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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 #1

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Summary general comments:

This study examines the origins of an observed moisture patch to the south of Kathmandu, Nepal during the StratoClim campaign in 2017. The analysis was done using aircraft measurements, along with satellite observations and a large-domain Meso-NH convection-permitting simulation over 3 days to determine the source and evolution of the patch throughout its journey prior to being observed over northern India. Overall, this study shows overshooting convection hydrating a sub-saturated lower stratospheric region. While it is important to document a realistic case study, there are several queries that I would like the authors to address before this study can be con-

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sidered for publication. These mainly relate to the discussion of processes that govern the evolution of the hydration patch along its trajectory. The overall impression that I get is that the net moisture content of the advected plume decreases over time because of further mixing with tropospheric air, which is argued to dilute the patch along its journey. However, the authors do not show the time evolution of the mixing along the pathway and fail to systematically discuss the important role of ice microphysics in regulating the humidity of the patch within the moist layer (ML), specifically via the 'vapour scavenging' effect of ice.

Specific Comments:

Tracking the enhanced moisture patch over northern India back to Sichuan Basin, China. I would suggest to the authors that they do an offline HYSPLIT back-trajectory analysis with their hourly simulation results. In my mind, this would really help to confirm that the observed moisture patch/layer was indeed coming from where the authors state the overshooting convection was happening. I found it hard (and other readers may too) to visually track or 'chase back' (L160-161) what is plotted in Fig. 4 and reconcile with what is shown in the satellite images in Fig. 5. There are also other locations that appear to show overshooting tops (e.g., Fig. 5g, h). In other words, how do we robustly know that this is the same advected plume that originated over China from overshooting convection on 6 August and not influenced/added to by other convective overshoots along the way to northern India? The lat/lon extent in Fig. 4 is also smaller than Fig. 5. I would suggest the authors either expand Fig. 4 to match the lat/lon dimensions of Fig. 5 or draw a box in Fig. 5 denoting the extent of Fig. 4 to help orientate the reader.

In addition, it would be useful to draw wind vectors and pressure contours on the 410 K isentropic surface shown in Fig 4. This would help the reader get a sense of the synoptic upper-level flow that would be steering the enhanced moisture patch from the purported source in China towards northern India.

Tracer implementation and parcel origin of the enhanced moisture patch.

I found the implementation of the tropospheric tracer in the model study too simplistic. Specifying 1 for tropospheric air below the 380 K level and 0 above results in sharp vertical gradients near the boundary. This is what you see in Fig. 8a and Fig. 10a at 13UTC 6 Aug. These sharp gradients would then be numerically diffused over the course of the model integration, thus what you get is likely to be dominated by artificial smoothing and not by physical turbulent mixing, once the convective overshooting is over and the perturbed isentropes return to equilibrium (Fig. 8h-l). In addition, the formulation of the tracer makes it hard to properly distinguish whether the residual tracer amount came from lower, middle or upper tropospheric/TTL air. This is important in the context of understanding the nature of the vertical transport coming from overshooting convection. I think this is a caveat/shortcoming that should be acknowledged by the authors in their subsequent discussions in the text involving any reference to tracer concentrations.

For comparison, Hassim and Lane (2010) showed that their simulated enhanced moisture plume from overshooting convection in the tropical lower stratosphere was composed largely of TTL air and not lower tropospheric/boundary layer air. They imposed two different types of passive tracers (initial height scalar and initial water vapour mixing ratio scalar at each grid point) to form a smooth and continuous distribution in the vertical in order to minimise the numerical artifacts that occur near sharp gradients. An estimate of the parcel origin is then simply given by the perturbation value, indicating the degree of the vertical displacement of air and whether there is mixing between significantly different air masses.

[Hassim, M. E. E. and Lane, T. P.: A model study on the influence of overshooting convection on TTL water vapour, Atmos. Chem. Phys., 10, 9833-9849, https://doi.org/10.5194/acp-10-9833-2010, 2010]

Amount of water vapour injected by convective overshooting

L218-219: Are the authors able to quantify how many overshoots are simulated in domain 4e/4d over the 7 h period reported? A total of 6401 tonnes over a 7 h period implies that approximately 915 tonnes of water were injected every hour, on average. What would be the average amount per overshoot and how does this compare to other studies of single and multiple overshooting cores? Also, can the authors comment whether the convectively-injected amount they derive is sensitive to the model resolution. I suspect that this would be the case since you would get much wider updrafts at 2.5 km resolution compared to a <= 1 km grid spacing.

I also think that it is important to state in the text that the direct injection of water mass by convective overshoots occurs mainly in the form of ice, as the air within the overshooting turret is very cold and very dry. The moistening results from strong turbulent mixing of the ice-laden air with the entrained stratospheric air during the collapse of the overshooting top. The warm, subs-saturated stratospheric air causes the ice to rapidly sublimate into vapour at the top of the overshoot, moistening the layer. In lieu of the new higher parcel temperature (due to mixing), the enriched vapour layer then remains at this higher isentropic level after the overshoot collapses. The conditions and timescale at which this process occurs has been investigated by the second author in his 2018 paper and should be highlighted when describing Fig. 6 and Fig. 7 later, and in section 4.2.1.

L220-221: Is there a reason why the authors simply integrated the water vapour and not the water vapour anomaly (relative to the initial profile) between the CPT (17.8 km) and 22 km? Wouldn't this result in an overestimation? I would also contend that the net hydration analysis is better done on isentropic levels since, in the absence of diabatic processes, air parcels would remain and travel along a given isentrope determined by their resultant potential temperature.

L247, L257, L270: The small tracer concentrations mentioned here and at other locations in the text when discussing the ML and IL are likely to be numerical artifacts rather than from vertical transport.

L248: Insert the word ‘and’ between ‘UTC’ and ‘even’.

L263-278: I feel that the content of the sub-section does not really reflect its heading and does not tell me anything about the advection of the hydration patch. Some of the content describing the amount of water vapour and tracer content is better suited in the preceding sub-section, or perhaps better absorbed into Section 4.2.

L283: Do state the extent of the domain/s that you are averaging along the pathway of the hydration patch. Are they the same ones shown in Fig. 4?

Mixing processes affecting the overshoot and the hydration patch

L290: Shouldn’t the figure reference at the end of the sentence be Fig. 9, not Figs. 6 and 7?

L293: Replace the word ‘even’ with ‘and’.

L297-298: The difference between the yellow and green lines in Fig. 10a are likely dominated by numerical diffusion in the model acting to smooth the sharp gradients.

L300: ‘increases of 5

L303 onwards: In discussing Fig. 11, are the authors confident that they are sampling the same tracked air parcels at 12UTC 7 Aug (11c) and at 06UTC 8 Aug (11d), as those sampled initially on 21UTC 6 Aug (11b)?

L308: Shouldn’t ‘very small compounds of stratospheric air’ be ‘very small compounds of tropospheric air’ since you are referring to tracer content being less than 0.1

L321: This sentence sounds awkward to me. Consider revising. Also, what could be the cause of the mixing at the later times beyond 00UTC 7 Aug and the isentropes relax back to equilibrium after the overshooting stops? Also, Fig. 9b suggests that there is still a low concentration of cloud ice in both ML and IL long after the snow and graupel sediment out. The continued presence of cloud ice in ML suggests that the ice may have formed in-situ in response to wave-driven temperature oscillations that locally drive the RH to ice saturation. This would suggest that ice microphysics could be playing a pivotal role in controlling the eventual moisture content since ice nucleation and the subsequent growth process would slowly deplete the layer of water vapour.

L326: Insert ‘is’ between ‘The’ and ‘probably’. Again, what domain is the analysis in Fig. 9 performed for?

L333-342: I’m not fully convinced that turbulent diffusion is the sole cause of the decrease in water vapour content in ML and IL. For reasons above, vapour-scavenging by ice nucleation and growth within ML and IL might also explain the reduction in water vapour amount. I would recommend plotting Fig. 10b-d as anomalies with respect to the background initial profile at the various locations along the trajectory. This would allow the authors to properly discern whether net moistening or dehydration took place at the different levels and allow the dominant processes to be elucidated better. For Fig. 10e, it would be useful to also plot just cloud ice as dashed lines for the stated times.

L350: Should the figure reference be ‘10d’ and not ‘11d’?

L351: At which location along the trajectory of the hydration patch is cold tropospheric air being entrained to reduce the ML and IL temperature? It could just be that the patch encountered colder background temperatures on 06 UTC 8 Aug.

L361: ‘. . .decreases from 9.6 to below 6.2 ppmv. . .’

L363-364: I would argue that the reduction in ice content is largely due to sedimentation and not sublimation due to mixing as the water vapour content is also steadily decreasing with time. Only a fraction of the ice mass transported into the TTL will be sublimated before most of the ice sediments out.

L365: This statement is highly speculative without showing any profiles of TKE. By 012 UTC 7 Aug, the isentropes are back to a relaxed position and not perturbed further by overshoots.

L366: '. . .in excess of. . .'

L368: What do you mean by 'wind shear layer'? I also do not see a straight upward profile in temperature on 06 UTC 8 Aug.

Additional Comments:

L22: Change 'still remains' to 'remained'. Remove 'ASL' as the word altitude implies that it is above sea level.

L23-24: The two sentences here sound awkward in their construction. Suggest authors re-word them for clarity and flow of meaning.

L24-25: There are issues with the way the tracer is implemented for this statement to be robust and valid, without at least accounting for the role of ice microphysics, specifically in-situ ice nucleation and growth, which would deplete the water vapour content.

L35: '. . .and the Middle East, and is located. . .'

L43: '. . .high at about 4.2ppmv. . .'

L56: '. . .the most energetic air parcels form. . .'

Fig. 2: It would be useful for the reader if the background water vapour content value is shown by a vertical reference line in 2(a).

Fig. 3: Label (c) and (d) when captioning the 'ice content (eq. ppmv)' and 'water vapour (ppmv)', respectively.

Fig. 5: I suggest that the authors draw lines on the various panels to denote where the Fig. 6 cross-sections for the various times are taken from. Do state in the caption what the white arrows are meant to show.

Fig. 5: What is the small inset box in Fig. 5g and 5h?

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Fig. 6: It is hard to see the cloud boundaries in the figure as they are the same colour as the vector winds, which are not discussed at all. I suggest you plot the cloud boundaries as a thick white contour to stand out in Fig 6. Also, do state in the captions that the arrows plotted denote vector winds and discuss them in text; otherwise remove.

Fig. 9c: Change 'temerature' to 'temperature' for y-axis label.

L373: Change 'pathways' to 'pathway'.

L375: Change 'convective overshoot' to 'overshooting convection'.

Fig. 12 is missing critical components related to ice microphysics such as ice nucleation, growth and sedimentation that ultimately govern TTL humidity through the balance between moistening (from sublimation) and depletion (through scavenging).

L407-408: From a microphysical perspective, how does turbulent mixing cause water vapour to be partly deposited?

L413-414: I think this statement incorrect since average water vapour content is decreasing as shown by Fig. 9a and Fig. 10d, and not increasing.

L421-422: The sentence here is poorly constructed and its meaning is hard to understand.

L425-427: The statement here is a little far-fetched. After re-reading through section 4 again, the text lacks adequate and systematic discussion on the relative roles of transport, mixing, and ice microphysics to determine which process dominates (c.f. Hassim and Lane 2010).

L433-434: I think the value quoted here is an overestimation due to the way it is calculated.

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