Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-951-RC1, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of tropical deep convection on interannual variability of tropical tropopause layer water vapor" *by* Hao Ye et al.

Anonymous Referee #1

Received and published: 16 November 2017

Summary:

This paper expands on the regression techniques employed in previous work by one of the coauthors to explore spatial variations in the tropics. The paper seeks to draw some conclusions about the importance of convection in explaining variations in TTL water based on comparing the regressions of water measurements and water calculated from a trajectory model. Comparison with climate model water vapor simulations are also included.

General Comment:

Though the trajectory model physics is greatly oversimplified and clearly NOT state-of-the art, the approach has the potential to be useful. Unfortunately, the authors focus

on only one aspect of the results of their method, namely that convection moistens the TTL. This is certainly true, but has been previously established by a much more detailed and dynamically and microphysically realistic trajectory model (Ueyama et al, 2015).

The paper also shows that much of the spatial variability in the tropics can be explained by the regression variables (BDC, QBO, and tropospheric temperature). This second result is actually quite interesting, but its implications are not explored. For example, Figure 2 (regression with the BD circulation) shows some consistent spatial patterns in the regression coefficient. This is also true for the QBO coefficients (Figure 3). However, only the spatial variations of the tropospheric temperature coefficients are explored (to make the point about convection moistening the TTL). At the very least, there should be some discussion of these patterns (QBO and BDC coefficient patterns) and what they might mean, consistent with the objectives set out in the last paragraph of the introduction. Arguably the concentration of negative BDC coefficients in the western Pacific may not be a surprise, but why the QBO coefficients are strongest in the Indian ocean is puzzling.

To make this paper acceptable for publication, the authors need to do two things (as well as address the specific points below).

(1) As indicated above, the major result that the authors stress is the moistening of the TTL by convection. The authors need to be mindful (and point out clearly) that this is not new. What may be considered new is: (1) moistening in the central Pacific associated with El Nino (the Avery work is only a case study, so the general point is new); and (2) the method (regression) by which this result is arrived at. The authors need to clearly emphasize what is new and what is not.

(2) Explore the other implications of their regression analysis (see second paragraph above). The authors may want to save this for another paper, but ignoring most of the results of their regression analysis is simply unacceptable.

Specific points:

Page 2, Line 10: How good is Dessler's model in evaluating the increase in water vapor in the UTLS due to convection for climate models? I would argue that we really can't simulate convection to the appropriate level of detail to get the effect of convective injection on UTLS water vapor in the current climate, so it is going to be difficult to make forecasts. Dessler' 2016 paper is one of the motivators to looking at this problem, but the statement is too strong. "a significant fraction of this increase may be due to the evaporation of lofted ice..." is more appropriate.

Page 7, lines 12-15: Look at the figure! The BDC coefficients change quite a bit over continental areas, so the statement is wrong. The statement that this is due to instant evaporation needs to be supported by some reasoning or evidence.

Page 7, lines 15-16: Systematic differences are reduced (comparing 4i to 4k), but scatter is much larger. For the BDC coefficient, the agreement (2i and 2k) is actually substantially worse. A region of negative coefficients appears over Indonesia in Figure 4j. I guess that is not statistically significant, but neither are any of the negative regions in figures 4g, 4j, and 4h that are emphasized in showing how convection moistens the central Pacific. There needs to be more discussion of these points.

Page 7, last paragraph: The contents of this paragraph should be moved to the conclusions section. The point about long-term trends of stratospheric water vapor (which this paper does not address at all) is speculation. Speculation is OK, but not in the results section.

Page 7, line 33: How is it shown that a free running climate model GEOSCCM simulates TTL water vapor "over this period?" Since it is NOT a reanalysis, this needs to be explained.

Page 7, Line 4: I would use a study with a more detailed realistic model (e.g., Ueyama et al, 2015) to make this point.

Minor comments:

line 7: ...as THE troposphere warms...

Figure 3: Axes are mislabeled in (c) and (f)

Figure 5 caption: magenta is negative, and black is positive temperature anomalies (reverse of what is in the caption).

The black dots of significance are not always easy to see in the figures, suggest another color.

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