Review of de Vries et al ACP

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This article investigates the processes controlling the isotopic composition of upper-tropospheric water vapor and ice using an isotope-enabled simulation with a convection-permitting regional model. It identifies 5 main processes, as summarized in their last figure. Main strengths of this article are:

1. The convection-permitting simulation, combined with process-oriented diagnostics, allow for a very detailed process study.
2. This is the first study investigating the isotopic composition in the upper troposphere with such detail in a realistic setting.

This article is thus a significant contribution to the field. In addition, the article is well-written and well illustrated. For these reasons, I recommend publication of this study after some revisions.

1 Major comments

1.1 Improve the evaluation section

1.1.1 Evaluation of the isotopic distribution

The isotopic composition is evaluated using precipitation composition from the GNIP network. The GNIP stations are very sparse in this region. In addition, this allows to evaluate the composition only when it rains. The vapor composition would allow a more continuous evaluation is both space and time. Finally, the precipitation composition is strongly affected by post-condensation processes during rain fall. This does not tell anything about the realism of the simulation in the upper troposphere, which is the subject of this study. The vapor is actually what is most relevant to evaluate for this study, since this is what is ultimately transported to the upper-troposphere.

Therefore, I suggest to evaluate the isotopic simulation with respect to the water vapor composition. It is now available with good spatio-temporal coverage from satellite observations. For example, the IASI data is now available as handy 1x1° twice-daily maps, including for the period of interest ([Diekmann et al., 2021]).

This evaluation could come in addition or in replacement of the GNIP evaluation (which could go in appendix or SI).

In Fig 4a: plotting the water vapor $\delta^2 H$ would allow to directly compare with observations on the same plot. l 304: "not available at these time scales": the vapor composition is.

1.1.2 Evaluation of the precipitation distribution

High-resolution precipitation products are now available to rigorously evaluate precipitation simulation, for example IMERG ([Huffman et al., 2015]) or TRMM/GPM ([Huffman et al., 2007]).

The comparison of the simulated precipitation to the few GNIP stations in Fig 3j is very frustrating. Products are available that would give a much better view of the precipitation distribution and allow for a more rigorous evaluation.

Figs 4b and 4c: the simulation could also be directly compared to observations.

1.1.3 Evaluation in the upper troposphere

The study focuses on convective and microphysical processes in the upper troposphere. I'm aware that no isotopic product is available at this altitude with a good spatio-temporal coverage. But it would be useful for the reader to have at least some assessment of the realism of the simulation for convective and microphysical processes. For example, what is the realism of the water budget in Fig 15? Satellite datasets are available to perform some basic evaluation of the distribution of the relative humidity with respect to ice and of ice water content in the upper troposphere, at least from a climatological point of view (e.g. . For example: MLS ([Livesey et al., 2006]), AIRS ([Fetzer et al., 2003, Read et al., 2007]), Cloudsat ([Austin et al., 2009]).

1.2 Relevance of the water budget and isotopic processes for the transport of water vapor through the TTL

Section 5.3: the added value of this section, and the connection with previous sections, are not clear to me.

What do we know about the realism of this simulated budget? How does it quantitatively compare with previous studies? To what extent is this budget subject to debate? Are there any direct observations for it?

What do we learn from Fig 15? If this Fig is enough to describe the water budget of the TTL, then why should we bother during all previous sections to investigate water isotopes? Wouldn't Fig 15 suffice by itself to solve all questions?

If observations for this budget are uncertain, then could water isotopic observations help to constrain it? If so, the connection with the previous sections would be clearer.

l 591-593: this is confusing. Only Fig 15a contributes to this debate. This article mainly contributes to a better understanding of the processes controlling the isotopic composition, but how is this understanding useful for quantifying the budget of the TTL?

Finally, my understanding is that the main debate is not about the ice water content in the TTL, but rather about water fluxes through the tropopause (e.g. [Bolot and Fueglistaler, 2021]).

Here are some suggestions:

- remove this sub-section
- use this water budget to support the interpretation of previous figures, especially Fig 13e. In this case, then Fig 15a could come just before or after Fig 13 to help interpret it.
- use this water budget to compare with observations, if any, to evaluate the realism of the simulation in the upper troposphere (see my comment 1.1.3). In this case, move it to the evaluation section.
- extend this sub-section to show the connection with previous sub-sections, e.g. could we reconstitute this budget just based on isotopic variables in the simulation?

2 Minor comments

- around l 12-15: it would be useful to tell the reader here what is the horizontal and vertical resolution of the simulation that is investigated in detail, and tell that it is convection-permitting. All the process analysis is done on this simulation, so describing the other simulations in the abstract is not so important.
- l 16-17: this sentence is confusing, especially "leading to". It looks like the injection of ice is what leads to the depletion of the water vapor. Rather, it is the condensation and deposition that leads to the depletion of water vapor.
- l 21: "statistical evaluation" is mysterious at this stage. Reword with something like "statistical analysis over a 1x1 spatial domain and over one month".
- l 26: "base of convective updrafts": this is confusing because we can imagine the base of convective updrafts at the lifting condensation level, well below the freezing level. Reword as "in the mid-troposphere", or give a more specific altitude range.
- l 54: "North Africa": this usually refers to North of the Sahara. Your domain rather corresponds to Western Africa, or to the Sahel.
- l 189: why switching off the shallow convective scheme as well?
- l 245-255: here the d-excess is evaluated. Yet, the d-excess is never discussed again in the process analysis. The d-excess is strongly influenced by post-condensation processes as rain falls. So the model-data agreement or disagreement for d-excess does not tell anything about the realism of the simulation in the upper-troposphere, which is the subject of this study. I suggest to remove these paragraphs, or move it to appendix or SI.

- l 247: how does condensation temperature evolves along air trajectories in Western Africa?
- l 260: "strongly changes": this looks tiny from Fig 3.
- l 291: these 4 phases are not obvious from Fig 4, especially from Fig 4a (black curve). Maybe the domain for the average is too large: it incorporates both the position of the ITCZ before and after the monsoon onset. So we cannot clearly see the monsoon onset when averaging over such a large domain. Maybe try 10N-20N?
- Fig 3: how are the GNIP stations ordered? By latitude? Longitude?
- l 307: why was this MCS chosen? At first, I thought that this MCS, which happens before the monsoon onset, would not be very representative of the full period. I would have expected that the drier air would lead to more ice sublimation, and thus higher $\Delta\delta^2 H_{ice}$, than for MCS during the monsoon period. But Fig 7 suggests that this is not the case. Why? Any comment on this?
- Fig 5c, and everywhere else: the color scale does not allow to distinguish the white regions due to low ice water content, and the white regions due to near zero disequilibrium. Could you for example put some yellow instead of white in the middle of the color bar?
- Fig 6: the numbers are quite small, I had difficulties to read them when printed.
- l 330, 352: I think it would be useful to refer to the appendix already here, or even before. As a reader, I found it very helpful to read the appendix before reading these sections. The appendix is advertised in l 364, but I think that if readers have been able to follow the text up to this line, they don't need the appendix any more. Many readers will need it before.
- l 322-364: All along the discussion of these processes, I was wondering to what extent they are consistent with previous studies. Which processes are new? Which ones are already well established? More references to previous studies would be useful here, to help the readers assess the contribution of this study compared to previous ones.
- l 365-374: This paragraph is crucial to show the representativeness of your case study for the whole period.
- l 376: why only July?
- section 4.3: do you still use the same spatio-temporal domain for this analysis?
- l 447-448: recall these process: condensation/deposition in updrafts and sublimation in downdrafts?
- Fig 8: Add some horizontal lines on fig a and b for 125, 200 and 450 hPa?
- Fig 9: why can't we see the positive vertical velocity anomaly for (1) on Fig 9b? Is it because $\delta^2 H_{ice}$ is also very high in convective downdrafts?
- Fig 10f: is there anything to tell about the super-saturation in strong updrafts?
- Fig 10f: strong updrafts above 200hPa are as dry as strong downdrafts. Why? And why isn't there any sublimation that would enrich the vapor, as in strong downdrafts?
- Fig 12: "black hatching": I cannot see it when printed. Try another color? Or wider rectangles?
- l 493: "mixed" is confusing, it recalls the mixed-phase clouds. Rather call it "outflow"?
- Fig 15: add horizontal lines to recall where the TTL is.
- l 585: add "idealized" in front of "large-eddy simulation". The limitation of previous modeling studies was their idealized configuration, not their resolution.
- l 696: why "and sublimation"? My understanding is that this section only discussed sedimentation, not sublimation?

References

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