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Atmospheric Chemistry and Physics
Discussions

Interactive comment on “Processes influencing lower stratospheric water vapour in monsoon anticyclones: insights from Lagrangian modeling” *by* Nuria Pilar Plaza et al.

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1 General comments

We thank the Reviewer for the very thorough and detailed comments which will definitely help to substantially improve the paper. We see the critical, but also very constructive, tone in some of the comments, and we did some extensive work (including substantial extension of the methodology, several additional sensitivity simulations, significant text changes) and think we can finally address all of the comments very well. Below, the reviewer comments are in blue, our replies in black, indicating parts in the manuscript with major changes. The major changes in the revised version are:

- Clearer description of the paper goals (effects of small-scale mixing transports on monsoon water vapour), and more appropriate relation of the used methodology to literature. In particular, we made it clearer that the focus is on comparing effects of different processes in sensitivity simulations (with all these based partly on simplifying assumptions) and not presenting a “best simulation case”.
- Added study of effects of supersaturation using sensitivity simulations with varied nucleation barrier (150% vs. 100% relative humidity) and added a new Fig. 3.
- We included effects of convection in the study, as suggested by the Reviewers, and added a discussion of related sensitivities (new Fig. 6).
- An analysis of the robustness of the calculation of the process effect as difference, from new sensitivity simulation VMIX without ice microphysics.
- Improved discussion of air parcel density in the monsoon, based on new sensitivity calculations.

2 Specific comments

#1: The microphysical model in this simulation is quite simple compared to the schemes used by Ueyama et al. and Schoeberl et al. This could be important in determining the water vapour field in the upper troposphere and the conclusions of the authors. Line 166 describes their scheme. Basically, water vapour in excess of saturation is made available for ice particle formation, since the particle number density is imposed, this fixes the particle size, and dehydration occurs through settling. Particle number densities are derived from Krämer et al (2009). First, rereading Krämer, it wasn't clear whether the particle number densities used here were temperature dependent as shown in Krämer Fig. 5. Second, with fixed particle sizes, this scheme will likely overestimate dehydration. Once crystals form, particle growth occurs and the dehydration rate is initially slow because the settling rate is slow for small particles. If the parcel warms up during the beginning of the cloud formation process, the ice will evaporate producing almost no dehydration. This is how short horizontal wavelength gravity waves can produce clouds with almost no effective dehydration. The author's formulation of the microphysics, I believe will low bias the water vapour concentration. Third, the saturation level (100%RH) is used to trigger dehydration yet Krämer clearly shows that UTLS air can be supersaturated without cloud formation (a result also found by ATTREX flights, Jensen et al., 2017). Neglect of super saturation will also low bias the water vapour compared to observations.

Reply: We agree with the reviewer that the used approach simplifies key microphysical processes and their interactions with dynamics. Indeed, there are many unknowns in the treatment of microphysical processes in the TTL and, in particular, their interactions with gravity waves (including supersaturation and nucleation delays, heterogeneous nucleation, nucleation quenching by gravity waves, interactions with sedimentation). However, our goal in this paper is to focus on the effects of large-scale temperature and dynamics and the additional effect of small-scale atmospheric mixing, whose impact on water vapor in the lower stratospheric monsoon anticyclones has not yet been considered thoroughly. Hence, we do not aim at a detailed microphysical process description, which is better treated elsewhere, but try to keep it as simple as possible to enable clear interpretation of transport effects. Nevertheless, the simplified representation of microphysical processes used here has been shown appropriate in previous works and is still considered as state-of-the-art for a sensitivity analysis like here (e.g., Fueglistaler et al., 2005; Schoeberl et al., 2011; 2012; 2013; Zhang et al. 2016, Poshyvailo et al. 2018, Wang et al. 2019). We will better explain the used simplifications and the scientific focus of our approach in the revised paper to make these points clearer (e.g., the used particle number density, which is here taken not depending on temperature, and will also improve the discussion of relations to published work including more detailed microphysics. Furthermore, to highlight the potential role of the observed supersaturation within clouds, we have performed a sensitivity analysis varying that parameter (see answer to Reviewer 2).

#2: Another important consideration is the convective parameterization used in this simulation. Ueyama et al. (2018) used observed convective heights to add water to the parcel distribution if the parcels are below the convective top and near the convection. I am not exactly sure what is used here (this aspect of the paper needs improvement, but on lines 331, 388 it states that convection is not included). Convection is an important part of the water vapour budget over the monsoon regions, and not including it colors the validity of the simulations and conclusions reached here. Lack of convective moistening could also lead to the low water vapour bias over the monsoons shown in C2the CIRRUS simulation, for example. I suggest that you take the approach used by Ueyama and Schoeberl. Get the convective heights from ERAi and saturate parcels nearby and below convective tops.

Reply: Convection was indeed not included in the previous simulations, but we agree with the reviewer that it is an essential process which should not be ignored here. Therefore, we have decided to carry out and include additional sensitivity simulations with convection in the revised paper. Since ERA interim fields at about 1° rely on a convective parameterization essentially tuned to match the tropospheric heat and water budget, it is likely poorly suited for TTL water vapor and cloudiness. In order to include an estimate of the convective impact in our study, we follow the reviewer's suggestion (and Ueyama et al., 2018; Tissier and Legras, 2016) and use infrared brightness temperature from geostationary satellites to infer the convective cloud top information. More precisely, we use the NOAA GridSat-B1 product (Knapp et al. 2010) and ERA interim temperature and pressure profile data to deduce cloud top altitudes using the method outlined Tissier and Legras (2016).

Figure 1 shows the water vapour distributions at 100hPa performed by our non-convective experiment, TRAJ, (Fig. 1a), our convective experiment CONV (Fig. 1b) and the difference between both experiments for JJA in 2008. According to these results, convection has little impact on the Asian Monsoon. This disagreement with Ueyama et al. (2018) could be caused by the different diabatic heating rates used to transport air parcels vertically. In CONV, we use ERA-Interim total diabatic heating rates, while Ueyama et al. (2018) use Yang's radiative diabatic heating rates (Yang et al. 2010).

Another difference is the time period of the study. While we consider the whole summer of 2008, Ueyama et al. (2018) focused on a 7-day period in the middle of the summer in 2007. To see if this issue explains our disagreement with Ueyama et al. (2018), we have plotted the convective water vapour distributions for the same 7-day period. However, our results still differ from Ueyama et al. (2018). Our experiment TRAJ (no convection) performs a maxima of water vapour not seen in their results with the non-convective experiment. In addition, CONV stays very similar to TRAJ during the same period of time. This disagrees with the increase in water vapour registered by Ueyama et al. (2018) using their experiment with convection.

Though our results show differences to Ueyama et al. (2018), they lead to similar conclusions as other studies that have found a weak to negligible impact of convection (Randel et al. 2015, Wang et al. 2019). Nevertheless, our approach still has several limitations. For instance, we have not included additional ice formed during the convective processes, which has proven to have an impact on the LS water vapour budget, according to Wang et al. (2019). These discrepancies should be studied in a deeper future follow-up study, in which all the sub processes involved in convection are more carefully controlled. However, we think that also the simplified parameterization of the convective effect as we included in the revised manuscript (and as suggested by the Reviewer) adds substantial additional value to the manuscript. The effects of convection are mainly described in the revised manuscript on P14, L443-481.

#3: CLaMS apparently mixes water vapour as it mixes other tracers - as described in McKenna et al. (2002) - and that transport can be cross isentropic. Unlike chemical tracers, water vapour concentration can be temperature sensitive if saturation is reached. Does CLaMS consider that the mixing between parcels may undergo temperature excursions that could remove water due to ice formation? In strong shear zones, the Richardson number will fall below 1/4 and the turbulent field will produce strong temperature excursions, cloud decks and dehydration. In any event, Is the total water content mixed - ice plus water vapour or just the vapour?

Reply: In a turbulent layer, adiabatic temperature excursions indeed occur, but they are transient and might not be long enough to lead to dehydration. It is not obvious how to represent them and this effect is hence not accounted for - Thanks for pointing to that. What is however included is the effect of mixing on both mean potential temperature and water vapor separately. Due to the vertical temperature variability between two mixed air parcels (Fig. 2a) and the convexity of the Clausius-Clapeyron function (Fig. 2b), mixing might lead to supersaturation even when both mixed air parcels are subsaturated. To handle this, dehydration in the cirrus module is run both before and after the mixing. The description of the representation of the small-scale mixing effect is improved in the revised version (e.g., P17, L535-541).

#4: The authors spend some time discussing how LTF might be biased by having too few parcels in the AMA anticyclone. They note that CLaMS mixing simulations - by spawning new parcels - resolves this problem. But by spawning new parcels, CLaMS increases the parcel density over the whole domain (Table 1). For a rational comparison, the authors should try to increase the LTF injection rate to improve resolution above AMA. If the water vapour field above the AMA begins to converge they likely have reached a high enough injection rate.

Reply: One of the aims of this paper is to study the effects on water vapour distribution of the different setups, LTF and ST-Filling, used with CLaMS. Though the LTF scheme has been used frequently in the literature, we are not aware of reporting of its lower density of air parcels in the AMA compared to other regions. The existence of gaps in a 5x2 longitude x latitude grid reflects this feature.

One way to avoid the presence of gaps is to enlarge the size of the bins when gridding the air parcels. Doing this, we increase the number of air parcels per bin and discard possible empty places. Figure 3 shows the water vapour average in the AMA for the TRAJ experiment using different bin sizes, 5x2 and 10x4 lonxlat-grid, and directly the Lagrangian air parcels water vapour content without projecting them onto a regular grid. Water vapour averages in the monsoon do not show large differences between these different computations. Therefore, our results with TRAJ seem to be invariant to the existence of gaps end, hence, the density of air parcels.

#5: The authors make many comparisons to MLS, but they should run their model simulations through the MLS vertical averaging kernel to correctly make such comparisons. This will tend to increase the water vapour in the models somewhat because of the strong non-linear vertical gradient in water in the upper troposphere.

Reply: We did not apply the AK in the submitted version by purpose, as we're interested in the sensitivity of model results to certain processes, and we don't want to smear out the detailed effects. However, we agree with the Reviewer that it is necessary to estimate the potential effect of the AK on the presented comparison, and we do so in Fig. 4. At 100hPa there is of course some sensitivity to applying the AK, as the vertical gradient is rather large, at 82hPa there is not much of an effect left. Importantly, also at 100hPa the AK effect does not change the patterns in the distribution in the tropics and subtropics. Hence, neglecting the AK effect in this paper will not affect our conclusions strongly. We included a short related comment in the revised version on P8, L235.

3 Technical corrections

Ln 28: also reference Randel and Park (2019; JGR) **Reply:**We have done it in the paper, as suggested.

Ln 43 you may also want to reference Randel et al 2011 for a discussion of the differences between AMA and NAMA with regard to the water vapour field. **Reply:** We think the Reviewer refers to Randel et al. (2012). We included this relevant paper in the introduction (P2 L45).

Ln 78 'has not been assessed yet' please see Schoeberl et al. (2018) Fig. 3 **Reply:** Schoeberl et al. (2018) performed several experiments to study the impact of tropical convection on stratospheric water vapour, concluding that convection has little effect. However, their results are focused on the analysis of changes in global stratospheric water vapour, with special attention to the winter season, without studying the effect on monsoon regions during boreal summer. More recently, Schoeberl et al. (2019) found that convection in the Asian Monsoon is tied to the highest RHi region consistent with Ueyama et al. (2018), which has been already mentioned in our paper (P3, L70). However, they do not focus in explaining this process but in the TTL boreal winter. Nevertheless, we have changed our sentence to “has not been fully assessed yet.” (P3,L77).

Ln 84 - what does 'they' refer to? **Reply:** We were referring to air masses. The text has been modified accordingly.

Ln 100 Please use the latest version of MLS. V4.2 is somewhat wetter than V5 **Reply:** As stated by the Reviewer, the most recent version of MLS is v5 which has been proven to be 5-10% drier than v4.2 at stratospheric levels but with increased humidity in the tropics at 147hPa. However, as this version was released during the period of writing the paper we just keep v4.2. Also, our main conclusions concern the differences between the various sensitivity simulations including different processes and are unaffected by the version of used MLS data.

Ln 138, 145 Mention here that small scale mixing by CLaMS spawns new parcels thus producing a large variation in # of parcels from 400,000 to 20 million shown in Table 1. **Reply:** We have done the change proposed, but only in L145 as in L138 we are describing the Schoeberl et al. Domain Filling technique in general.

Ln 153 Assuming that LMR is defined by 100% RH? Please be specific. **Reply:** We have changed the paper to make this point clearer (P6, L161)

Ln 168 Please elaborate on what the 'characteristic length' is? The loss of ice from a parcel per time step is âĹij Ice*ws*?t/L where Ice is the ice mixing ratio, ws is the settling velocity, ?t time step and L is the cloud depth. Is L the characteristic length? **Reply:** We agree that we were not clear about this aspect in the submitted manuscript - thanks for pointing that out! The characteristic length parameterizes the ice fall-out in a simple way, just as suspected by the Reviewer. Hence, in each model time step t a sedimentation length for a mean spherical ice particle (of mean radius, given the ice water content and an empirical particle density, see e.g., Ploeger et al., 2013) is calculated as s = w*t and compared to the characteristic length L. The ice loss during that time step is Ice*(s/L). This is explained in more detail in the revised version (P6, L78).

Ln 185 Please explain how supersaturation can develop after the mixing step if you have already restricted supersaturation before mixing. Also in the small scale mixing, is the ice divided up as well ? **Reply:** Please, see major comment #3 above.

Ln 187 Does convective moistening also occur with the convective updrafts? Shouldn't you be carrying ice into the updraft region. It seems to me this could be important to the water vapour budget over the monsoons. **Reply:** The vertical mixing procedure enhances mixing in the vertical coordinate. This is done in the same way as small-scale mixing works. Therefore, not only water vapour but also ice is mixed, which could result in ice injection at higher altitudes. Once vertical mixing is considered, we apply again

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the cirrus parameterization, to set possible supersaturated air parcels to saturation conditions (RH=100%). We clarified the respective text parts in the manuscript (P7, L209).

Ln 200 I don't understand what is going on in STANDARD. Parcels released at 360 ascend into the stratosphere, dehydrate, then descend into the troposphere and mix with other parcels. It seems to me this would produce a very dry troposphere compared to what is observed, if I am understanding this correctly. **Reply:** STANDARD is a long multiyear full-CTM CLaMS simulation with air parcels initialized throughout the domain at the beginning of the simulation and thereafter initialized in each time step only in the boundary layer (e.g., Pommrich et al., 2014). Therefore, it differs from the rest of experiments that use LTF set up. In STANDARD, air parcels are not released at 360K but fill the whole atmosphere considered by the model, including the troposphere. Whenever the air parcels move below about 500hPa (exactly, the boundary condition is at 250K hybrid potential temperature, the vertical coordinate in CLaMS), their water vapour content is set to the water vapour fields of ERAinterim.

Ln 210 It would be useful to see a distribution of parcels with altitude for the various experiments. The STANDARD experiment I expect would have a large number of parcels in the troposphere. **Reply:** The vertical distribution of air parcels for one given day in the TRAJ, SSMIX and STANDARD experiments is shown in Fig. 5. While LTF experiments, such as TRAJ and SSMIX, do not have air parcels below 250hPa, STANDARD fills up the troposphere as well (as explained in the reply to the previous comments).

Ln 213 You should update to MLS V5 **Reply:** See our reply above.

Ln 220 To make an exact comparison to MLS you should run the model output through the MLS averaging kernels. Please explain why you did not do this, or indicate that you did do this. **Reply:** See above (major comment #5).

Ln 227 I am not surprised that Traj and Chem have such low water vapour as has been found by others (e.g. Schoeberl et al., 2016). Basically, the inclusion of a cloud model and setting the nucleation RH to greater than 100% increases water vapour substantially over simply using the LDP value of water. **Reply:** We have added “as expected” (P8, L249).

Ln 240 None of this is surprising and consistent with the water vapour budget of the stratosphere. You could use a few references here on methane oxidation and conser- vation of 2 CH4+H2O in the stratosphere. **Reply:** We have added “as expected” (P9,L265), and also references concerning methane oxidation (eg., Randel et al., 1998).

Ln 260 The fact that small scale mixing increases water mostly in the monsoon only is a puzzle. According to you the mixing avoids the cold traps, but adiabatic turbulence produce cold temperatures and dehydration? The mixing scheme transfers water but doesn’t take into account the temperature variation during that transfer - thus it would always overestimate the moistening by mixing. Since the model lacks ice injection by convection we can’t tell if this mixing process competes with convective moistening. **Reply:** The mixing scheme is considering both temperature and water vapor, as explained above (answer to major comments). Only small-scale transient temperature fluctuations in turbulent layers are neglected, but those are short-lived. We now mention those assumptions explicitly in the main text. Furthermore, we have included an estimate of the convection effect as suggested by the Reviewer and compare the mixing results to that (as explained above).

Ln 277 I am not sure I agree that the effect of convective updrafts are limited by removing air parcels below 250hPa. The authors need to explain in more detail how mixing enhances convection. The authors should also re-read how convective influence is parameterized in Ueyama et al. (2018). Ice is added to parcels passing near convec- tion that are below the convective cloud tops. A similar scheme is used by Schoeberl et al. (2019). The advanced cloud model in Ueyama et al. hydrates the air appro- priately for parcels that have collided with convection. From ln 277 to ln 280 is pretty speculative. **Reply:** We agree that convective updrafts are important for vertical transport of tracers, such as the water vapour, into the LS. In our experiments, we consider this process inside VMIX. However, as VMIX is configured using the LTF scheme, air parcels are removed below 250 hPa. What we meant here is that this removing of parcels likely limits the accuracy of VMIX to reproduce properly convective updrafts. Therefore, our results cannot be seen as the full picture of convection yet. As described already above, in the revised version we included another sensitivity experiment to account for the effects of convection using a similar approach as Ueyama et al. (2018).

Ln 290 The STANDARD experiment shows interesting results, but I am not sure I agree with its conclusions. The question that needs to be asked is where does water in the mid-troposphere come from? In the tropics, water vapour is detrained from convection moistening air that is descending from even higher levels. In the mid-latitudes, moist air also rises along frontal systems. I have no doubt that CLaMS can simulate the horizontal transport of water vapour, but the rehydration of parcels through convective processes is not clearly specified. If the LTF is set up correctly, and water vapour fields are initiated at the 360 K surface from observations, the results should be correct. The comments about the deficiencies of LTF are based on the idea that STANDARD is correct which needs to be demonstrated. This point is reinforced later in the paper (Fig. 5) that shows STANDARD produces anomalous water vapour fields compared to MLS especially under the AMA anticyclone. **Reply:** In the troposphere, the air parcels of STANDARD are set to ERAinterim water vapour values. Of course STANDARD has its limitations (as noted by the Reviewer), but in general it is the closest experiment to MLS as it is able to reproduce not only the spatial pattern of water vapour but also its variability in monsoon regions. Besides, LTF is not initialized using observations of water vapour, but setting an homogeneous 50-ppmv field at 360K, which is less realistic than STANDARD at that level. In any case, our results are independent of the initial water vapour used to release air parcels, as the comparison between the two sensitivity SSMIX experiments with different initial conditions (SSMIX-50ppmv and SSMIX-100ppmv) shows.

Ln 295 I agree that NAMA looks closer to MLS observations in STANDARD. But what about the high water vapour fields south of the equator? They are as large as NAMA and are not apparent in the MLS observations. I would argue that STANDARD is a worse simulation than SSMIX. **Reply:** Thanks for this remark! It is true that SSMIX represents water vapour patterns in some regions better than STANDARD, but STANDARD simulates better the water vapour variability in those regions (correlations above 0.7 in AMA and NAMA compared to 0.6 achieved by rest of experiments and Zhang et al. (2017)). It is worth mentioning here, that the partial high-bias of STANDARD water vapour at 100hPa (Fig. 1) does not occur at upper levels (e.g., 82hPa). But we agree, it is indeed not straightforward whether STANDARD or SSMIX globally yield the better agreement with MLS. Hence, we changed the manuscript text accordingly to avoid a clear judgement of the best agreement, but focus more on the relative differences between the experiments related to the particular processes.

Ln 300 It would be very useful to put the temperature cycle on Figure 2 - at least at the tropopause level and perhaps the saturation mixing ratio. This might be a nice quantitative measure of how much water vapour is being enhanced by CLaMS mixing. **Reply:** We do not think that considering mean tropopause temperatures will give much more insights. The water vapour at 100hPa is not set by the averaged tropopause temperature but by the Lagrangian Cold Point LCP (e.g., Zhang et al., 2017). However, the mean saturation mixing ratio at the LCP is just the water vapour in the TRAJ experiment in Fig. 4. Hence, comparison to TRAJ in Fig. 4 already provides the requested information.

Ln 315 ... I would argue that VMIX, SSMIX and STANDARD do the worse job compared to other simulations based on the peak to valley change seen in MLS at 100 hPa. Basically, if you remove the offsets and judge the annual cycle, the CLaMS mixing is creating too much water during the monsoon in Fig. 2. It might be interesting to plot then all normalizing by the April value. **Reply:** Thanks for this good remark! Following the suggestions of the referee, we have replaced Fig. 2 by the amplitude of water vapour cycles in the AMA for each experiment. Indeed, at 100hPa the annual cycle amplitude is too large in the mixing experiments when being compared to MLS. However, at 80hPa the amplitude in the mixing experiments is in very good agreement with MLS. The 100hPa level is frequently below the tropopause in the monsoon region, and the water vapour at this level can not yet be regarded as stratospheric entry values, but is very sensitive to small biases in tropopause height, etc. Hence, we think that comparison at slightly higher levels (here 80hPa) provides actually a better picture of which processes influence stratospheric entry water vapor. We tried to improve the related discussion in the manuscript (P11, L350).

Ln 350 The fact that STANDARD, SSMIX and VMIX produce too rapid a rise in water vapour over the monsoon suggests to me that the mixing rate is too high. Since it can be tuned lower, you might try a sensitivity experiment where the Lyapunov trigger is increased. **Reply:** The proposed sensitivity study has been done in Poshyvailo et al. (2018) (their Figure). Here we used the optimized value they propose. And as described in the previous reply, at levels entirely above the tropopause (e.g., 80 hPa) this choice leads to the best agreement with MLS.

Ln 401 I totally agree that convection is important as I have argued above. So, in these model simulations there is only one process that can transport additional water into the upper troposphere: mixing. No wonder you conclude it is important. The study is flawed unless you include convection and compare the results to mixing. **Reply:** See our response to Major comment #2.

Ln 405 I would argue that you need to tell us more details about water vapour mixing to make sure the readers understand the process. The fact that you have to invoke the dehydration process before and after the mixing step suggest that it is somewhat complicated. How often after you mix does the second application of dehydration actually do something. That would be interesting to know. **Reply:** Please, see major comment #1 from Referee 2, in particular Figure 1.

Ln 415 see comment on 401

Ln 438 I am not surprised by the lower density of air parcels over the AMA anticyclone shown in Figure A2 and the results from Fig 4. The divergent flow will tend to exclude parcels from that region, and the only source of parcels will those rising up from the region below. LTF is, in some sense, a natural sampling system (as opposed to Reverse Domain Fill where the sampling density is chosen ahead of time). The fact that there are some empty bins suggests that the gridding - which is arbitrarily chosen by the authors - is too small or that the parcel release rate is too low. This hypothesis is supported by the lack of gaps in STANDARD which has more than 4 times more parcels than CIRRUS (Table 1). It would be interesting to re-run the CIRRUS experiment quadrupling the parcel release rate. If this experiment is run, and the results are changed then this suggests that the CIRRUS experiment is operating with too low an injection rate and the water vapour field has not converged. **Reply:** See comment #4 about the density of air parcels.

Ln 464 'explains why the occurrence of these gaps gives rise to drier conditions' I am somewhat confused by this statement. I certainly agree that LTF not resolving the AMA through lack of parcels will sample bias the water vapour, but I understood that you argued earlier in the paper that it was water vapour mixing by CLaMS that was increasing the water vapour in the AMA. I think that the experiment suggested above might be able to sort this out. It is likely that transport of water vapour through mixing (whether cor- rect or not) is increasing water vapour in the AMA and the consequential increase of parcels through CLaMS spawning of new parcels is improving the sampling. I might add that LTF simulations performed by Schoeberl and colleagues typically have over 2 million parcels in the stratosphere, a resolution similar to STANDARD. The discussion in paragraph 467 is along the lines of the statements above - basically STANDARD is more successful because of the larger number of parcels. **Reply:** Please, see response to comment #4 above. Regarding the number of air parcels in similar published studies, the number of parcels in our pure trajectory simulations (e.g., TRAJ) should be similar, at least to Schoeberl et al. (2011), who state 500 000 parcels in their simulations (their paper, p. 8435). Therefore, we think that the set-up we use should be comparable, at least with some, published experiments.

Ln 488 'SSMIX ... is set to 50 ppmv' yet Figure 5c shows much lower values and a variation. Is this due to air parcels dehydrating at this level after release? You might want to add an explanation here similar to the point made on line 499 **Reply:** Once air parcels are released, they dehydrate according to the temperature of the region where they have been launched. This is the main reason why the distribution of water vapour at 360K does not correspond to a homogeneous 50ppmv-field. Besides, there are air parcels coming from other levels that contribute to dehydrate the levels. Because of the Brewer-Dobson circulation, most of the air parcels reaching this potential temperature level come from high latitudes after leaving the stratosphere. This means that their water vapour content is the characteristic of the stratosphere which is much lower than 50ppmv. As result, the horizontal distribution is an average between the downwarding and spawning air parcels with the ones newly released.

Ln 490 Figure 5a 'STANDARD water vapor (sic) distribution agrees quite well with MLS' You're kidding, right? The next couple lines outline the quite large differences. In any event, it seems like some of the good agreement at 100 hPa due to STANDARD is that air rising through the monsoon has very high water vapour amounts. **Reply:** We agree that our wording here was too positive and misleading. And actually we did not expect a good quantitative agreement at levels below the tropopause (360K in the monsoon region, where the tropopause can be as high as 400K), as the freeze-out process has not been completed for many air parcels. We have deleted our comment.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1010, 2020.

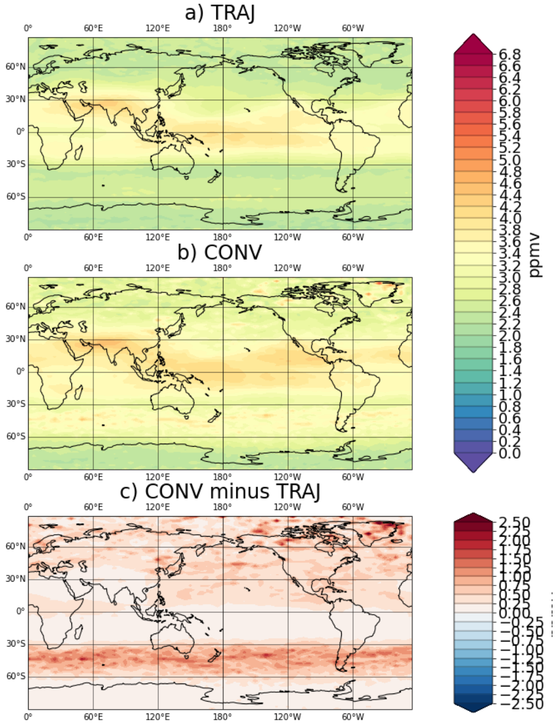

Fig. 1. Boreal summer distribution of water vapour at 100hPa in 2008 for a) TRAJ and b) CONV experiment and c) their differences.

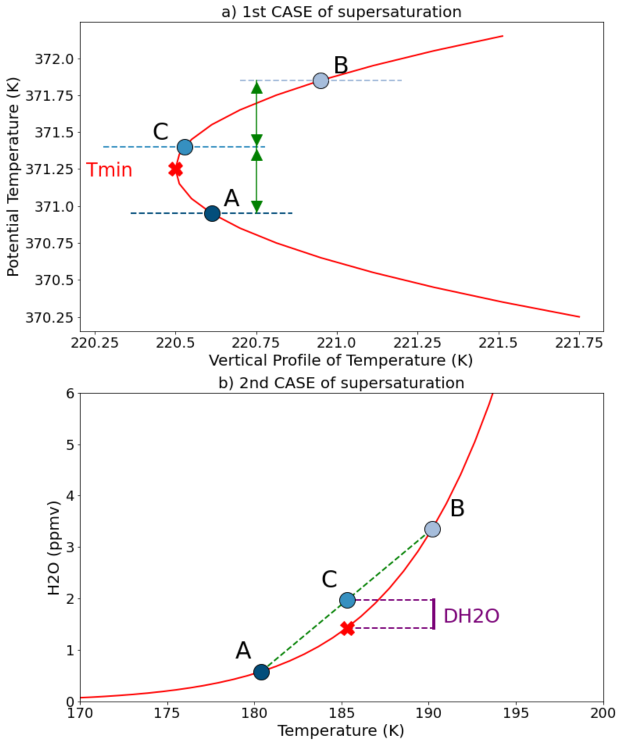

Fig. 2. Supersaturation caused by mixing due to (a) the occurrence of a local temperature minimum between the two mixed APs and (b) the convexity of the Clausius-Clapeyron relation.

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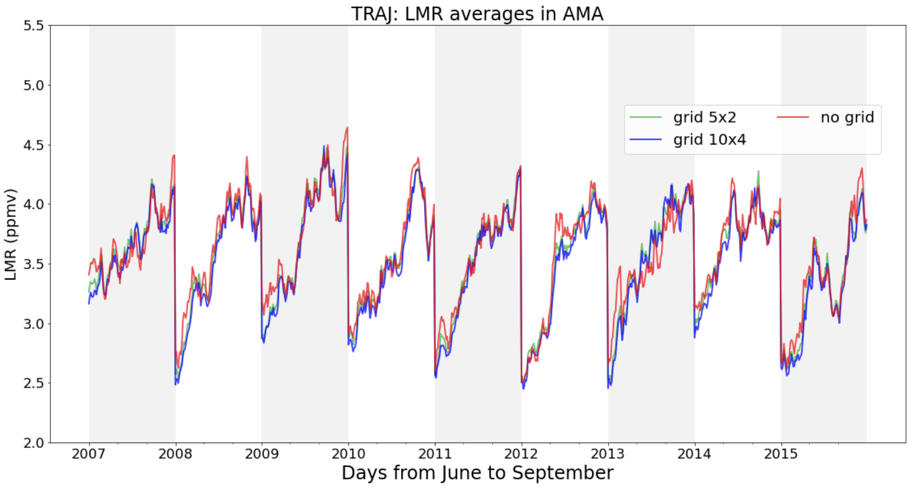

Fig. 3. Water vapour averages in the AMA performed by TRAJ using (red) non-gridded air parcels, (green) 5°x2° and (blue)10°x4° <lonxlat> binned air parcels at 100 hPa.

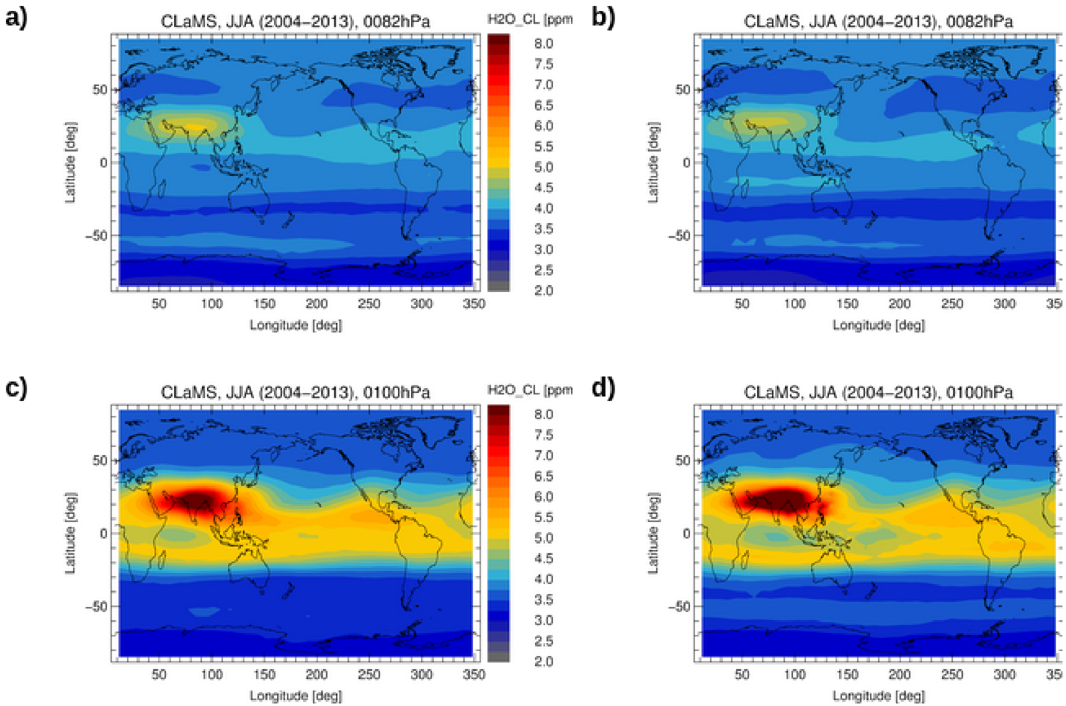

Fig. 4. Distribution of water vapour simulated by STANDARD (left column) using the averaging kernels of MLS and (right column) not using them at a-b) 82 hPa and c-d) 100hPa.

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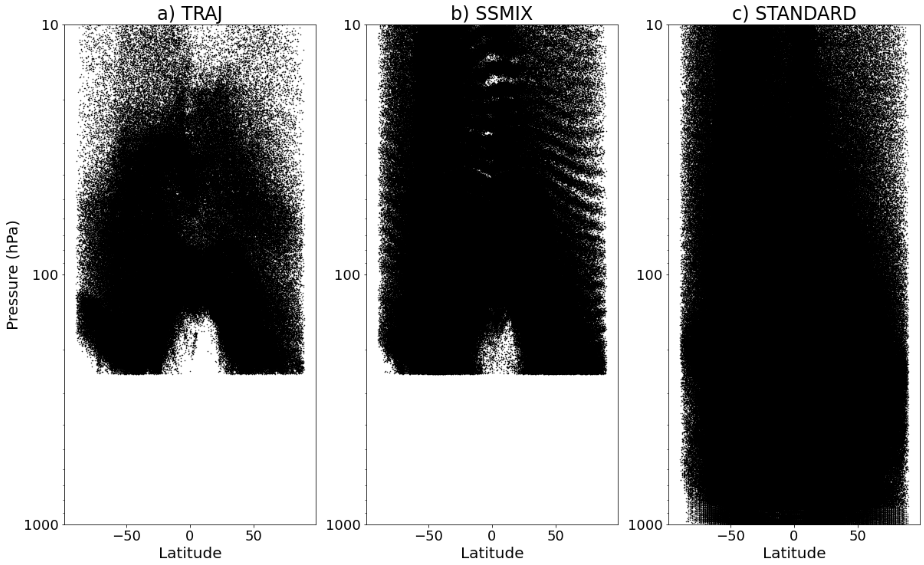

Fig. 5. Vertical distribution of air parcels present in simulation the 8th of July 2013 for a) TRAJ, b) SSMIX and c) STANDARD experiments.