Review of

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

by E. Christner et al.

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# 1 General Comments

In this paper continuous measurements of $\delta$D in near-surface atmospheric vapour from a station near Karlsruhe in Germany are presented and used together with a Lagrangian isotope model to investigate the moisture source conditions during cold spells. The authors find that below a critical skin temperature of $-7.1^{o}$C the moisture sources are dominated by non-fractionating sublimation, whereas above this critical skin temperature and up to $0^{o}$C fractionating evaporation of meltwater is dominating. The separation into the two fractionation regimes is done by comparing the modelled $\delta$D with the measurements in Karlsruhe. These results have important implications for the isotope modelling of atmospheric moisture sources in snow covered regions, which are generally treated in a very simplistic way in isotope-enabled numerical models (only non-fractionating sublimation, see e.g. Yoshimura et al. (2006); Werner et al. (2016)).

I recommend publication of this original, very well-written paper and I suggest only a few minor revisions that are listed below.

# 2 Specific comments

1. p. 1, Title: In my opinion it would be helpful to refine the title. First, I think it might be interesting to state somewhere in the paper (maybe in Section 4) the percentage of the total moisture measured over the 17 months in Karlsruhe that originates from snow covered regions. This would quantify the importance of snow covered moisture sources for Central Europe. Second, I wondered whether adding "meltwater evaporation" to have "The influence of snow sublimation and meltwater evaporation..." would be helpful for a potential reader. Third, I think it would be good to say "... on $\delta$D of water vapour...".

2. p. 1, L. 3: Is "evaporation of snow" really what the authors mean? Isn't it sublimation of snow and evaporation of meltwater to be precise? So could one say maybe "isotope fractionation at snow covered moisture sources" or something similar to stay general and not imply only one of the two fractionation regimes that are discussed?

3. Abstract in general: 1) It might be helpful for the reader to see the suggested quantified importance of snow covered moisture sources (percentage of snow covered moisture sources in the measurement period) as suggested in the first comment also in the abstract. The 17 months of data when reading about snow sublimation and meltwater evaporation seems a bit surprising. 2) I would suggest one last sentence to conclude the abstract with an outlook on the impact of the major finding presented in this study on modelling and experimental stable water isotope research. Ideally saying that the existence of two fractionation regimes has important implications for the more realistic modelling of isotope processes at snow covered moisture sources and that more detailed experimental studies at snow covered sites are needed to better describe the potential coexistence of the two regimes.

4. The introduction was a pleasure to read!

5. p. 3, L. 15: A short discussion on the choice of the length of the back-trajectories and on the potential reduction of uncertainty by using e.g. 10 days back trajectories should be provided. Läderach and Sodemann (2016) use the same moisture source diagnostics as the authors of this paper and find about 4–5 days for the global mean residence time.

6. p. 3, L. 22: Additional vertical displacement of the trajectories would allow an assessment of the uncertainty related to the arrival altitude of the investigated air parcels.

7. p. 4, L. 30: Does "$q$-weighted" mean weighted with $q$@arrival? If so it would be helpful to say it explicitely.

8. p. 5, L. 10-15: In the considered cold snap conditions, I am not totally convinced that supersaturation in ice clouds would be so low. Is there a way to assess this source of uncertainty for the subsequent analysis of snow covered moisture sources?

9. p. 5, L. 13: I would recommend to say "examined transport conditions" or "examined moisture transport regions" or similar.

10. p. 5, L. 22-25: I am not sure if I understand this paragraph correctly but it points to an important assumption and source of uncertainty of the moisture source diagnostics. Could the authors rephrase? Maybe just removing "In this case" and adding at the end of the last sentence ", only by the freshly evaporated moisture"?

11. p. 5, L. 30: Could the authors quantify the influence of non-equilibrium fractionation on $\delta$D? It should be around 1-3‰ depeding on the environmental conditions.

12. p. 7, L. 1: No seasonal changes in FT?

13. In general: I would suggest not using italics for $^{18}$O and D.

14. p. 8, L. 8: "ignored"? What does that mean? That it does not impact total uncertainty?

15. p. 9, L. 8: "of the whole calibrations", maybe clearer if one said "found using all calibrations".

16. p. 9, L. 13: What does "WSW" mean?

17. p. 10, L. 12: It would be nice to have the period that is covered by the measurements right at the beginning of Section 4.

18. p. 11, L. 4: Fig 3c in JJA I do not see the continental effect so clearly, is it just the colorbar range or is it that in summer continental recycling smears out the strong continental gradient visible in winter. So should only a reference to Fig. 3b be given here?

19. p. 12, L. 7: Shouldn't it say "Sublimation of snow or snow-melt evaporation?"?

20. p. 12, L. 19: "To this end, we identified moisture uptake at $T_{skin} < 0$°C". This is a repetition the sentence before is enough in my opinion.

21. p. 14, L. 8: I assume the air masses originating from higher altitudes are also very dry so that the subsequent history of the air mass plays an important role and particularly subsequent uptakes so that I would not be so worried about the uncertainty associated with these air parcels' initialisation.

22. p. 15, L. 18: In table 1 only $\Delta\delta$D are stated and I would find it important to also mention the root mean square or absolute difference. Figure 9 of course also helps with respect to this point.

23. p. 16, L. 8: $°C$ should be C

24. p. 24, Figure 1: I find this Figure very helpful. I just did not understand whether the dashed blue and green lines indicate exact locations of precipitation/uptakes? If yes then why is the green line not continuous between -2.5 and -2 days? If no, then the authors should explain how the lines should be interpreted in the caption.

25. p. 27, Figure 5: state the period associated with these climatological source regions.

26. p. 29, Figure 8: Why is the total identified moisture along the trajectories only 48%? It seems low to me. Is it due to the trajectory length?

# References

Läderach, A., and H. Sodemann: A revised picture of the atmospheric moisture residence time, *Geophys. Res. Lett.*, 43, 924–933, doi:10.1002/2015GL067449, 2016.

Werner, M., Haese, B., Xu, X., Zhang, X., Butzin, M., and Lohmann, G.: Glacial–interglacial changes in H218O, HDO and deuterium excess – results from the fully coupled ECHAM5/MPI-OM Earth system model, *Geosci. Model Dev.*, 9, 647–670, doi:10.5194/gmd-9-647-2016, 2016.

Yoshimura, K., Miyazaki, S., Kanae, S., and Oki, T.: Iso-MATSIRO, a land surface model that incorporates stable water isotopes, *Global and Planetary Change*, 51, 90-107, doi:10.1016/j.gloplacha.2005.12.007, 2006.