Review 2

Dear reviewer,

Thank you for your careful review of our paper. We have mostly made all the corrections requested for the text and the figures as explained below in blue. Changes are tracked in red in the revised version of the manuscript.

Assessment: The paper does its job well, introducing the model, its implementation and performance in a demonstration simulation, while highlighting the water isotopic signals in rain evaporation and how they relate to different regions/phenomena within squall lines. I have some questions about details of the isotopic implementation below, specifically the Bergeron process and the kinetic fractionation of deposition onto ice, and ask for clarification from the authors.

Reference: Looking at this analysis of rain evaporation made me think of the bin model implementation of H218O in Hiron and Flossman (2020, https://doi.org/10.1029/2019JD031753). They have a plot of isotopic composition as a function of droplet size in their figure 8. Not sure that paper needs to be cited here, but I find it interesting to think about how the broad range of radii in the rain category of a bulk scheme might have a variety of isotopic compositions in reality.

Thank you for bringing this article to our attention. This reference has been added in the introduction, in the list of models that contains a stable water isotope scheme. The bin approach for microphysics, as in the DHARMA model, is undoubtedly an advantage for accounting for different isotopic compositions depending on particle size, but it comes at a very high computational cost.

In the new version of the manuscript, the microphysics of the different models that integrate a stable water isotopologue scheme is discussed: “The DHARMA cloud model integrates the evolution of $HD^{16}O$ within a bin microphysics scheme explicitly representing cloud condensation nuclei, water droplets, and ice crystals using 16 mass bins, as described by Smith et al. (2006). The 2-moment bin mixed-phase microphysics scheme DESCAM has recently been extended to simulate the evolution of oxygen-18 within a 1.5D dynamical framework (Hiron and Flossmann, 2020). Such bin microphysics schemes are undoubtedly an advantage for accounting for different isotopic compositions depending on particle size, but it comes at a very high computational cost. Thus, DHARMA and DESCAM typically track only one water stable isotopologue in addition to light water species. On the other hand, numerical models employing bulk microphysics schemes usually simulate the evolution of both $HD^{16}O$ and $H_2{}^{18}O$. This includes models such as SAM-ISO (Blossey et al., 2010), COSMO-ISO (Pfahl et al., 2012), WRF-ISO (Moore et al., 2016), and, more recently, NICAM-WISO (Tanoue et al., 2023). All these models incorporate the evolution of the two isotopologues into a one-moment bulk microphysics scheme. The microphysics schemes in SAM-ISO, WRF-ISO, and NICAM-WISO typically describe the evolution of water vapor, cloud water, rain, cloud ice, snow, and graupel mass. However, graupel is not considered in COSMO-ISO.”

4/85: The arrow representing the Bergeron process in Figure 1, which connects cloud liquid water directly with cloud ice, causes some concern. My understanding of the Bergeron process is the transfer from cloud liquid water to cloud ice occurs through the vapor phase due to the differing saturation vapor pressures of liquid and ice. Cloud liquid evaporates (presumably at saturation with liquid and in isotopic equilibrium, since the scheme uses

saturation adjustment), and deposition onto ice occurs in supersaturated conditions with fractionation as described in section 3.2.3. If this is the way that the Bergeron process is implemented, please adjust the arrow in Figure 1. If not, please explain in the reply how the Bergeron process is implemented and whether that representation of the process differs from what I've described here.

You are right: the way the Bergeron process is treated in the model was not clear. In the new version of the manuscript, we added a paragraph to discuss this process: “Concerning the Bergeron process (BER), we assume it does not fractionate. The Bergeron process is the evaporation of cloud droplets followed by the ice deposition in mixed-phase clouds, when the air is subsaturated with respect to liquid and supersaturated with respect to ice. Since ice deposition in supersaturated conditions is associated with kinetic fractionation, the Bergeron process is often assumed to fractionate (Ciais and Jouzel, 1994). When the Bergeron process is at play in convective updrafts, it acts to enrich the vapor and decrease its d-excess at altitudes above the 0°C isotherm (Bolot et al 2013). In ICE3, the mass transfer rate from cloud droplets to ice crystals, is assumed to equal the water vapor deposition rate onto pristine ice crystals, which in turn equals the cloud droplets’ evaporation rate. This keeps the water vapor reservoir neutral. Therefore, we ignore fractionation during the Bergeron process, as in Blossey et al. (2010). Due to this assumption, we may slightly overestimate the isotopic depletion and the d-excess in the upper troposphere. This is not an issue for science applications focusing on surface precipitation or low-level water vapor, but future refinements might be required for applications in the upper-troposphere.”

7/168: The exponents in the formula for dynamic viscosity should be "-5", not "5" as is shown in the pdf.

We are sorry for this error. It is corrected.

8/191: Add "equilibrium" when describing alpha_l/v

This is done.

8/195: Couldn't c^{'iso} be described more simply as "c^{'iso} = c' (D/D_iso)^(1/3)"? This simpler definition might help the reader focus on the differences between the light and heavy water equations.

You are right. This is done.

sec 2.3.2-2.3.3: I'm curious about how the ventilation factor affects the kinetic fractionation of water isotopes as a function of drop or particle size. If we defined alpha_k = D_iso*f_iso/(D*f) = (Diso/D)^n, the exponent n would tell us how strongly the different diffusivities impact the isotopic transfer between vapor and condensate across those particle sizes. Presumably, n~1 for small particles and decreases from there. Stewart (1975, https://doi.org/10.1029/JC080i009p01133) suggests such a formulation in the paragraph after their equation 1.

Eq. 25 that describes the evolution of raindrop mixing ratio for heavy isotope water, is

integrated over the raindrop particle size distribution (PSD) since ICE3 is a bulk microphysics scheme. Therefore, diffusion and kinetic effects are not explicitly treated for each particle size. The bulk effect of particle size is expressed in M(1) and M((d+3)/2) which are proportional to λ^{-p}. λ is the slope parameter of the size distribution. A high λ means a narrow PSD and numerous small particles. On the contrary, a low λ means a large PSD and presence of large particles. Then, for large raindrops, the ventilation effect increases (as shown in Pruppacher and Klett (1997) for example). Since $\chi_{iso} > \chi$, we would expect that $f_{iso} > f$.

If $\alpha_K = D_{iso}*f_{iso}/(D*f) = (D_{iso}/D)^n$, for small ice crystals, $f = f_{iso} = f_0 = 1$, then n=1. For larger particles, since $f_{iso} > f$, n is supposed to decrease. Jouzel and Merlivat (1984) neglected the influence of ventilation for water vapor deposition on ice crystals. In their appendix, they computed the f/f_{iso} ratio for columnar ice crystals: f/f_{iso} is a decreasing function of the length of the column, and f/f_{iso} values are very near to 1 with $f < f_{iso}$.

Short paragraphs have been added in the new version of the manuscript to discuss this effect (at the end of Sections 3.3.2 and 3.3.3).

9/eqn 31: I compared this formulation of alpha_k with equation B26 in Blossey et al (2010), whose b and S_s seems equivalent to the present manuscript's F and (S_i+1), respectively. The two formulas don't quite match, making me wonder whether the (S_i - 1) terms in the numerator and denominator of equation 31 should actually be (1 + S_i), since S_i is defined as the supersaturation over ice just after equation 26. Please double check. If I've misunderstood something, please do clarify. I'm trusting that this is coded correctly in the code regardless.

You are right: there is typo in Eq. 31. F in our manuscript and b_s in Blossey et al. (2010) are equivalent. The only difference is that we use $e_{si}(T)$, the saturation vapor pressure over ice, while in Blossey et al. (2010), the saturation vapor density over ice $\rho^{*,s}$ is used, with $\rho^{*,s} = e_{si}(T) / (R_v T)$, R_v being the specific gas constant for water vapor. But, in our manuscript, since S_i is the supersaturation over ice, we should have written (S_i+1) instead of (S_i-1). Equation 31 has been corrected. It is now consistent with Eq. B26 in Blossey et al. (2010) and Eq. 14 in Jouzel and Merlivat (1984). We checked the code and it's written correctly.

10/246: Move the sentence about open boundary conditions to the end of the paragraph on lines 239-240, since (in my mind) the boundary conditions should be included in the description of the model domain. Could the impact of inflow at the boundaries on water and its isotopes be described briefly? Something like: "Where inflow occurs at the domain boundary, the temperature, water vapor mixing ratio and its isotopic composition are taken from the values in the initial soundings at that height." I think an additional sentence would make more clear the impact of the open boundaries. Also, I didn't quite understand what was meant by "There is no lateral advection." Perhaps, clarifying that would also help the reader.

You are right: this paragraph was not clear and has been rewritten. As in Caniaux et al. (1994), we use open boundary conditions in the x-direction, meaning that there can be inflows and outflows of isotopologues. In this idealized case study, the large-scale field of water isotopologues is similar to the initial field (as described in the first paragraph of Section 4.2). As you suggested, we have added a sentence to explain what open boundary conditions means.

10/sec 4.3: While the water vapor mixing ratio and the isotopic composition of precipitation do reach a quasi-steady state from hours 6-8, the rain rate itself is changing with time (by about a factor of two) during this period. While the quasi-steady-state of some variables is interesting, the simulation isn't really in steady state, so perhaps a different name for section 4.3 would work better, maybe "Quasi-steady-state regime" or "Mature squall line behavior" or something like that?

We agree. We have replaced the title of the section by: “Evolution of rain and water vapor isotopic composition along the 8-hour simulation”

12/290: "Fig. 4c and _4d_". In the figure itself, the fourth panel is labeled "e", which is consistent with the text but surprising to this reader.

The fourth panel in Figures 5 (formerly 4) and S3 is now labeled “d)” instead of “e)”. The text has been modified accordingly.

13/305: Is this the first time (\delta^{18}O_p)_{eq} appears? I feel that this variable and (d_p)_{eq} appear so often in this paper that they deserve numbered equations defining them, perhaps back in section 3. I realize that equation 11 does this implicitly, but for the uninitiated, perhaps the definition could be made explicit.

The definition of $(\delta^{18}O_p)_{eq}$ has been added in this section, the first time it appears in the text. It can be calculated from: $\alpha_{l/v} = (\delta^{18}O_v+1)/((\delta^{18}O_p)_{eq}+1)$.

14/316: The diffusivities of HD^16O and H_2^18O differ but only slightly, by about 3 per mille of each other (e.g., Merlivat and Jouzel, 1979, bottom of p. 5030). The larger fractionation factor of HDO relative to H218O makes it likely that the jump in isotopic composition (expressed as delta) between rain and vapor is larger (by roughly that factor of eight) for HDO, leading to fluxes that are roughly eight times larger, with a slight modification because D_HDO > D_H218O. Perhaps, this could be rephrased in terms of fluxes rather than as "eight times faster.

Actually, we have removed at the end of Section 3.1 the link between deuterium excess in precipitation and evaporation.

14/317-319: Re "rain evaporation leads rain to be more enriched ..." Graf et al (2019, https://doi.org/10.5194/acp-19-747-2019) nicely talks about this and could be a good reference here or wherever the d18O_p - (d18O_p)_eq quantity is introduced, because Graf spends a lot of time thinking about how that and the d_excess equivalent behave.

This is done. Graf et al. (2019) is now cited in Section 4.5.1.

18/373-376: Lee and Fung (2008, https://doi.org/10.1002/hyp.6637) analyze how much the

near-surface layer will affect the isotopic composition of precipitation to understand its dependence on drop size and rain rate. Perhaps, such arguments could apply here.

This short paragraph has moved in Section 4.5.3 to avoid repetition and Lee and Fung (2008) is now cited.

19/Fig 8: Please add thin, possibly dashed line showing the zero values in panels a-c, including a vertical one in panel c. This would make it easier for the reader to see the sign changes of disequilibrium and enriching/depleting rain-vapor exchange. If the authors don't like lines, at least add tick marks on the right-hand and possibly top axes.

Dashed lines have been added in Figures 9 (formerly 8) and S6 to show the zero values.

20/Fig 9: Do the high evaporation ratios happen only in regions where the mixing ratio is very small?

No, high evaporation ratios do not happen only in regions with a small rain mixing ratio. The high evaporation ratios occur where the precipitation flux is small and where the relative humidity is low. This is especially the case at the end of the stratiform region where almost 100% of the rain flux evaporates before reaching the ground (Fig. 10a). Figure 10 (formerly 9) has been revised and now consists of two panels. In panel (a), isolines (30, 60 and 90%) for the vertically integrated evaporation fraction are also plotted to illustrate the cumulative effect of precipitation evaporation. Panel (b) shows how the high evaporation of rain at low relative humidity drives rain enrichment and stronger isotopic disequilibrium. The text has been modified to account for the change in this figure.

21/431: "... despite the non-isotopic steady state of the near-surface water vapor ..." As noted above in my comment about section 4.3, I don't think the transient behavior of the isotopic composition of near-surface vapor is a problem. I would suggest rephrasing in a more neutral tone: "While the isotopic composition of near-surface vapor continues to evolve late in the simulation while the water vapor mixing ratio and the isotopic composition are quasi-steady, the isotopic results are actually ..."

That works for us. We have modified the text as follows: “... while the isotopic composition of near-surface vapor continues to evolve late in the simulation while the water vapor mixing ratio and the isotopic composition of precipitation settle into a quasi-steady-state, the isotopic results …”

21/435: "minor problem". As above, I don't think this is a problem.

We have changes as follows: “... at the end of the simulation does not prevent us from exploring the isotopic variations of water in our simulation.”

SI/Fig S1: This is repeated in the pdf I have. Please check.

In the supplementary material downloaded from the EGUsphere website (https://egusphere.copernicus.org/preprints/2026/egusphere-2026-548/egusphere-2026-548-supplement.pdf), there is only one Figure S1.

SI/Fig S2: Could additional panels be included showing the d18O and d excess of all non-precipitation condensed water? This could strengthen the argument about why the precipitation just below the melting layer is more depleted in the stratiform region.

An additional figure showing the $\delta^{18}O$ and d-excess for all hydrometeor species has been plotted and added in the Supplementary Material (Figure S2). A reference to this figure is added in Section 4.5.1.

SI/Fig S6: Same comments about zero lines as for Fig 8 above.

Done.

Typographical suggestions:

9/206: "interacts"

12/284: "snapshots"

21/432: "This is also _true_ earlier in the simulation ..."

22/457: "The _plans for_ this implementation ..." Is a simpler phrasing.

All suggestions have been accounted for.