Shi, W., R. W. Higgins, E. Yarosh, and V. E. Kousky, cited 2001: The annual cycle and variability of precipitation in Brazil. NCEP/Climate Prediction Center Atlas, No. 9, National Oceanic and Atmospheric Administration. National Weather Service. [Available online at http://www.cpc.noaa.gov/researchppapers/ ncep_cpc_atlas/9/index.html.]

3) Trends in cloud cover and precipitation are attributed to global warming, AMO, and PDO over the 1983-2009 period, yet this is a very short interval for isolating signatures from decadal modes of variability. Additionally, all three of these indices (global temperature, AMO, and PDO) experience trends over this period. So, is this period even long enough to attempt an analysis like this, because it's less than one full oscillation for the PDO and AMO? How do you have enough degrees of freedom to accurately identify the pattern of cloud and precipitation anomalies associated with the PDO and AMO and distinctly separate it from the global warming trend contribution? And, just because global temperatures are warming, it doesn't mean that concurrent trends in clouds and precipitation are necessarily caused by global warming.

Thank you for a highly significant criticism. From a different angle, the other two referees have raised the same concerns. In our response to referee 3, we now have revised the manuscript by adding a quantitative evaluation of the primary tropical widening over the Maritime Continent. Shown in Figures 2b-2d in our response to referee 3 are the changes (blue curve) from the climatology (1983–2009) (black curve) in the annual total precipitation of the 16 belts of Figure 1 (response to referee 3) as a function of global temperature (GT), AMO and PDO, respectively. The formula for calculating the blue curve, for instance for the change in precipitation as a function of global temperature (Figure 2b), is d(TP)/d(GT)*ΔGT, where ΔGT denotes difference in the global temperature between 1983 and 2009. It can be seen that Figure 2b (GT) agrees very well with Figure 2a both qualitatively and quantitatively; while Figures 2c and 2d have significantly greater positive values (significant widening) compared to the small negative values (contraction) of Figure 2a for the inner 5 belts, resulting in a significant enhancement of the overall precipitation. This discrepancy is crucial, as the global total annual precipitation, which is equal to global evaporation and determined by the global surface energy budget, increases with global temperature at a rather small rate of about 2%–3% K^{-1} (Cubasch et al., 2001). Therefore, based on the results of Figs. 3a-3d, we propose that the trend in global temperature, rather than those of AMO and PDO, is the primary contributor to the observed linear trend of precipitation in 1983–2009. Similarly, it can be seen that Figure 2f agrees with Figure 2e significantly better than Figures 2g and 2h, such that the trend in global temperature, rather than those of AMO and PDO, can be proposed to be the primary contributor to the observed linear trend of total cloud cover in 1983–2009.

The similarity in Figs. 1 and 3 is by construction, as the global temperature time series is dominated by an increasing trend (so any trend in clouds and precipitation will by definition be highly correlated with global temperature). It would be better to define Figure 3 using a detrended global temperature timeseries (as Reviewer #3 also suggests). Another related concern is a lack of independence of the global temperature, AMO, and PDO indices (because they all have trends over the 1983-2009 interval).

Thanks, you are right! In our response to the same question by Referee#3, we have re-evaluated Table 1 using detrended data of TCC, TP, GT, AMO, PDO and Niño3.4 (Table S1 in response to referee 3). The correlation coefficients are all less than 0.33, implying that consecutive yearly variabilities contribute insignificantly to the high correlation coefficients in Table 1, and the high correlation coefficients are nearly entirely contributed by the long-term linear trends of GT on PDO and AMO. One of the reasons for the lack of correlation in the detrended data could be due to the small ratio between the consecutive yearly variabilities and the long-term linear trends (about 0.1) for GT, PDO or AMO (Figure S4).

How can the global warming trend explain 67% of the variance in the global cloud cover trends and the AMO trend explain 49% (line 158)? You can't explain more than 100% of the variance, unless the indices are not independent of one another. In other words, it doesn't appear that the global warming, PDO, and AMO indices are actually orthogonal to one another (as is claimed on lines 166-167).

We agree there is a problem of explaining more than 100% of the variance. We didn't try to hide the problem, as we stated in the original manuscript: "PDO together with AMO and GT, obviously has a problem of over 100% explanation of the spatial variabilities of linear trends in cloud cover and precipitation. Since the trend of global SST has been removed from the PDO and AMO indexes in this study, in theory GT should be orthogonal to those of PDO and AMO." In practice the orthogonality is not attained because the trend of global SST doesn't equal to the real influence of global temperature on PDO or AMO. It is difficult to remove exactly the influence of global temperature from PDO or AMO index. This is likely the main reason of the problem of over 100% explanation.

4) The authors are examining cloud and precipitation features in the deep tropics and attributing them to a poleward shift in the Hadley cell edge and midlatitude jet streams (lines 131-132, 138-140). The expansion of the Hadley cell and poleward shift of the jet streams affects precipitation in the subtropics and midlatitudes (poleward of 30 degrees latitude), not in the deep tropics. For tropical precipitation changes, the authors need to really be comparing their results with recent changes in the ascending branch of the Hadley cell (Intertropical Convergence Zone), not the descending branch in the subtropics.

Thanks for an excellent point! In the Figure 2e (in our response to referee 3), one can see that the expansion of the Hadley cell as measured by clouds starts at belt 2 (3.75^{o} latitude), i.e. the blue curve starts to move to the right of the black curve near 3.75^{o} latitude. This is near the center of the ascending branch of the Hadley cell in the Maritime Continent. The expansion of the Hadley cell as measured by precipitation

(Figure 2a) starts near belt 5 (12.5º latitude). This is likely due to the constraint on the change of global total annual precipitation, which is equal to global evaporation and determined by the global surface energy budget, increases with global temperature at a rather small rate of about 2%–3% K^{-1} (Cubasch et al., 2001).

5) Section 3b seems like a separate study and to not be related to the rest of the paper. Trends in a small region are not necessarily affected by global drivers, and regional influences are not discussed at all. This data analysis also suffers from similar problems as the global analyses in section 3a (see major comments #2 and #3).

All three referees raised this important concern. We have made changes in both the abstract and the beginning of section 3.2 to better connect the global part and the analysis of data in China (see below). Moreover, we now have established a more consistent results for the two parts.

The new addition to section 3.2 is: The global analysis is extended by investigating connections between clouds and precipitation in China, which has a large number of long-running, high-quality surface weather stations over the period of 1957–2005. The long-running data enable the analysis to be carried out over a period that AMO loses while PDO flips its linear trend. More importantly, the high-quality data allow us to make a meaningful analysis without using the correlation method, which has an intrinsic weakness in implying a cause-effect relationship as discussed above.

The revision to the abstract on this issue is: The global analysis is extended by investigating connections between clouds and precipitation in China, which has a large number of long-running, high-quality surface weather stations in 1957–2005, which reveals a quantitative matching relationship between the reduction in light precipitation and the reduction of total cloud cover. Furthermore, our study suggests that the reduction of cloud cover in China is primarily driven by the global temperature conditions, PDO plays a secondary role, while the contribution from AMO and Niño3.4 is insignificant, consistent with the global analysis.

Minor Revisions Lines 20-29: The trends described in this paragraph do not appear to closely match those shown in Norris et al. (2016), especially over land and over the Indian Ocean.

We are confused by this comment. We checked and compared Figure 1 in Norris et al. (2016) with our Figure 1, they are very consistent.

Lines 54-71: Somewhere in this paragraph, it is probably worth mentioning that the constraint on global precipitation is 2–3% per K, and not 7% per K. See, for example, Jeevanjee and Romps (2018; https://doi.org/10.1073/pnas.1720683115).

Agree, this is now added in two places. One is in the 3rd paragraph of section 3.1, the other in the 7th paragraph of the same section.

Line 69, 131-132, 138-140: See major comment #4. The expansion of the Hadley cells has nothing to do with enhancement of tropical precipitation. It is related to subtropical static stability (Chemke and Polvani 2019: https://doi.org/10.1175/JCLI-D-18-0330.1). If anything, an expansion of tropical precipitation would contradict the literature, which suggests a narrowing of the Intertropical Convergence Zone in a warming climate (Byrne and Schneider 2016: https://doi.org/10.1002/2016GL070396; Su et al. 2017: https://www.nature.com/articles/ncomms15771).

We understand this is a controversial point. Please see our response to your major comment #4.

Line 160: The figure for the PDO really belongs in the main body of the paper, as it is part of the main conclusions of the paper (see abstract).

Thanks, we now have two figures (Figures 2d and 2h in our response to referee 3, i.e. Figures 3d and 3h in our revised manuscript) for the PDO.

Line 187: No, the key difference here is that Chen et al. (2019) use the first 300 years of control model simulations to define the cloud cover patterns associated with the PDO and AMO, which avoids the issues of concurrent trends in the indices using the observations (see major comment #3 above).

We disagree on this point, because we question the credibility of climate models in the simulation of changes in clouds and precipitation as a function of AMO or PDO.

Lines 189-193: Why is the PDO deemed insignificant here? Is this based entirely on Eastman and Warren's analysis? Nothing shown in this paper appears to make the PDO less significant than the AMO (see Table 1).

Please see our response to your major comment#3. The new results on the widening of the Hadley circulation (Figures 2a-2h in our response to referee 3) suggest that the contribution of both PDO and AMO are insignificant compared to the global temperature increase.

Lines 208-210: Could the increase in non-precipitation days and decrease in light precipitation days reflect a change in reporting method? How do you know that these changes are in fact physical?

Trenberth et al. (2003) summarized the global warming hypothesis by explaining that the precipitation intensity of storms should increase at about the same rate as atmospheric moisture, which is about 7% K^{-1} according to the Clausius–Clapeyron equation. The precipitation intensity could even exceed the 7% K^{-1} because additional latent heat released from the increased water vapour could invigorate the storm and pull in more moisture from the boundary layer, forming a positive feedback cycle (i.e. the moisture-convection-latent heat feedback cycle) and leaving less moisture available for light and moderate precipitation.

Lines 237: Difficult to read as written. The equation should be spaced out. Figures: I would suggest inverting the color bar such that blues correspond to more clouds/precipitation and reds correspond to less.

Thanks for the suggestion. After some deliberation we choose to retain the current color bar.

Table 1: How are you evaluating significance? I have a difficult time believing that a correlation of 0.02 is still significant at the 95% confidence level. Are you taking into account autocorrelations among neighboring grid points, which would greatly reduce the number of degrees of freedom in your t-test? Table 2: Similarly, how is significance being evaluated here? A trend of 0% (see T60%) should not be statistically significant at all, especially at the 99% level.

We used the function imbedded in R named corr to do this significance test. The function corr we chose applies Pearson correlation formula:

$$r = \frac{\sum(x - m_x)(y - m_y)}{\sqrt{\sum(x - m_x)^2 \sum(y - m_y)^2}}$$

m_x and m_y are the means of x and y variables.

The p-value of the correlation is determined by calculating the t value as follow:

$$t = \frac{r}{\sqrt{1 - r^2}}\sqrt{n - 2}$$

then using t distribution table for the degrees of freedom: df = n-2 to get the p-value.

We believe even when the correlation coefficient r is very small, due to the big value of n (the number of samples we used in calculation), the t value should remain a very big value, therefore brings a reliable significance.

Typos Line 20: are of great importance

Thanks, changed accordingly.

Line 27: places affiliated to Australia – not sure what this means, please rephrase

Rephrased to “around Australia”.

Line 98: provided by

Changed accordingly.

Line 99: retained

Changed accordingly.

Line 105-106: Incomplete sentence . . . please rewrite.

Rewritten accordingly.

Line 145: is robust

This part is rewritten.

Figure 6a: bottom 10%-40%

Changed accordingly.