Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-577-RC2, 2020

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Observed Trends of Clouds and Precipitation (1983–2009): Implications for Their Cause(s)" *by* Xiang Zhong et al.

Anonymous Referee #2

Received and published: 4 September 2020

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) 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.

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. 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). 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).

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.

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).

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.

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).

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).

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).

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).

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).

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?

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.

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.

Typos

Line 20: are of great importance

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

Line 98: provided by

Line 99: retained

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

Line 145: is robust

Figure 6a: bottom 10%-40%

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-577, 2020.