Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-302-SC2, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of convectively lofted ice on the seasonal cycle of tropical lower stratospheric water vapor" *by* Xun Wang et al.

Eric Jensen

ericjj50@gmail.com

Received and published: 4 September 2019

[titlepage,12pt]article

natbib graphicx indentfirst times color

Second comment on "Impact of convectively lofted ice on the seasonal cycle of tropical lower stratospheric water vapor" by X. Wang et al.

In their response (AC3) to my first comment, the authors have partially addressed some of my concerns. However, some of the important issues were side-stepped, and some of the issues were dismissed based on inaccurate statements. Details are provided below.

1. An important issue not addressed in my previous comment is the similarity between the current analysis of convective impact on UTLS water and the *Ueyama et al.* (2015) and *Ueyama et al.* (2018) analyses. These studies used a much more detailed model with full treatment cloud microphysics and vertical redistribution of water vapor by both in situ clouds and convective clouds. A convective cloud-top product derived from observations was used. The focus of the Ueyama et al. studies was processes controlling water vapor at 100 hPa, including deep convection. They showed that convective hydration has a significant impact in both Boreal winter (2015 paper) and summer (2018 paper). They showed that convective hydration is responsible for much of the geographic structure in 100-hPa H_2O during summertime observed by MLS, and the overall tropical-mean 100-hPa water vapor is increased substantially during summertime. Together, the papers showed that convective influence increases 100-hPa water vapor more during summertime than wintertime. Hence, these studies already showed that convective influence was responsible for much of the seasonal cycle in MLS-observed water vapor at 100-hPa during summertime. The *Wang et al.* manuscript does not cite or discuss the Ueyama et al. papers. This oversight should be corrected.

2. As noted in the first comment, the plausibility of the *Wang et al.* results depends entirely on how well the GEOSCCM convective ice water content product represents the occurrence of convective clouds above the tropopause. In their response to my first comment, the authors state that "our goal is not to validate the GEOSCCM." Yet, their use of multiple figures to compare the GEOSCCM convective IWC to CALIOP

observations is obviously an attempt to validate this aspect of the model. In fact, the discussion in the original manuscript gives the impression that the GEOSCCM product is perfectly reasonable.

Even more alarming is that the "convective" subset of CALIOP IWC shown in AC3 Figure 1 seems to extend to much higher altitudes than the full CALIOP IWC shown in Figure 3 of the originally submitted manuscript. In Figure 3 of the submitted manuscript, the occurrence of all clouds detected by CALIOP appears to drop off to near zero just above 100 hPa. In Figure 1 of AC3, the "convective" subset extends to above 68 hPa! This contrast is physically unrealistic, and it is not clear what the authors are actually doing with the data. Are they really claiming that convective clouds extend above 68 hPa as indicated in Fig. 1 of AC3?

Lastly, and perhaps most importantly, the critical issue here is how much IWC the GEOSCCM product predicts to be above the *local* tropopause. The basic result of the paper is that sublimation of convectively-lofted ice in the lower stratosphere is an important source term in the stratospheric water vapor budget. This result depends entirely on how much ice exists above the local tropopause where it will sublimate in the warm, dry lower stratosphere. None of the figures shown by *Wang et al.* directly evaluates the plausibility of GEOSCCM convective ice occurrence above the local tropopause. Addressing this issue would require examination of the GEOSCCM IWC field in tropopause-relative coordinates. This evaluation is critical in order to determine the plausibility of the central results of the paper.

3. In AC3, the authors dismiss the value of water vapor isotope measurements for assessment of transport and hydration/dehydration mechanisms. In fact, as noted extensively in the literature, the HDO/H_2O fraction is extremely sensitive to sublimation of convectively-lofted ice. If nothing else, water isotope measurements are very useful for identifying where this process might be occurring and contributing significantly to the water vapor budget. *Randel et al.* (2012) used global ACE-FTS data to examine the

geographic distribution of HDO/H_2O fraction, and they showed that HDO enrichment is clearly evident over the north American monsoon, but there is no indication of such HDO enrichment over the Asian monsoon. The ACE-FTS isotope measurements are consistent with the in situ and MLS measurements indicating convective plumes with enhanced H_2O deep in the stratosphere over the north American monsoon (*Schwartz et al.*, 2013; *Smith et al.*, 2017). As noted previously, the calculations presented here based on the GEOSCCM convective IWC product indicate lower-stratospheric hydration is predominantly occurring over the Asian monsoon, which is in direct conflict with available observational evidence.

In AC3, the authors state that *Schwartz et al.* (2013) shows anomalously high water vapor mixing ratios over both monsoon regions at 100 hPa. This statement is apparently meant to be interpreted as evidence that convective hydration of the lower stratosphere is occurring over both monsoons. However, as is well known, the tropopause is very high over the Asian monsoon (near 390 K potential temperature, about 82 hPa), therefore 100 hPa is well within the troposphere over the Asian monsoon. In contrast, the tropopause is relatively low over the north American monsoon region, and many of the anomalously high water vapor concentrations at 100 hPa are well within the stratosphere. Again, the observational evidence suggests convective overshooting deep into the stratosphere occurs over the north American monsoon region but not over the Asian monsoon region. The modeling results presented here indicate the exact opposite. This is an important point that should be addressed by the authors.

3. In AC3, the authors dismissed the *Schoeberl et al.* (2018) paper as irrelevant because it only addressed the convective influence on stratospheric humidity during wintertime. As a reminder, *Schoeberl et al.* (2018) used much the same model as the *Wang et al.* paper here, yet they used the observation-based convection product described in the *Ueyama et al.* papers discussed above. They showed that convective influence has a minimal impact on wintertime stratospheric humidity. This paper actually does conflict with the current study. Examination of the *Wang et al.* paper figures

indicates they are getting a substantial increase in stratospheric humidity throughout the year. The obvious difference is that *Schoeberl et al.* (2018) used the observation-based convective cloud-top product which indicates that convection extends above the local tropopause far less frequently than indicated by the GEOSCCM convective IWC product. The discrepancy between the *Wang et al.* model results and those of *Schoeberl et al.* (2018) should be addressed.

4. One last note: In AC3, the authors now show that the GEOSCCM convective IWC should be reduced by a factor of 5 (or 10) to provide better agreement with observations. If the GEOSCCM convective IWC is far too high, as the authors now seem to agree, that presumably invalidates the results of *Dessler et al.* (2016). The earlier study used the GEOSCCM convective IWC along with the trajectory model to argue that convective ice sublimation in the stratosphere accounts for a significant fraction (20–50%) of the global-model-predicted increase in stratospheric humidity over the 21st century. Presumably, if the GEOSCCM convective IWC is far too high, these estimates of convective contribution to future stratospheric humidity increase are also much too high. This issue should be acknowledged by the authors.

References

Dessler, A. E., T. Wang, M. R. Schoeberl, L. D. Oman, A. R. Douglass, A. H. Butler, K. H. Rosenlof, S. M. Davis, and R. W. Portmann (2016), Transport of ice into the stratosphere and the humidification of the stratosphere over the 21st century, *Geophys. Res. Lett.*, *43*, doi:10.1002/2016GL067,991.

Randel, W. J., E. Moyer, M. Park, E. J. Jensen, P. Bernath, K. A. Walker, and C. Boone (2012), Global variations of HDO and HDO/H_2O ratios in the upper troposphere and lower stratosphere derived from ace-fts satellite measurements, *J. Geophys. Res.*, *117*, doi:10.1029/2011JD016,632.

Schoeberl, M. R., E. J. Jensen, L. Pfister, R. Ueyama, M. Avery, and A. E. Dessler (2018), Convective hydration of the upper troposphere and lower stratosphere, *J. Geophys. Res.*, *123*, doi:10.1029/2018JD0282,865.

Schwartz, M. J., W. G. Read, M. L. Santee, N. J. Livesey, L. Froidevaux, A. Lambert, and G. L. Manney (2013), Convectively injected water vapor in the north American summer lowermost stratosphere, *Geophys. Res. Lett.*, *40*, doi:10.1002/grl.50,421.

Smith, J. B., D. M. Wilmouth, K. M. Bedka, K. P. Bowman, C. R. Homeyer, J. A. Dykema, M. R. Sargent, C. E. Clapp, S. S. Leroy, D. S. Sayres, J. M. Dean-Day, T. P. Bui, and J. G. Anderson (2017), A case study of convectively sourced water vapor observed in the overworld stratosphere over the united states, *J. Geophys. Res.*, *122*, doi:10.1002/2017JD026,831.

Ueyama, R., E. J. Jensen, L. Pfister, and J.-E. Kim (2015), Dynamical, convective, and microphysical control on wintertime distributions of water vapor and clouds in the tropical tropopause layer, *J. Geophys. Res.*, *120*, doi:10.1002/2015JD023,318.

Ueyama, R., E. J. Jensen, and L. Pfister (2018), Convective influence on the humidity and clouds in the tropical tropopause layer during boreal summer, *J. Geophys. Res.*, *123*, doi:10.1029/2018JD028,674.