Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-21-RC2, 2016

Atmospheric Chemistry and Physics
Open Access
Discussions

Interactive comment on "Impact of geographic variations of convective and dehydration center on stratospheric water vapor over the Asian monsoon region" *by* K. Zhang et al.

Anonymous Referee #2

Received and published: 27 March 2016

First, my apologies to the authors and the editor for the long delay in publishing this review.

This manuscript examines how changes in the distribution of convective sources and dehydration locations of air in the Asian monsoon upper tropospheric anticyclone affect the amount of water vapour entering the lower stratosphere in this region. The concept is worthwhile, and the paper makes some valid points about how seasonal and intraseasonal variability in convective sources influence the moisture content of air near the tropopause. However, some aspects of the methodology and argument are problematic, and require more justification at the very least.

The biggest weakness of this paper is that it takes water vapour variations at 100 hPa

to be representative of the 'lower stratosphere', neglecting previous work indicating that final dehydration for air entering the tropical stratosphere via this region typically occurs at lower pressures / higher altitudes. This does not necessarily invalidate the core conclusions of this paper (the processes controlling water vapour variability at 100 hPa are also important to understand, particularly if they propagate to higher levels), but it does imply strong limitations on their applicability that are not effectively communicated or explored in the paper. At the very least, the authors should clarify that 'LS' in this case means 100 hPa, and discuss the limitations that that entails. Even better, the authors could use their trajectory simulations to connect the results and conclusions at 100 hPa to final dehydration statistics and stratospheric entry mixing ratio. In other words: do these intraseasonal differences in source location / temperature distribution / transport affect the amount of water vapour entering the global stratosphere via the Asian monsoon anticyclone, and, if so, how much? These additions would help tremendously in establishing how this work fits in the context of other studies of water vapour transport and variability in this region.

1 General comments

1. My main concern is that the analysis focuses almost exclusively on water vapour at 100 hPa, and particularly that this is assumed to represent lower stratospheric water vapour. The vertical location of dehydration for air entering the stratosphere varies quite a lot, and is typically higher (in altitude) than 100 hPa. Would the results still be valid for variations in water vapour at 83 or 68 hPa, or are they only relevant to a shallow layer bracketing the tropopause? If I have understood the analysis correctly, this might be checked by analyzing the 'final dehydration' locations and temperatures for these trajectories during transit to the stratosphere in addition to the 'latest dehydration' locations and temperatures for the model results at 100 hPa. Are the statistics of final dehydration for these trajectories

significantly different from those with convective sources over the eastern side of the region? Regardless, more needs to be done here, either to connect these results more clearly to stratospheric entry mixing ratio (post-final dehydration) or clearly distinguish between studies for which 'LS' means 'above the tropopause layer' and/or 'after final dehydration' and this study (where 'LS' means 100 hPa, well within the tropopause layer and likely prior to final dehydration).

2. I'm not convinced that the idealized experiments that separate the dehydration temperatures and dehydration locations are viable in this case. This approach works well when either temperature changes or circulation changes are dominant (and therefore separable), but has little meaning when temperature and circulation changes are tightly coupled. Another way of thinking about this is that separability is a justifiable assumption in situations where changes in dehydration location are dominated either by (1) an unchanged circulation sampling a modified temperature distribution or (2) a modified circulation sampling an unchanged temperature distribution. Too much overlap between these situations results in degeneracy, at which point the contributions of temperature changes and circulation changes cannot be reliably distinguished. My expectation is that in this case the tight couplings among convection, circulation (especially diabatic heating) and temperature at 100 hPa violate separability, as also briefly mentioned by reviewer #1. I am willing to be convinced otherwise, but additional justification for these simulations is needed if they are to be used as supporting evidence here.

3. This work would benefit from a more complete analysis of confidence intervals and significance testing. As also noted by reviewer #1, many of the arguments rely on changes and/or differences that are relatively small. This is particularly relevant for the time series in Fig. 3 and the August minus June differences shown in Fig. 4 and Table 1, and otherwise reported in the text.

4. The text is clear for the most part, but the manuscript would benefit from English-

language editing by a colleague or professional editor. There are a few points where editing will be necessary to improve the clarity; see technical comments below.

2 Specific comments

p.2, l.24-25: this sentence is vague and should be reworded for clarity — at seasonal time scales and large spatial scales both the temporal evolution and geographic distribution of LS water vapour are correlated with convective activity, it's just that these correlations do not generally extend to variability within the anticyclone itself.

p.2, l.28: Given the uncertainties and competing hypotheses put forward by subsequent studies, it would be more appropriate to change 'they can' to 'they proposed that this convection can'

p.3, l.6: Wright et al. (2011) did find large discrepancies among the different reanalysis data sets, but the qualitative results were robust: trajectories originating from convection over Tibet were consistently moister but less numerous than trajectories originating from convection over the other regions, so that these trajectories had relatively limited impacts on water vapour in the global tropical LS.

p.3, l.23: It would be useful to note also the relative precision here (as xx–yy%)

p.4, l.16: In this case, since the focus is on the evolution at 100 hPa, I presume that 'latest dehydration' refers to most recent dehydration rather than final dehydration. This choice should be stated explicitly to prevent confusion – 'latest dehydration' is by itself too vague, as it could mean either 'most recent' or 'final'.

p.4, l.18-19: How is this done, by gridding the simulated water vapour mixing ratios and then applying the averaging kernels to construct a vertical grid? I recommend expanding slightly on this description. Also, as noted later on this page, the exclusion of simulated values below 100 hPa results in a dry bias. I assume this statement is based on testing the sensitivity to whether those values are included. Does this testing indicate whether including/excluding the simulated values at lower levels has any impact on the qualitative evolution of the variability?

p.5, l.5-9: It is very difficult for the reader to evaluate the statement that '100 hPa temperatures over the southeastern flank of the anticyclone ... do not show as significant increase as water vapour from May and June to August'. This argument should be made more quantitative. This could be as simple as a calculation relating the May/June to August mean temperature change to a fractional change in mean saturation specific humidity (with appropriate uncertainty estimates), which can then be compared to the fractional change in simulated water vapour mixing ratio at 100 hPa (with appropriate uncertainty estimates).

p.5, l.13: (Fig. 2) Does 'tropopause temperatures' mean '100 hPa temperatures', or are these evaluated at a diagnosed tropopause?

p.5, l.20: (Fig. 3) Here it would be helpful to include also the evolution of mean 'latest dehydration' temperatures over the eastern and western parts of the domain, with uncertainty estimates. This would help to clarify that it is in fact the shift in dehydration location (and not the temperature evolution) that dominates the seasonal evolution of water vapour at 100 hPa, and could perhaps supplement or replace the idealized simulations in the overall argument.

p.5, l.26: (Table 1) If using these simulations, it would be useful also to include the August–August results to give a quantitative benchmark for evaluating the idealized June–August and August–June simulations. I know that these are shown in Fig. 1, but so are the June–June results. I could not find this number reported anywhere in the text.

p.6, l.12: (Fig. 4) Is there any benefit to including the profiles of diabatic heating below 300 hPa? Including these estimates requires the use of a relatively large scale,

and makes it difficult to distinguish the variations in the UTLS (which is what we are particularly interested in). Moreover, there is a negative anomaly centered around 70 hPa above the location with enhanced convective activity in August relative to June. Is this negative anomaly significant? If so, this suggests that upward motion in this region is weaker than during June above 100 hPa, which might mean that the trajectories involved circuit the anticyclone more times during ascent. This relates to general comment #1 above: how much does this westward shift of convective source location ultimately impact stratospheric entry mixing ratios?

p.6, l.20: (Fig. 5) The use of colours here is confusing, with red sometimes meaning a positive change and sometimes a negative change. Moreover, 'physically consistent with a moist anomaly in the UT' is sometimes indicated by the yellow/orange/red half of the colour scale and sometimes by the green/blue/purple half of the color scale. I recommend that you either make the use of this default colour table logically consistent across panels or use different colour tables for anomalies in different quantities. I also agree with reviewer #1 that the fonts are too small and difficult to read in several of the panels included in Fig. 5, and I can barely even see the variations of the lines in panel (f). If the variations for all of the years are necessary, perhaps it would be better to make panel (f) a separate figure and split it into multiple panels, one for each year? If not, it might be best to show the variations for a selected time period covering one or two years, so that those variations are easier to identify in the figure.

p.7, l.1: Which data is used to identify the increase in cirrus clouds?

p.7, l.3: What mechanism drives the enhanced ascending motion? Enhanced latent heating above 370 K? Enhanced cloud radiative heating? Is this enhanced ascending motion consistent between ERA-Interim and MERRA? This could be explored by looking at the components of the heat budget — both ERA-Interim and MERRA provide clear-sky and all-sky radiative heating products.

p.7, l.9: (Fig. 6) For clarity, the definitions of 'wet' and 'dry' days should perhaps be moved from l.20 to here.

p.7, l.18-19: It would be useful to include the correlation for traj_MERRA if data from the mismatched period in 2006 is excluded.

p.8, l.25: The presented work only supports this statement if we consider 100 hPa to be representative of the LS in this region — no confirmation has been shown that this seasonal evolution in the convective source extends to lower pressures / higher altitudes, which should also be considered part of the LS.

p.9, l.4: What is meant by 'cold-point' here? The coldest temperatures in the geographic distribution between 370 K and 100 hPa? The vertical cold point tropopause?

3 Technical suggestions

page 1
l.24: recommend changing this to 'Due to the warmer dehydration temperatures, anomalously moist air enters...'
l.25: typo: 'frank' → 'flank'

page 2
l.13: 'moist' → 'moisture'
l.18: recommend moving 'controlling transport' to after 'processes' instead of before
l.24: 'ascent' → 'ascends'

page 3
l.1: recommend moving 'over the Bay of Bengal and Southeast Asia' to after 'direct convective injection' for readability.

page 4
l.25: recommend deleting 'It is featured with' for clarity

page 5
l.7: 'as significant increase' → 'as significant of an increase'

page 6
l.20: 'described in Section 2 to the data' → 'to the data as described in Section 2'
l.28: typo: 'principle' → 'principal'

page 7
l.3: typo: 'dehydration' → 'dehydrated'

page 8
l.3: typo: missing 'mid' or 'late' in 'early-to-summer moistening'?

page 9
l.6: the meaning of 'convective protrusion' here is not clear — do you mean that convection over these regions is particularly deep relative to other parts of the monsoon domain, that convection is particularly frequent in these regions, or something else?
l.11: typo: 'studies' → 'study'

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-21, 2016.