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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.

Hao Ye et al.

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General Comments:

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: (a) moistening in the central Pacific associated with El Nino (the Avery work is only a case study, so the general point is new); and (b) the method (regression) by which this result is arrived at. The authors need to clearly emphasize what is new and what is not.

Response: We have revised the introduction to clarify how our work fits into the previous literature. See page 2, lines 8-14 and lines 20-27.

(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.

Response: In our opinion, the spatial patterns of the QBO and BDC coefficients are not interesting enough to warrant additional discussion. Thus, we have not added anything to the paper in response to this comment. We are open, however, to suggestions from the reviewer about what other implications are noteworthy.

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's 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.

Response: This is covered in some detail in Dessler et al. (2016). Our assessment is that that analysis was able to unambiguously identify the influence of convective ice evaporation in the models. That said, we now note that this analysis only analyzed two models, so we've modified some text to make this clearer: "a significant fraction of this increase was found to be due to the evaporation of convective ice from convection in two chemistry-climate models (Dessler et al., 2016)" (Page 2, lines 16-18).

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.

Response: We agree this was inartfully worded. We changed the statement to "The scatter plot of GEOSCCM vs. traj_ccm_ice BDC coefficients (Fig. 2k) shows larger scatter than the comparison without ice (Fig. 2i). The increase in scatter is likely the result of the crudeness of our microphysical assumptions, particularly the assumption that convective ice evaporates instantaneously. However, the comparison between the tropical average GEOSCCM BDC coefficient, -6.2 $\mathrm{ppmv}\ (\mathrm{K\ day}^{-1})^{-1}$, and those from the trajectory models, -5.8 and -6.9 $\mathrm{ppmv}\ (\mathrm{K\ day}^{-1})^{-1}$ without and with convective ice evaporation, respectively, is similar." (page 7, lines 23-28).

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.

Response: We have re-written this discussion to highlight that the scatter has visibly increased in the coefficient scatter plots when we add ice (page 7, lines 29-34).

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.

Response: Done.

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.

Response: We have modified this statement and added some discussion (See page 3, lines 13-16 and page 9, lines 21-24).

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.

Response: We have re-written this sentence as "In the last section, we hypothesized that this difference in the coefficients were due to evaporation of convective ice in the MLS data, a process not included in the trajectory model" (page 7, lines 14-16).

Minor comments:

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

Response: Added.

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

Response: Corrected.

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.

Response: We have removed the dots from the plot. We did this because what is important in the plots is not whether the coefficients are non-zero, but rather their overall spatial pattern. In its place, we've added a discussion about the significance of differences in the tropical average quantities (page 6, lines 16-20 and page 7, lines 13-14).