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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.

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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 geo-graphic structure in 100-hPa H2O during summertime observed by

MLS, and the over-all 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."

We agree that we should reference Ueyama et al. (2015, 2018) and will do so in the revised version.

While our results are broadly in agreement with Ueyama et al., we have an entirely different methodology, as Jensen describes above. Jensen may feel that a paper he is a co-author on has answered all questions about this issue, but we respectfully disagree with that assessment.

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 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."

No. What this result depends entirely on is that the GEOSCCM water vapor reproduces the MLS water vapor. Once that is established, we can then tear into the model to determine what processes in the model are responsible. We find that convective ice evaporation is playing a key role.

We included comparisons of the model and observed IWC field because one of the reviewers of Dessler et al. (2016) (discussed below) gave us a very hard time in the review of that paper, and we thought that it would be useful to show those comparisons. For the purposes of this analysis, it is our opinion that the GEOSCCM IWC fields are 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 68hPa as indicated in Fig. 1 of AC3?"

Figure 1 below is a re-plot of Fig. 1 from AC3, using the same units and color-scale as the Fig. 3 from the originally submitted manuscript. It confirms that the GEOSCCM has too much IWC in the TTL. However, as we showed in AC3, if we lower the IWC so that it agrees better with CALIOP (i.e., GEOSCCM IWC divided by 5), the result stays the same.

Jensen points out potential issues with the GEOSCCM putting ice too high into the stratosphere. To quantify the impact of errors in the altitude distribution, we show in Fig. 2 a run where we don’t allow any ice evaporation above 90 hPa. The difference between the full convective ice evaporation run (Fig. 2c) and this test run (Fig. 2d) is small between 30°S-30°N (Fig. 2e). The larger moisture difference at higher latitudes comes from the lowermost stratosphere. We conclude that the convective ice above 90 hPa has little impact on the water vapor seasonal cycle at 100 hPa. Thus, even if the GEOSCCM puts ice too high, it does not impact our analysis.

"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."

Whether or not the air is above the local tropopause is an irrelevant detail. We know that dehydration is occurring above 100 hPa, so we make no claim anywhere in the paper that convective hydration at 100 hPa is important for the bulk stratosphere âĂŤ as discussed below, Schoeberl et al. (2018) shows it mostly doesn’t. Our analysis focuses on 100 hPa because it’s where the MLS data are available, and it is a level of interest owing to its position in the mid-TTL. Thus, we do not feel that a tropopause-relative coordinate analysis is needed here.

However, we agree that we could discuss this better in the paper, and we have done so.

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/H2O 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 distribution of HDO/H2O 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 H2O 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."

This essentially repeats a point Jensen made in his first comment, but does not engage the substance of our response, so we repeat our response here: “We agree that more work should be done on HDO and we have now put in a statement to that effect. However, we do not think that this is a strong argument against our conclusions. One thing that is clear in the 20+ years people have been analyzing stratospheric HDO is that it is not a strong constraint on water vapor processes and multiple sets of processes can produce the observed HDO fields. For example, Dessler et al. (2007) argues convection is required to explain stratospheric HDO, but Gettelman and Webster (2005) argue that it is not required. It is our hope that our paper will motivate future work on this issue.”

We believe it is also worth noting that Ueyama et al. (2018), which Jensen mentions several times in his comments and on which Jensen is a coauthor, state in the abstract: “Parcels are most frequently hydrated by deep convection in the southern sector of the Asian monsoon anticyclone and subsequently dehydrated downstream of convection to the west, shifting the locations of final dehydration northwest of the cold temperature region in the northern Tropics.” Thus, our analysis agrees with Jensen’s prior work.

As we said in our last response, we will edit the text in the paper to acknowledge that analyses of HDO would be beneficial.

"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 con-

trast, 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."

We agree with this point. We do not intend to make claims that 100 hPa is in the stratosphere at all locations. We acknowledge that this point may not be sufficiently clear in the manuscript and will make it clear in the revised manuscript.

4. "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… 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 clear resolution of this discrepancy is to point out that Schoeberl et al. (2018) was looking at 18-30 km average water vapor, while our paper examines the summertime 100-hPa surface. There is dehydration occurring above 100 hPa, so Schoeberl et al. (2018) can claim correctly that there is not much of an effect in the bulk of the stratosphere and we can correctly claim that there is an observable impact at 100 hPa.

We additionally note that Schoeberl et al. (2018) used the same observation-based convection product described in the Ueyama et al. papers, but Ueyama found a much bigger response at 100 hPa. This provides additional support for our interpretation of these papers.

We will edit the text in the paper to make sure there is no confusion on this issue.

5. "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."

The point of Dessler et al. (2016) was to diagnose the cause of the trend in the model. Dessler et al. (2016) did indeed find that convective ice evaporation was responsible for a significant part of the long-term trend in the model and that conclusion is still true, regardless of whether the model's IWC fields are accurate.

Jensen incorrectly implies that Dessler et al. claimed that the model's IWC fields were correct âĂŤ in fact, Dessler et al. went out of their way to say that their analysis should encourage more research on the reality of the GEOSCCM's convective ice field. Here is a quote from that paper: "Nevertheless, the CCMs' predictions of ice lofting into the lower stratosphere have not been quantitatively tested against observations. The CCMs' predictions rely on their convective parameterizations, and until verified with observations, one could reasonably question the realism of their representation of the infrequent but intense convective systems that penetrate the stratosphere."

"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."

This is an elementary error: Jensen mistakes a bias in the mean field with a bias in the feedback. It is well-known, for example, that many climate models have biases in their water vapor field when compared to observations. However, all the models predict the same water vapor feedback because they all predict the same increase in water vapor with temperature. Thus, a bias in the stratospheric ice, when compared to observations, does NOT mean that the change in ice water content over the 21st-

century as the climate warms is also wrong. It may be âĂŤ and subsequent research may show that it is – but until then we can't say anything about the reality of the long-term trend in the model.

Because we believe Jensen's comments on this point are without merit, we have not made any changes to the manuscript in response to this.

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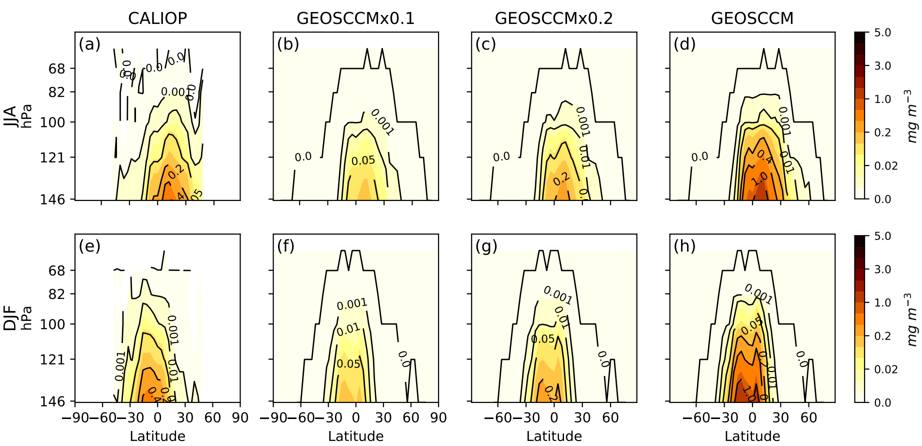

Fig. 1. . CALIOP, GEOSCCM, and reduced GEOSCCM zonal mean convective ice (mg m-3) in a pressurelatitude domain.

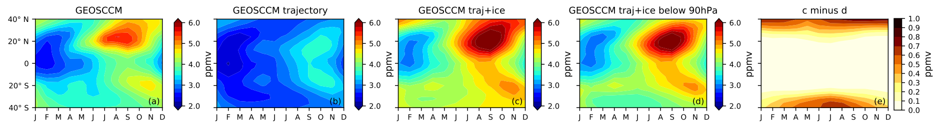

Fig. 2. Zonal mean water vapor seasonal cycle at 100 hPa from GEOSCCM and GEOSCCM trajectory models (1-d). Panel e shows the difference between panel c and d.