Review of "Controls on the water vapor isotopic composition near the surface of tropical oceans and role of boundary layer mixing processes"
by Camille Risi et al.

This paper presents an analytical steady state vertical mixing model to investigate the controls on the water vapor isotopic composition in the subcloud layer over the tropical oceans. It is a nicely simple model that considers the most important processes, which are surface evaporation and vertical mixing and predicts the subcloud layer water vapour isotope composition from a combination of mass balance equations for all isotope species. I enjoyed reading this paper very much, I particularly like the approach chosen for testing this analytical model, which combines model simulation data and ship-based observations. The ideas presented in this paper are exciting and very valuable for upcoming large field campaigns in which isotope observations are planned in different parts of the lower troposphere.

I thus recommend minor revisions with the following minor points:

1) In the abstract it should be clearly stated that the proposed analytical model is a steady state formulation, which neglects horizontal gradients and thus the impact of horizontal advection.
2) P. 1, L. 10: "When the air mixing into the SCL is lower in altitude it is moister": I think this is true most of the time and certainly in a climatological sense, but of course when including differential advection elevated moist layers can occur. I guess adding "it is generally moister" would make me very happy.
3) P. 3, L.4: Shouldn't cloud top cooling be mentioned here as well?
4) P. 5, L. 4: Neglecting the large-scale horizontal gradients in air properties, particularly in the trade wind regions seems to me like a strong assumption. Given the sensitivity of δD to SST and the considerable SST gradient across the North Atlantic, I find that this caveat could be discussed a bit more explicitly here.
5) P. 5, L. 20: "qs is the saturation specific humidity at SST"
6) P. 6, L26: In the closure section and the discussion of the free tropospheric profile the role of horizontal advection is again neglected. This is maybe a good assumption in the tropic but it should still be mentioned explicitly.
7) P. 8, L26: "Depending on microphysical details that are too complex to be addressed here", Graf et al. 2019 could be referenced here
8) P. 9. Fig. 3: Which value was chosen for the SST? This could be mentioned in the caption as well as a reference to which equilibrium fractionation factor was used
9) P. 12, L. 2: "However, if the end member is defined below 500 hPa (e.g. 600 hPa), results are not always reasonable", why is this so?
10) P. 12, L. 7: In my opinion, this makes it difficult to interpret rorig. But probably there are conditions when rorig and thus zorig are more physically meaningful than others. Could the authors maybe add a list with explicit and quantitatively expressed conditions in which they would argue that the assumptions involved in Eq. 9 are satisfied?
11) P. 13, L. 4-5: Is there a literature reference that the authors could indicated for this calculation of zi from observations?

12) P. 13, L. 15: I did not immediately understand what was meant by composites belonging to a given interval of omega500, I was expecting a map. A reference to the results figure referred to here would have helped me.
13) P. 13, L. 22: By how much (range of variability) were the four factors varied?
14) P. 14, Section 4.1: This section seemed very technical for me. I also see it more as a methodological aspect than a result. I would recommend to either shift it to a technical appendix (since the paper is quite long) or to the methods section.
15) P. 15, Fig. 7: Mention that these are different random (?) grid points in the caption. I would have liked a more general evaluation also describing the temporal and spatial variability in the vertical profiles simulation by LMDZ.
16) P. 15, L. 3: could the authors mention the region where they think that alphaeff may also reflect horizontal advection effects?
17) P.16: I find it interesting that the mixing and Rayleigh lines have large biases in front of the eastern continental boundaries where the inversion is strongest and, where there is a strong decoupling between the FT and BL. In particular in these regions, I would expect horizontal advection to play a key role, (e.g. the SAL layer in front of the eastern North African Coast, see Lacour et al. 2017, ACP). Maybe the authors find a good way to shortly note this in the text.
18) P. 17, L. 2: Maybe one could add **oceanic** upwelling and **atmospheric** deep convection. Jumping from upwelling to deep convection in the same sentence, I was not sure whether deep convection in the ocean or the atmosphere was meant here.
19) P. 17, L. 4: “Decreases as omega500 is more strongly ascending or descending” -> “with increasing vertical winds (omega500) of both signs”
20) P. 17, L. 11: “in more ascending regions” -> a reference to Fig. 8d would have helped me here.
21) P. 17, L. 25: “The fact that the effect…” I had difficulties to understand this sentence.
22) P. 17, L. 33: h0 (62%) is the largest explained fraction of all the variables considered and should thus be put first. This could be a hint that large-scale horizontal advection plays an important role at the synoptic timescale in these regions.
23) P. 19, Fig. 10: The bin sizes (number of data points per bin should be added). If I understood correctly from the caption, the authors used the seasonal averaged fields from LMDZ. Why not making these composites using the 6-hourly outputs? For me there is a timescale discrepancy between the processes (mixing, evaporation) that the authors look at and the averaging timescale of the used fields.
24) P. 27, L. 28: a reference to a more technical paper such as Aemisegger et al. 2012 AMT, would be nice here.

Small technical comments:

1) P. 2, L. 22: “suffers **from** a low bias”
2) P. 3, L. 28: “capturing **the** second-order…”
3) P. 8, L. 26: no parenthesis after B)
4) P. 12, L. 7: “based” -> “b**i**ased”
5) P. 15, L. 1: Figure 8d
6) P. 15, L. 4: “Values **of** alphaeff…”
7) P. 15, L. 5: using a fractionation coefficient alpha eq **as** a function of temperature”
8) P. 17, L. 14: space missing between rorig and (
9) P. 18, L. 1: “Overall, **the** results…”

10) In general, the authors do not consistently use B15 for Benetti et al. (2015)
11) P. 21, L. 2: “with **the** strongest inversion”
12) P. 27, L. 27: measurement errors
13) P. 28, L. 2: “if **we** measure…”
14) P. 29, L. 14: very precise
15) P. 30, L. 11: **from** which altitude the air comes

References:

Aemisegger, F., Sturm, P., Graf, P., Sodemann, H., Pfahl, S., Knohl, A., and Wernli, H.: Measuring variations of $\delta^{18}O$ and $\delta^{2}H$ in atmospheric water vapour using two commercial laser-based spectrometers: an instrument characterisation study, Atmos. Meas. Tech., 5, 1491-1511, https://doi.org/10.5194/amt-5-1491-2012, 2012.

Graf, P., Wernli, H., Pfahl, S., and Sodemann, H.: A new interpretative framework for below-cloud effects on stable water isotopes in vapour and rain, Atmos. Chem. Phys., 19, 747-765, https://doi.org/10.5194/acp-19-747-2019, 2019.

Lacour, J.-L., Flamant, C., Risi, C., Clerbaux, C., and Coheur, P.-F.: Importance of the Saharan heat low in controlling the North Atlantic free tropospheric humidity budget deduced from IASI δD observations, Atmos. Chem. Phys., 17, 9645-9663, https://doi.org/10.5194/acp-17-9645-2017, 2017.