Review of 'Processes influencing lower stratospheric water vapour in monsoon anticyclones: insights from Lagrangian modeling' by Plaza et al.

Overview: This paper uses the CLaMS trajectory model to fill the stratosphere with parcels which the authors call Lagrangian Trajectory Filling (LTF). This technique (when mixing is shut off) is identical to Forward Domain Filling (FDF) pioneered by Schoeberl and Dessler (2011). Using a series of experiments the authors investigate the water vapour anomalies associated with the monsoon regions comparing their results to those obtained by other authors using trajectory simulations.

Upper tropospheric and stratospheric water vapour can be tricky to simulate since convective sources, small scale temperature anomalies due to gravity wave and cloud formation processes all play a role – see Schoeberl et al. (2018) Fig. 3. CLaMS parameterization of mixing is unique and therefore brings an additional process to bear.

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 evapourate 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.

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 the 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.

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 ¼ 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?

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 in 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.

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 increase the water vapour in the models somewhat because of the strong non-linear vertical gradient in water in the upper troposphere.

Overall, I like the idea that CLaMS is introducing a new – and probably relevant process – into the discussion of water vapour and determining the impact over the monsoon. The lack of convective moistening provides us with no real insight on how important the mixing processes might be. Also, looking at Figure 2, it looks to me like mixing is adding too much water. Below I have made a large number of suggestions to improving this paper and I hope the authors make appropriate revisions and resubmit the manuscript.

Minor comments:
Ln 28 also reference Randel and Park (2019; JGR)

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.

Ln 78 ‘has not been assessed yet’ please see Schoeberl et al. (2018) Fig. 3

Ln 84 – what does ‘they’ refer to?

Ln 100 Please use the latest version of MLS. V4.2 is somewhat wetter than V5

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.

Ln 153 Assuming that LMR is defined by 100% RH? Please be specific.

Ln 168 Please elaborate on what the 'characteristic length' is? The loss of ice from a parcel per time step is ~ Ice*w_s*Δt/L where Ice is the ice mixing ratio, ws is the settling velocity, Δt time step and L is the cloud depth. Is L the characteristic length?

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 ?

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.

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.

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.

Ln 213 You should update to MLS V5

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.

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.

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 conservation of 2 CH4+H2O in the stratosphere.

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.

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 enhanceds convection. The authors should also re-read how convective influence is parameterized in Ueyama et al. (2018). Ice is added to parcels passing near convection 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 appropriately for parcels that have collided with convection. From ln 277 to ln 280 is pretty speculative.

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.

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.

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.

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.

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.

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.

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.

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.

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 correct 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.

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

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.