Review for

The influence of snow sublimation on stable isotopes of water vapor in the atmospheric boundary layer of Central Europe

General comments:

The manuscript by Christner et al. presents 17 months of new, continuous, low-level atmospheric water vapor δD measurements at a site near Karlsruhe, Germany in conjunction with a Lagrangian isotope model to inform understanding of the along-trajectory controls on measured δD variability. Integration of HYSPLIT-calculated, low-level back-trajectories with the isotope model shows that much of the observed variability in measured δD values is the result of identifiable along-transport processes most generally associated with moistening and dehydration of the air parcel through evapotranspiration and precipitation processes, respectively. A subset of back-trajectories associated with 'cold snaps' result from a systemic shift to continental source regions and easterly trajectories. For this subset, the authors investigate an additional controlling mechanism for isotopic evolution of these easterly trajectories, that of isotopic modification via moistening from surface snow sublimation. In the closing section of the manuscript, the authors investigate a range of possible conditions for isotopic modification of regional snowpacks that best explains measured δD, namely skin temperature controls on fractionating versus non-fractionating sublimation processes. From this, the authors determine the relevant skin temperature window for which post-depositional isotopic modification of snowpacks and associated impacts on low-level atmospheric moisture δD is most relevant.

This is a detailed and comprehensive manuscript that presents a new and robust long-term δD dataset that proves valuable for investigating controls on the isotopic evolution of low-level atmospheric moisture. The methods applied are appropriate. I particularly like the integration of HYSPLIT-derived trajectories into the new Lagrangian model presented and find the observed-model δD congruence (Figs. 6 and 7) impressive and supporting of the Lagrangian-model approach. I find the discussion of the isotope effects of sublimation both nuanced and convincing, which is important given that this impactful process is generally neglected or assumed to be negligible in similarly-focused studies. I expect the findings and research design of this study will be of interest to a broad audience, particularly in light of the expansion of laser-based isotope analyzers that are sure to increase the number of similar isotope records in coming years. Accordingly, I strongly support publication of the manuscript in ACP pending some minor/moderate revisions to the text and some figures in order to (1) reduce redundancies in the text, (2) provide additional clarification for components of the HYSPLIT-Lagrangian isotope model integration and (3) ensure the more complex aspects of the manuscript are understandable to the broader audience likely to be interested in this work (e.g., earth scientists interested in proxy-based investigations of paleoaltimetry and paleoclimate).

Specific comments:

(1) *Reorganization and reducing redundancies* – much of the text in Section 3.3 is redundant with Section 2.3.2. It seems much of 3.3 could be moved and combined with 2.3.2. Similarly, section 3.4 falls under the heading 'Measurements'; however, this moisture source data seems more relevant to the model description in Section 2.3.

The opening paragraph of Section 2.3.3 presents some basic back-trajectory statistics but explanation is limited here. Important clarifying information is not provided until Section 4, specifically Section 4.3. I suggest saving back-trajectory statistics for Section 4 when more details needed for clarification are presented.

Discussion of 24-hour smoothing window on page 4 could be removed there and saved for the same discussion on p. 14 (lines 19-28).

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(2) <u>*Model clarification and limitations*</u> – Given the uncertainty in vapor δD at high altitudes (> 2 km) and low proportion of trajectories (2%) encountering these altitudes, it seems these trajectories could simply be removed from the analysis.

Beyond the vapor measurements at the Karlsruhe study site, are there other published datasets that would ground truth the isotope model presented? Specifically, are there any regional records of soil water δD ($\delta^{18}O$) that can be presented to constrain the RCWIP values used? Additionally, are there any regional snowpack δD ($\delta^{18}O$) records that would give a better understanding of the degree of snowpack δD variability? It is likely that snowpack δD varies both spatially and temporally throughout the accumulation and melt season in the study region, thus some discussion on how this variability limits the model presented is important.

Another limitation that might be more explicitly discussed or tied into the previous point about spatial δD variability of the snowpack is that of the 1°x1° resolution of the GDAS data set. How might this spatial smoothing impact ability to model δD variation?

(3) <u>*Clarifying complexity*</u> –I find Section 5.2 and corresponding Figure 10 difficult to comprehend. I understand the general idea that multiple model runs were used to identify the cutoff temperature between fractionation and non-fractionating sublimation, but is not clear to me how the 16 scenarios of 'side constraint' variability and associated 128 total model runs correspond to the lines shown in Fig. 10. How do 128 total model runs translate to 9 distinct lines in Fig. 10? Please clarify in the text and figure caption and reconsider what Figure 10 should show to more clearly communicate the information in this section.

Given the focus on easterly trajectories, a figure more clearly showing association of easterly trajectory pathways with corresponding snow cover in that region would be helpful. This might be accomplished by adding a panel to Figure 8 showing snow cover.

(4) The title, introduction, and conclusions sections place focus exclusively on the sublimation aspects of this work. I think this undersells and undervalues the importance of the broader Lagrangian isotope model approach and its applications. Sublimation appears to only relate to the easterly trajectory subset (< 50% of trajectories investigated). I am not sure if there is a companion paper planned/submitted/published for the (north)westerly trajectories but these trajectories seem important to discuss in more detail as well, even with a single summary paragraph somewhere in Section 4 of what was learned from modeling these trajectories. If manuscript focus is to be exclusively on sublimation effects, as the title implies, I don't think all trajectory data should be included (i.e., Fig. 5) and the δD record should focus on 'cold snap' trajectories. Currently, there seems to be too much data shown in Figures 5-7 that receive too cursory of a discussion.

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Technical comments

p. 1, Line 16: 'isotopologues' of water more appropriate since molecules are listed

p. 2, Lin4 4: somewhat misleading to say snowpacks are most sensitive to isotopic modification. Lakes, for example, generally provide longer-duration integration of post-depositional modification.

p. 2, Lines 7-8: Here and throughout I think 'fractioning' should be replaced with 'fractionating'. I am unfamiliar with the term 'fractioning' with regards to isotopes.

p. 2, Line 8: clarify what is meant by 'other part'

p. 2, Line 13: It might be helpful to introduce the delta notation earlier in the manuscript so that it can be used here, i.e., 'increase of δ values'

p. 2, Line 21: What is meant by 'resublimation'?

p. 4, Lines 12-13: Quantify what is meant by 'fast' – sub-diurnal?

p. 4, Line 15: What is meant by 'displacement' of moisture uptake amount?

p. 5, Line 3: I think the phrasing 'more likely' should be changed – 'preferential fractionation of D into the liquid phase' is more precise language.

p. 5, Line 25: Clarify what is meant by 'not affected by dilution'

p. 6, Line 15: Where does the 2.6 day value come from?

p. 11, Line 1: Here and elsewhere, should use 'value' instead of 'ratio' to describe δD

p. 11, Line 4: I think the statement 'caused by the relation between δD and condensation temperature' is misleading. The more direct control on the continental effect is degree of rainout.

p. 12, Lines 18-19: Redundant sentences: 'Second….' And 'To this end….'

p. 12, Lines 20-21: Rationale for only considering trajectories above 28% median level needs to be more clearly communicated.

p. 13, Lines 4-5: The term 'side constraints' here and following is unclear to me. Can you clarify what is meant by 'side'?

p. 15, Line 29: Clarify what 'both groups' refer to.