Review of "Arctic sea ice loss amplifies local evaporation influence on water vapor isotopes: Insights from cruise observations" by Zhang et al., submitted to EGUsphere

The study by Zhang et al 2025 reports ship-based measurements of water stable vapour isotopes from an extensive cruise on the Chinese research vessel Xuelong 2, spanning across the arctic seas, from northern Norway to the Bering Strait. Combined with meteorological data, the authors use a Bayesian mixing model and Lagrangian trajectory analysis to quantify contributions of local (from the Arctic ice-free region) and remote (from lower latitudes) moisture sources in this area. The main conclusion of this manuscript is that sea ice change is a key modulator or Arctic water vapour isotopic variations. The presented dataset is a valuable and forms an important set of observations in an in general under sampled region. However, there are some aspects of this paper concerning the methods, presentation, and interpretation of results that should be addressed before publication in ACP.

Major comments

1.   The use of the MixSIAR model to determine the contributions of local versus remote vapour appears questionable for the problem at hand. There are other suitable tools available for this application that can calculate the moisture budget directly rather than using a statistical approach. This is particularly important since the authors claim to provide a 'physical consistent basis for the interpretation of Arctic vapor isotopes' (line 338). In addition, in this manuscript it is not clear how precisely MixSIAR is used. An important concern is that this model might not be the correct tool to use when there is effectively only one tracer available, as $\delta^{18}O$ and δD are highly correlated. Furthermore, the sensitivity of the results on the isotopic composition of the assumed North Atlantic end member should be discussed.

Thank you for your professional and constructive suggestions regarding the use of the MixSIAR model. We have carefully addressed the concerns as follows:

First, regarding the use of MixSIAR and the 'physically consistent basis' claim:

The MixSIAR model is used here to quantify source contributions from isotopic tracers, rather than to resolve fractionation mechanisms. We agree that a direct moisture budget is ideal for a fully physical interpretation. However, in the absence of high-resolution observational data on Arctic atmospheric moisture fluxes, an isotope-based mixing model represents a feasible alternative. We acknowledge that the statement in our original submission claiming a 'physically consistent basis' was not appropriate. We have revised the text to clarify the purpose of MixSIAR and the interpretation of its outputs (line 414), and all modifications are highlighted in the revised manuscript.

Second, regarding clarity of MixSIAR usage and tracer collinearity:

We fully understand the concern about the high correlation between $\delta^{18}O$ and δD. In this study,

only two moisture sources—local evaporation and low-latitude advection—are considered. In principle, their relative contributions could be estimated using a single tracer within a traditional mass-balance framework. Although $\delta^{18}O$ and δD are strongly correlated, their joint use allows a better constraint of the isotopic space and implicitly reflects differences in kinetic fractionation processes between the two source categories. The combined use of $\delta^{18}O$ and δD as tracers to quantify water source contributions has been adopted in several previous studies (Lao et al., 2022, Liu et al., 2025, Zhu et al., 2025). We nevertheless acknowledge the limitations associated with tracer collinearity and have now provided an explicit discussion of this issue in the revised manuscript (see Lines 427-430 for details).

Third, regarding sensitivity to the North Atlantic end-member:

The North Atlantic end-member is defined using summer vapor isotope data from cruise observations (Namyatov et al., 2023, 2024), which show substantial variability. This variability is one of the primary motivations for adopting the MixSIAR framework, as it allows process uncertainty to be explicitly incorporated through prior distributions, rather than relying on a traditional deterministic mass-balance model. In the revised version, we acknowledge the simplified representation of the low-latitude end-member and have expanded the Discussion section to explicitly discuss the limitations associated with endmember selection and the implications of these assumptions for our results (see Lines 422-427 for details)

2.   The authors use hourly, single, 5-day air parcel trajectories at 10 m arrival height to determine moisture sources and, in turn, use this as a basis to compute a weighted isotopic source mean. There are several problems with this choice. (i) The atmospheric moisture residence time can be substantially longer and vary with different weather situations at high latitudes (Gimeno et al., 2021). (ii) Trajectory end points, while indicating air parcel origins, do not necessarily reflect the actual moisture source. For this, one would need to take into account the moisture budget along a trajectory as in commonly applied trajectory-based diagnostic (Brunello et al., 2024). If the authors keep their choice of method, such substantial uncertainties need to be considered in the interpretation of the results, and require a more careful phrasing on how reliable the contributions from different regions found here are (see comment #3).

Thank you for the reviewer's advice regarding the HYSPLIT model. We acknowledge that our initial description of the HYSPLIT analysis was not sufficiently clear, and we apologize for any confusion. Our original intention was to identify the pathways and origins of air parcels and to examine their meteorological properties (specific humidity, temperature, and relative humidity), in order to qualitatively assess whether the local evaporation and advective processes discussed in the main text are physically plausible based on air-mass characteristics. Accordingly, HYSPLIT is not used to diagnose quantitative moisture source contributions, and we therefore did not apply the moisture-uptake functionality of the HYSPLIT/PySPLIT scripts.

To account for the longer and variable residence time of atmospheric water vapor in polar regions, we have extended the back-trajectory calculations to 10 days (Gemino et al., 2021, Thurnherr et al., 2020). In addition, we have modified the trajectory-based weighting to include specific humidity along the air-parcel pathways (see Section 2.5) to better characterize the thermodynamic context of the air masses. We emphasize, however, that this modification does not replace trajectory-based moisture budget diagnostics.

We further acknowledge that, compared to trajectory-based moisture source diagnostic methods that explicitly track moisture uptake along air-parcel pathways, our approach involves substantial uncertainties when linking air-mass origins to moisture sources. We have therefore revised the manuscript to clearly state that the HYSPLIT results are used only as qualitative supporting evidence, rather than as a quantitative basis for determining regional moisture contributions. To better illustrate the robustness of the qualitative interpretation, in the revised manuscript we have added supplementary back-trajectory experiments using longer trajectories and multiple arrival heights (Figs. S2–S4), and expanded the Discussion section to explicitly address the uncertainties associated with the HYSPLIT approach and their potential implications for the interpretation of the MixSIAR results (see Lines 423-425).

3.   Broad statements and general conclusions are at several places phrased such that they suggest being a result of this study. Wordings and conclusions throughout this manuscript appear often overstated. For example, lines 16-17: "These findings establish a reliable ...". This overlooks previous work by others in this region, and overstates the usefulness of the results as a benchmark without taking into account the uncertainty of the analysis. Similar criticism applies to the statement in lines 348-349 and at several other locations.   Please rephrase and modify these statements by considering what actually is new and added by this study. It is the reviewer's impression that this mainly is another ship-based dataset from the high Arctic with sea ice and vapour isotope measurements with corresponding interpretation of the respective local and remote influences on the measured signal. More examples for the use of language that appears to overstate findings or appears to be phrased unnecessarily authoritative are pointed out in the detailed comments.

Thank you for these constructive comments. We acknowledge that some statements in the original manuscript were overly broad and may overstate the findings. We have carefully revised these sentences throughout the manuscript to accurately reflect what is new in this study and to acknowledge the limitations of the dataset. In addition, the Discussion section has been refined to explicitly address the study's scope and the uncertainties related to interpreting the local and remote influences on the vapor isotope measurements.

4.   The authors state that there has been a discrepancy in the literature which they resolve through their study, namely that Klein and Welker (2016) report and anti-correlation between sea ice extent and d-excess, whereas Bonne et al (2019) report a correlation between d-excess

and sea ice extent (L. 47). In L. 328 onward, the authors claim to have resolved this discrepancy, but other than the claim it is not evident from the text how their study actually reconciled these opposite findings. There is also a citation of Samuels-Crow et al., 2014 in there, which is a study from the Chilean Altiplano at 5000 m a.s.l.. Clearly, the surface conditions there are not comparable to the Arctic. Also, some of the cited studies discuss precipitation d-excess, whereas others discuss vapour, which cannot be lumped together in this argumentation. Currently, the claim of reconciling conflicting interpretations does not appear to be laid out convincingly.

We thank the reviewer for these constructive comments and apologize for the unclear description in the original manuscript. Based on our results, d-excess does not exhibit an inherent or monotonic correlation with sea ice concentration. Instead, it shows a bimodal distribution (see Fig. S1), suggesting contributions from both local evaporation and Rayleigh distillation during long-range transport. We have expanded the Discussion section to elaborate on this point to better illustrate this pattern (see Lines 379-389).

Regarding the citation of Samuel's work, we acknowledge the differences in surface conditions between their study region and ours. In the revised manuscript, we refer only to their interpretations related to long-range transport and low-specific conditions, which are relevant and comparable in the Arctic context. We also recognize that some cited studies focused on precipitation rather than water vapor, which are not directly comparable. In the revised manuscript, we have clarified this distinction and revised the text to avoid conflating precipitation and vapor signals (see Lines 41-47, 432 for details).

5. Part of the analysis of the d-excess uses correlations with relative humidity (RH) calculated in relation to air temperature rather than the more relevant quantity RH calculated with respect to SST (Uemura et al., 2008; Pfahl and Sodemann, 2014). The figure panels a-c in Fig. 5 should be updated accordingly, and preferably plotted with consistent ranges on both axes to facilitate comparability.

Thank you for this suggestion. In-situ sea surface temperature (SST) measurements were not available during the voyage. Estimating relative humidity with respect to SST would require satellite or reanalysis data, but spatial and temporal mismatches with our ship-based observations introduce substantial uncertainty. We therefore used relative humidity measured directly by the onboard meteorological station. Previous work (Klein and Welker, 2016) suggests that the overall variability between surface air temperature and SST remains a reasonable approximation, so the inter-regional gradients we observe remain meaningful. In the revised manuscript, we have acknowledged this limitation and added a discussion of the associated uncertainties (Lines 398-400).

To improve comparability, we have revised Figures 4 and 5 by combining the related panels into a single multi-panel figure, with axes presented consistently across panels.

6.   More details should be given regarding the acquisition of the measurement data. This includes the exact location of the inlet on the ship, the flow rate in the inlet, the material of the inlet tubing, the heating of the inlet line, the time resolution of the data set, and a depiction or mathematical representation of the spectroscopic correction given in Eq. 3.

Thank you for this helpful suggestion. We have expanded the Data and Methods section (Lines 64-70) to provide a more detailed description of the measurement setup—including the inlet location, flow rate, tubing material and heating, and data time resolution. We have also explicitly presented the mathematical formulation of the spectroscopic correction applied in Eq. (3). The humidity-dependent correction functions for $\delta^{18}O$ and δD are now given in Eqs. (4) and (5), respectively. These additions improve the reproducibility and transparency of the measurement setup.

7.   The dataset of the measurements has not been made accessible to the reviewers. It is therefore not possible to inspect the actual dataset at this point.

Thank you for this comment. We apologized that the full dataset is not yet publicly archived. However, we can provide the data to the reviewers upon request during the review process, and the finalized dataset will be made publicly available upon acceptance.

8.   Several recent literature with other ship-based vapour isotope measurements in high latitudes should be considered to be referenced in this manuscript: Thurnherr et al., 2020; Thurnherr et al., 2021; Brunello et al., 2024; Sodemann et al., 2024 (see references).

Thank you for this suggestion. We have carefully reviewed the cited studies and included the relevant references (Thurnherr et al., 2020; Thurnherr et al., 2021; Brunello et al., 2024; Sodemann et al., 2024) in the revised manuscript. These works are cited in the Introduction and Discussion to contextualize our ship-based vapor isotope observations within recent high-latitude studies.

Minor comments:

1.   The description of the use of MixSIAR is insufficient and currently not reproducible.

Thank you for this important comment. We agree that the original description of the MixSIAR implementation was insufficient for reproducibility. We have therefore expanded the Data and Methods section to provide a more detailed description of the model setup, including priors, error terms, and MCMC configuration (See Lines 142-168 for details).

To further support reproducibility, we will make the full MixSIAR input files and scripts publicly available upon acceptance, and we can provide them to the reviewers during the review process if needed.

2.   Figure 7a and b look exactly the same (same sample points and axis) while they should

reflect delta18O and d-excess. It is very difficult to separate the ship track/arrival points of the trajectories in Figure 8.

Thank you for pointing this out. We apologize for this oversight. In the revised manuscript, Figure 7 has been corrected to accurately reflect $\delta^{18}O$ and d-excess, and Figure 8 has been revised to improve the visibility and distinguishability of the ship track and trajectory arrival points.

3.   Throughout manuscript: Using the word 'melt regions' suggests that the measurements were taken in an area where there was an ongoing melting process. Do you rather mean ice-free and ice-covered regions as there were large ice-free regions where ice was already melted?

We thank the reviewer for this comment. We agree that the term "melt regions" may be misleading. To avoid confusion, we have replaced "melt regions" with "ice-free regions" throughout the manuscript and clarified the definitions accordingly.

Detailed comments:

Line 12: 'ice-phase processes'. You mean 'in cloud ice phase processes'?

Thank you for your comment. We intended to refer to the processes occurring during long-range transport and residence over ice-covered surfaces—including cloud ice-phase processes, sublimation, and related mechanisms. To avoid confusion, we have revised the phrase in Line 12.

Line 21: Change 'These parameters' to 'these isotopes fractionate'

Thank you for your suggestion. We have revised the wording accordingly in the updated manuscript.

Line 31: Add reference to these studies after the words 'ice-free ocean'

Thank you for your suggestion. We've added the references accordingly.

Line 35: Which sources are meant here, local or lower latitude sources?

We apologized for the misleading description here. We are intend to refer to the relative contributions of these two sources. We've adjusted the according phrase.

Line 42: This is not consistent with Thurnherr and Aemisegger (2022) who observed negative d-excess in extra-tropics.

We thank the reviewer for drawing our attention to Thurnherr and Aemisegger (2022). Their study documented negative d-excess values associated with an extratropical cyclone event (24–27 December 2016), where warmer region of the cyclone exhibited suppressed ocean evaporation and dew deposition—processes that temporarily lower d-excess. While our study

focuses on the mean state of lower latitude moisture sources, we acknowledge that synoptic-scale events such as cyclones can produce transient d-excess signals that deviate from this baseline. We have now noted this in lines 46-47.

Line 130: What sources were used as input.

We apologize for the incomplete description. The detailed input setting has been added in the section 2.5 (see Lines 142-168 for details).

Line 130 (paragraph 2.5): But this uses water stable isotopes as passive tracers? Why not use ensemble trajectory analysis HYSPLIT as tracer option rather than statistical mixing method?

Thank you for your comments. The MixSIAR model is used here to quantify source contributions from isotopic tracers, rather than to resolve fractionation mechanisms. We agree that a direct moisture budget is ideal for a fully physical interpretation. However, in the absence of high-resolution observational data on Arctic atmospheric moisture fluxes, an isotope-based mixing model represents a feasible alternative. We have revised the text to clarify the purpose of MixSIAR and the interpretation of its outputs (lines 414-415), and all modifications are highlighted in the revised manuscript.

Line 133: 'Figure 1 shows the', this comes a bit unexpected and is posed as a conclusion while the 'co-variation' has not been introduced to the reader and is also not clear from this figure. Rather reword to something like: Figure 1 suggests/shows a that there may be a co-variation…..

Thank you for your suggestion. We've adjusted the relative phrases (line 171).

Line 179: no more so on relative humidity refer to Pfahl and Sodeman (2014)

We apologize for the issues arising from the use of relative humidity derived from air temperature. In-situ sea surface temperature (SST) measurements were not available during the voyage. Estimating relative humidity with respect to SST would require satellite or reanalysis data, but spatial and temporal mismatches with our ship-based observations introduce substantial uncertainty. We therefore used relative humidity measured directly by the onboard meteorological station. Previous work (Klein and Welker, 2016) suggests that the overall variability between surface air temperature and SST remains a reasonable approximation, so the inter-regional gradients we observe remain meaningful. In the revised manuscript, we have acknowledged this limitation and added a discussion of the associated uncertainties (see Lines 398-400 for details).

Line 204: The word 'trajectory' is confusing in this context, you mean 'curve'?

We apologize for the confusion caused by the original description. Accordingly, we have

replaced "trajectory" with "curve" in the revised manuscript (line 243).

Line 231: you mean d-excesss in surrounding vapour

Thank you for your suggestion. We've adjusted the word accordingly (line 271).

Line 236: supersaturation was only mentioned in relation to cloud processes?

Thank you for your suggestion. Ice cloud process and dew deposition uder supersaturation condition could both cause the lower d-excess value, we've adjusted according descriptions to better explain it (see Lines 272-280 for details).

Line 278: supersaturation is not a process but a condition? You mean crystal formation under supersaturated conditions?

Thank you for your suggestion. We've revised the according words in line 330.

Line 281: It is usually the opposite, air masses from the south are warm and moist in general, please explain.

We thank the reviewer for pointing out this ambiguity. In the original text, we described southerly air masses as "dry" based on their relatively low relative humidity (~80%) observed in the Ice-free region. We acknowledge that this wording was misleading, as these air masses actually carry high specific humidity due to their warm origin. Elevated temperatures in ice-free regions create a large saturation vapor deficit (via the Clausius–Clapeyron relationship), which maintains low relative humidity despite high specific humidity, that's the reason why we decribe the air mass as warm and dry. We have revised the manuscript to clarify this distinction.

Line 301: (single) 10m trajectory arrival does not necessarily represent the moisture source.

Thank you for your suggesstion, we have added a set of tests at different heights to enhance the robustness of the explanation (Figure S2, S3, S4).

Reference

Brunello, C. F., et al. "Moisture transformation in warm air intrusions into the Arctic: Process attribution with stable water isotopes." Geophysical Research Letters 51.21 (2024): e2024GL111013.

Klein, Eric S., and Jeffrey M. Welker. "Influence of sea ice on ocean water vapor isotopes and Greenland ice core records." Geophysical Research Letters 43.24 (2016): 12-475.

Lao, Qibin, et al. "Quantification of the seasonal intrusion of water masses and their impact on nutrients in the Beibu Gulf using dual water isotopes." Journal of Geophysical Research: Oceans 127.7 (2022): e2021JC018065.

Liu, Sihai, et al. "A dual water isotope dataset for quantifying summer water mass transport in the northern South China Sea." Scientific Data 12.1 (2025): 1123.

Thurnherr, Iris, and Franziska Aemisegger. "Disentangling the impact of air–sea interaction and boundary layer cloud formation on stable water isotope signals in the warm sector of a Southern Ocean cyclone." Atmospheric Chemistry and Physics 22.15 (2022): 10353-10373

Thurnherr, Iris, et al. "Meridional and vertical variations of the water vapour isotopic composition in the marine boundary layer over the Atlantic and Southern Ocean." Atmospheric Chemistry and Physics 20.9 (2020): 5811-5835.

Thurnherr, Iris, et al. "The role of air–sea fluxes for the water vapour isotope signals in the cold and warm sectors of extratropical cyclones over the Southern Ocean." Weather and Climate Dynamics 2.2 (2021): 331-357.

Zhu, Shilong, et al. "Quantitative analysis of recharge sources in the karst underground river basin of Maocun, Guilin, based on hydrogen-oxygen isotopes and the MixSIAR model." Environmental Earth Sciences 84.21 (2025): 597.

References

Brunello, C.F., Gebhardt, F., Rinke, A., Dütsch, M., Bucci, S., Meyer, H., et al. (2024). Moisture transformation in warm air intrusions into the Arctic: Process attribution with stable water isotopes. Geophysical Research Letters, 51, e2024GL111013. https://doi.org/10.1029/2024GL111013

 Thurnherr, I., & Aemisegger, F. (2022). Disentangling the impact of air–sea interaction and boundary layer cloud formation on stable water isotope signals in the warm sector of a Southern Ocean cyclone. Atmospheric Chemistry and Physics, 22(15), 10353-10373.

Sodemann, H., Weng, Y., Touzeau, A., Jeansson, E., Thurnherr, I., Barrell, C., et al. (2024). The cumulative effect of wintertime weather systems on the ocean mixed-layer stable isotope composition in the Iceland and Greenland Seas. Journal of Geophysical Research: Atmospheres, 129, e2024JD041138. https://doi.org/10.1029/2024JD041138.

Thurnherr, I., Kozachek, A., Graf, P., Weng, Y., Bolshiyanov, D., Landwehr, S., Pfahl, S., Schmale, J., Sodemann, H., Steen-Larsen, H. C., Toffoli, A., Wernli, H., and Aemisegger, F., 2020: Meridional and vertical variations of the water vapour isotopic composition in the marine boundary layer over the Atlantic and Southern Ocean, Atmos. Chem. Phys., 20 (9), 5811–5835.

Thurnherr, I., Hartmuth, K., Jansing, L., Gehring, J., Boettcher, M., Gorodetskaya, I., Werner, M., Wernli, H., and Aemisegger, F.: The role of air–sea fluxes for the water vapour isotope

signals in the cold and warm sectors of extratropical cyclones over the Southern Ocean, Weather Clim. Dynam., 2, 331–357, https://doi.org/10.5194/wcd-2-331-2021, 2021