Review of De Vries et al., submitted to ACP

Title: Stable water isotope signals in tropical ice clouds in the West African monsoon simulated with a regional convection-permitting model

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General comments :

This manuscript investigates the processes setting the isotopic composition of vapor and ice in tropical ice clouds simulated by a regional convection-permitting model. This study is the first to use an isotope-embedded convection-permitting model for that purpose. The authors find five key processes in tropical ice clouds that can be distinguished based on their isotopic signature, and which are summarized in the last figure. I find that the manuscript is well written and constitutes an interesting contribution. I therefore recommend publication after minor revisions.

Specific comments :

The evaluation section is focused on the comparison of precipitation to GNIP observations. But, as the authors mention, the isotopic composition of precipitation is set by post-condensation processes, and therefore it doesn't bear a direct link to the isotopic composition of vapor and ice in the upper troposphere, which is the focus of the present paper. I think the authors should mention this more clearly.

In Figure 2(j), I suggest the authors use some satellite dataset of precipitation instead of GNIP to perform basic comparison with the model. One example would be TRMM/GPM. Alternatively, the authors could use the precipitation analysis from ERA5.

I have a comment concerning the budgets in the TTL at section 5.3 and the water budget displayed at Figure 15. While the partitioning of water discussed by the authors is interesting, it does not directly address the issue of the water budget in the TTL, since it does not address the underlying fluxes, or how the isotopic information gives specific insights on that issue. I suggest the authors merge this section with the previous one and use Figure 15 to help with the interpretation of Figure 13 instead.

Technical corrections :

Lines 168 – 175: "Fractionation is parameterized using equilibrium fractionation factors with respect to liquid water and ice, following Majoube (1971) and Merlivat and Nief (1967), respectively. Non-equilibrium fractionation effects occur, for instance, if the air is supersaturated with respect to ice, which is taken into account by a combined fractionation factor" I find the formulation a bit confusing as it seems to suggest that fractionation is parametrized overall as an equilibrium process, before mentioning the parametrization of non-equilibrium effects. I suggest reformulating: "Fractionation at thermodynamic equilibrium is parametrized using equilibrium fractionation factors […]. Non-equilibrium effects are taken into account by a combined fractionation factor […]. Such effects occur, for instance, if the air is supersaturated with respect to ice." I also suggest you mention that non-equilibrium effects arising from ice surface kinetics (Nelson, 2011) are neglected in the model.

Lines 280 – 282: “At this resolution, there is no clear difference between the parameterized and explicit convection setup as EXLP14 performs better than PAR14 in June, while no clear differences between both simulations emerge in July”. Please repeat the resolution for clarity: “**At 14 km resolution**, there is no clear difference between the parameterized and explicit convection setup as EXLP14 performs better than PAR14 in June, while no clear differences between both simulations emerge in July”

Line 316: Typo. “Figure 4c shows the deviation of the isotopic composition of ice (δ2Hice as in Fig. 4b) from the ice that would **form** from local vapour under equilibrium fractionation”

Around line 317: I suggest you mention that disequilibrium in ice can be produced both as a result of non-equilibrium conditions at fractionation and/or because of the lack of diffusive exchanges between vapor and ice, which allows enriched ice lofted from below to persist at higher levels in the atmosphere.

Lines 350 – 353: I realize that the model probably doesn’t have a Wegener-Bergeron-Findeisen effect in mixed-phase cloud layers. Such an effect would also introduce disequilibrium in ice. This should be mentioned somewhere, probably in Section 2.1

Lines 463 – 465: “As already explained in Sect. 4.1, this signal stems from the lower equilibrium fractionation factor of condensation than that of vapour deposition.” You could add “[…], **thus resulting in liquid water being isotopically lighter than ice**.”

Lines 489 – 494 and thereafter: The formulation of ice cloud categories as “convective”, “mixed” and “cirrus” is a bit misleading in my opinion. IWP discriminates between regimes over the entire depth of the troposphere, not individual clouds. For instance, in line 520, you mention “cirrus clouds” and “lower troposphere” in the same sentence, which contradicts the ISCCP classification of cirrus having cloud top pressure less than 440 hPa. I also think that the term “mixed” is misleading because it could be interpreted in the sense of “mixed-layer”. I suggest you use the denominations “convective regime”, “anvil regime” and “thin cirrus regime” when classifying the regimes by IWP, and consistently modify everywhere.

Lines 573 – 575: “In the upper troposphere and the lower TTL (125-200 hPa), deep convection contributes to more than 40 % to the total water budget” You should reformulate as “More than 40% of total water is within the deep convective regime.” Your initial formulation could be interpreted as saying that total water even outside of deep convection bears a convective origin, which is not what you show here. Again, I think this section could be merged with the previous one to help explain the results of Figure 13.

Line 637: “Deep convection, although only occurring at about 3.8 % in time and space, contributes to about 40% of the total water budget” Same here, I would suggest: “Deep convection, although only occurring at about 3.8 % in time and space, **contains about 40% of total water**”

Lines 683 – 686: I suggest you write “alpha_eq(T) = **R_ice,eq** / R_vap > 1” to distinguish between the values of R_ice at a particular level and **R_ice,eq** entering the definition of alpha_eq. At line 686, you should write that R_{cloud ice} is **approximately equal** to alpha_eq R_vap, since kinetic effects can induce deviations, especially in strong updrafts where supersaturated conditions may prevail.

Line 697: Again, **R_ice is approximately equal to alpha_eq R_vap** since non-equilibrium conditions can occur, as you mention. Besides, this relationship applies here because you assume bulk equilibrium between ice and vapor, as expected under in situ formation conditions. Otherwise, when ice crystals are grown from the nucleus, fractionation equilibrium only applies between the surface of ice crystals and ambient vapor, since diffusive exchanges cannot take place with the inner part of ice crystals. This condition of bulk equilibrium should be stated.

Figures 12, 13, 15: I would use the denominations “convective”, “anvil” and “thin cirrus” for the type classification, and use “cloud regime regions” instead of “cloud type regions” in legend.

References:

Nelson, J. (2011). Theory of isotopic fractionation on facetted ice crystals. *Atmospheric Chemistry and Physics*, *11*(22), 11351–11360.