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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Convective hydration in the tropical tropopause layer during the StratoClim aircraft campaign: Pathway of an observed hydration patch" *by* Keun-Ok Lee et al.

Anonymous Referee #3

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Summary:

This article investigates an observed hydration patch in the area of the Asian Monsoon observed by the StratoClim aircraft campaign. The Meso-NH model is run over several days and accurately simulates the hydration patch and the proceeding convection. The simulation demonstrates the hydration patch can be attributed to convective influence from the days prior to the observation. I think this is an interesting case and the tracking back in time of the hydration patch to see how the atmosphere responds to the convective influence is compelling. However, this article also attempts to describe in more detail the physical and dynamical processes that explain the evolution of the at-

mospheric environment. I found much of this text to be poorly supported. The specific statements that need more support are listed below. Or the authors could choose to remove some of these statements because they can't be investigated with the current model (or time does not permit investigation). In that case, the authors can submit a revised article that is a much more concise case study of an interesting simulated hydration event.

Additionally, the authors fail to reference several articles on midlatitude hydration and/or convective transport events that seem relevant for this study. A non-exhaustive list of these articles are included in the comments below.

Comments on Process Statements:

1. The separation between the troposphere and stratosphere is identified as the 380 K level. This is a reasonable definition in the mean, but is an oversimplified definition at convection-allowing scales. I think this study clearly shows deep injection of water vapor by convection, but there should be some discussion of the difficulty of defining a tropopause in convective environments. For example, see:

Maddox, E.M. and G.L. Mullendore, 2018: Determination of Best Tropopause Definition for Convective Transport Studies. J. Atmos. Sci., 75, 3433–3446, https://doi.org/10.1175/JAS-D-18-0032.1

2. Using the tropopause definition above, a tropospheric tracer is defined to track mixing between the troposphere and stratosphere. There is the issue discussed above of an oversimplified tropopause definition. But more importantly, this is an odd approach to highlighting moistening from the troposphere. I expect the moisture content of the upper troposphere in this region is much lower than the moisture content of the lower troposphere. The approach used here has no way to distinguish between mixing of lower tropospheric (as transported by deep convection) or upper tropospheric air. Additionally, because the tropospheric air being tracked is right at tropopause level, a lot of the observed mixing may occur due to numerical diffusion, which tends to be stronger

than true atmospheric diffusion. (And even if the numerical diffusion is physical, may track mixing of air parcels that are not very different in terms of water vapor content). This issue can be seen in Figure 8 with the strong gradient that forms at all locations over time, which just looks like diffusion. It would be instructive to show what the tropospheric tracers looks like away from convective events to assess what portion of the vertical mixing is convectively enhanced and what portion is just diffusion. Figure 11 is not compelling. Finally, I don't know of any tropospheric chemical tracer that looks like that (Figure 8) across the tropopause.

Note: I do agree that the mixing is enhanced around the time of convection, but it's not clear if the mixing is of significantly different air masses, or just enhancing the diffusive processes in the vicinity of convection. Again, this problem is due to the fact that you don't know where in the troposphere this air is from. Many articles have used tracers to study deep convective transport, but are able to better distinguish source. For example:

Homeyer, C. R. (2015), Numerical simulations of extratropical tropopauseâĂŘpenetrating convection: Sensitivities to grid resolution. J. Geophys. Res. Atmos., 120, 7174–7188. doi: 10.1002/2015JD023356.

Mullendore, G. L., D. R. Durran, and J. R. Holton (2005), CrossâĂŘtropopause tracer transport in midlatitude convection, J. Geophys. Res., 110, D06113, doi:10.1029/2004JD005059.

3. The definition of ML and IL is based on height ranges. Line 294 states that "the temperature increases in both layers, indicating the mixing with the warmer stratospheric air." As shown in Maddox and Mullendore (2018; reference above), during the convective event, the tropopause can not be reliably defined, but we at least know that the tropopause surface is pushed upward during the event. "Mixing with stratospheric air" doesn't make sense here. If you are showing the influence of overshoots only, which are colder than the surrounding air, the layer temperature will go down. You may instead be seeing the tropospheric column heating occurring due to the convection (e.g.,

see comment in Maddox and Mullendore, p. 3438). Or this may be due to local heating due to ice particle formation.

4. Mixing pathways are not investigated sufficiently in this article to accept Figure 12 as a proven mechanistic diagram. The authors state several times that gravity waves are likely an important, but don't do any investigation into gravity wave overturning. At the resolution of this model, you should be able to observe steep isentropes in the vicinity of the overshoot that indicate gravity wave breaking. And I have detailed other concerns about the amount of mixing from diffusion above. I don't dispute that convection has played an important role in the observed hydration, and the article can still be published as an important case study. But Figure 12 is an overreach based on the analyses done here. I think it should be removed.

5. Besides sensitivity to the turbulent mixing parameterizations in this model, and possibly to the numerical diffusion (separate from turbulence parameterizations), other numerical sensitivities may be in play and are not discussed. The most significant of these is the sensitivity to the microphysical parameterization, which could have a significant impact on the hydration processes discussed.

Additional Comments:

line 35: "…Middle East and is located on the edge…"

line 43: "…relatively high at about 4.2 ppmv…"

line 56: "The most energetic one forms…" Energetic what? I assume you mean convective core, but this phrasing is clunky.

line 59: I realize your list of prior studies analyzing water vapor injection by convective overshoots is not meant to a complete list, but some recent articles are missing from your list and I want to make sure you have incorporated their findings into your assessment:

1. Homeyer, C. R., et al. (2014), Convective transport of water vapor into the lower

stratosphere observed during double- tropopause events, J. Geophys. Res. Atmos., 119, 10,941–10,958, doi:10.1002/2014JD021485.

2. Homeyer, C.R., J.D. McAuliffe, and K.M. Bedka, 2017: On the Development of Above-Anvil Cirrus Plumes in Extratropical Convection. J. Atmos. Sci., 74, 1617–1633, https://doi.org/10.1175/JAS-D-16-0269.1

line 79: "…that was measured by aircraft in connection to a convective overshoot."

line 80: "…and spaceborne observations as well as…"

line 160: The hydration patch is "chased" back in time: You need to include a more quantitative explanation of exactly how the location of the hydration patch was identified going back in time.

line 223-225: Looking at Figure 5, there do seem to be some isolated overshoots at 13 UTC, as there are some areas of very cold brightness temperatures.

Figures 6, 7, and 8: The small black lines are not labelled, but they look like wind vectors. These should be removed because they are not discussed, and they make these plots hard to read.

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