Response to Reviewers for the manuscript

"Arctic Sea Ice Loss Amplifies Local Evaporation Influence on Water Vapor Isotopes: Insights from Arctic Cruise Observations" (EGUSPHERE-2025-5306)

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Dear Editor,

We sincerely thank the editor and the reviewers for their valuable time and constructive comments on our manuscript. We received four detailed and insightful reviews, and all comments have been carefully considered during the revision process. In particular, following the reviewers' suggestions regarding the presentation of the moisture source attribution results, we made extensive revisions to improve the clarity, robustness, and transparency of the manuscript.

The major revisions include: (1) conducting a systematic sensitivity analysis of endmember selection and model configurations to further evaluate the robustness of the MixSIAR results; (2) providing more detailed descriptions of the experimental setup, analytical procedures, and uncertainty assessment; and (3) substantially reorganizing and revising the Results and Discussion sections to improve the overall clarity and readability of the manuscript.

Although these revisions do not alter the main findings or conclusions of the study, we believe they have significantly strengthened the manuscript. Below, we provide a detailed point-by-point response to each reviewer comment.

Response to Reviewer2

The authors have done a great job of revising the manuscript and have satisfactorily addressed the previous reviewers' comments. The overall quality of the paper has significantly improved. I think a minor revision is fine for me. I have a few remaining technical queries and suggestions:

1. I still have a slight lingering concern regarding the accuracy of the Bayesian isotope mixing model MixSIAR. The support provided by the results in Figure 6 seems somewhat limited. As an additional enhancement, it would be highly beneficial if the authors could further validate the model's accuracy or provide a brief comparison with other methods/datasets to strengthen this section.

Thank you for this constructive suggestion. To address this concern, we performed a series of sensitivity experiments and included the results in the Supplementary Material. By perturbing

the isotopic compositions of both source endmembers by ±20% and repeating the MixSIAR analysis 1,000 times, we assessed the sensitivity of the estimated source contributions and their associated uncertainties to endmember variability (Fig. S5). In addition, we evaluated the sensitivity of the model results to prior assumptions by testing alternative prior distributions within the MixSIAR framework (Figs. S6). These results indicate that the uncertainty associated with endmember selection and model configuration has a limited influence on the inferred source contributions, and does not affect the overall conclusions of this study.

2. Figure 4: There is a sudden and noticeable change in the trend of the red data points around 10 degrees. Please provide a clear physical or methodological explanation.

Thank you for this insightful question. We interpret the observed change in the isotope–temperature relationship around ~10°C as a transition in the dominant moisture control mechanism.

At lower-temperature conditions (typically associated with sea-ice influenced environments), water vapor isotopic composition is primarily controlled by large-scale moisture transport from lower latitudes. As temperature increases to ~10°C, enhanced saturation vapor pressure favors conditions that enhance local ocean evaporation and increase the contribution of locally recycled moisture, leading to a change in the slope of the isotope–temperature relationship. This interpretation is consistent with the MJ79 model simulations, which reproduce the observed isotopic variability at higher temperatures (Fig. 7), and is further supported by independent results from the MixSIAR analysis (Fig. 9), which show an increased fraction of locally derived vapor under higher-temperature conditions, primarily corresponding to the ice-free regime, compared with the sea-ice and transition regimes.

This has been clarified in the revised manuscript. Please see Lines 355-359 for details.

3. Figure 5: The relationship depicted in Figure 5 does not visually appear to be strictly linear. I suggest the authors re-evaluate the fitting method. Perhaps a non-linear regression would be more appropriate.

Thank you for this insightful comment. As the reviewer pointed out, the observed relationships between d-excess and relative humidity do exhibit slight nonlinear characteristics across the three regions. We therefore performed additional quadratic regression analyses to further evaluate the fitting relationship. The quadratic fit slightly modifies the correlation statistics but does not substantially alter the goodness of fit compared with the linear regressions (Figure R1).

Given that the approximately linear relationship between d-excess and relative humidity has been widely used in previous observational studies (Bonne et al., 2019; Brunello et al., 2023), we retain the linear regressions in the main manuscript to facilitate comparison with earlier work.

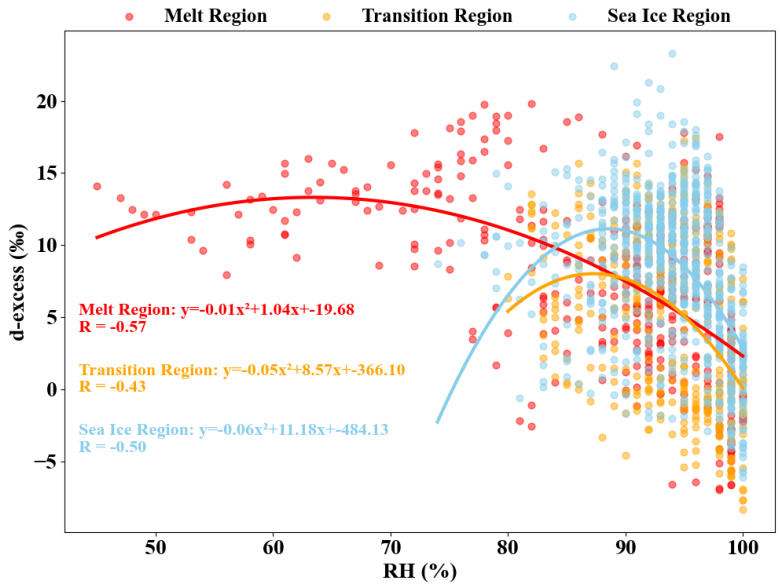

Figure R1. Correlation analysis of d-excess versus relative humidity using second-order polynomial regression.

4. Figure 6: The chosen values for the initial specific humidity and the corresponding isotope delta in Figure 6 appear somewhat arbitrary. To better reflect real-world environmental variability, I recommend plotting a variation range or confidence band (e.g., shaded areas) based on their actual fluctuation range.

Thank you for this valuable suggestion.

For Figure 6a, the initial specific humidity is physically constrained by the temperature-dependent humidity conditions (5–20 °C), which already represent a realistic range of environmental variability. Following the reviewer's recommendation, we additionally incorporated the observed variability in the initial isotopic composition by applying ±1 standard deviation around the mean isotope values and added the corresponding shaded uncertainty ranges in the revised Figure 6a.

For Figure 6b, however, we did not introduce shaded uncertainty ranges for d-excess. Unlike isotope δ values, the variability of d-excess is not primarily controlled by the choice of initial

endmember values, but rather by non-equilibrium fractionation processes, including ice crystal formation (Samuels-Crow et al., 2014), dew deposition under supersaturated conditions (Thurnherr and Aemisegger, 2022), and sublimation from snow and sea-ice surfaces (Bonne et al., 2019). These processes can produce substantial deviations from ideal Rayleigh behavior even under similar initial conditions. Therefore, uncertainty bands generated solely from varying initial values would not adequately represent the physical controls on d-excess variability and could potentially be misleading.

Accordingly, the purpose of Figure 6b is to illustrate how non-Rayleigh processes influence d-excess evolution under supersaturated conditions, rather than to quantify uncertainty associated with initial conditions. We therefore retain the original presentation of Figure 6b in the revised manuscript.

The revised Figure 6 now better reflects realistic environmental variability, and we thank the reviewer again for this helpful comment.

Response to Reviewer3

This study systematically investigates the regulatory mechanisms of sea ice changes on the isotopic composition of Arctic water vapor based on shipboard high-resolution water vapor isotope observations during the Arctic summer, and quantitatively evaluates the relative contributions of local evaporation and advected lower-latitude moisture transport. The findings provide critical observational constraints for understanding the Arctic hydrological cycle and interpreting paleoclimate records.

This manuscript is a second revised version. The authors have adequately addressed most of the comments raised by the two reviewers, and the quality of the paper has been notably improved. Both reviewers highlighted the issue of water vapor isotope data calibration, which is a fundamental prerequisite for this study. In the current revision, however, the detailed calibration procedure remains unclear. Specifically, how was the humidity-dependent correction function established? How frequently was this correction performed? Furthermore, the meanings of Equations (3) through (5) are still ambiguous.

Thank you for your valuable comments. We apologize for the insufficient description of the calibration procedure in the previous version and have revised this section to improve its clarity. During the campaign, standard water vapor measurements were conducted every 20 minutes using calibrated isotope standards under different humidity conditions. All standard measurements collected throughout the campaign were subsequently used to establish empirical

relationships between measured isotope values and water vapor concentration, which are expressed in Equations (3)–(5). These equations quantify the humidity-dependent instrumental bias for $\delta^{18}O$, δD, respectively, and provide the basis for the humidity correction.

Rather than deriving separate correction functions for individual calibration periods, a single correction function was established using the complete set of standard measurements and then applied to the entire dataset. All isotope measurements were normalized to a reference humidity level of 20,000 ppm to minimize humidity-dependent instrumental effects and ensure consistency among observations obtained under different water vapor concentrations. The physical meaning and application of Equations (3)–(5) have also been clarified in the revised manuscript (Lines 97–99).

Response to Reviewer4

Second review of "Arctic Sea Ice Loss Amplifies Local Evaporation Influence on Water Vapor Isotopes: Insights from Cruise Observations" by Zhang et al, submitted to ACP

This is my second review of this manuscript. In the revision, the authors have improved the manuscript in several points, in particular regarding clarifying some aspects of the methods. Otherwise few more substantial changes have been made. Clarifying the methods has revealed more clearly some of the deficiencies of the approach. I recommend more substantial revisions of the presentation and interpretation of the results.

Major comments

1. Uncertainty of moisture source appointment: In the abstract and conclusions, numbers from the source appointment of 18.6 % and 48.3 % are given without uncertainties. However, the uncertainties in obtaining these numbers are manyfold, and need more quantification. How sensitive is this result to the choice of the high-latitude and mid-latitude end member? How sensitive is the 95 % range to choices of the Bayesian mixing model? A systematic uncertainty analysis is needed, also to justify that the Bayesian model is a sensible choice over a simple linear mixing model.

Thank you for this constructive suggestion. To better quantify the uncertainty in the source contribution estimates from the MixSIAR model, we performed two groups of sensitivity experiments in terms of (1) the selection of source endmembers and (2) the configuration of the Bayesian mixing framework, respectively. The corresponding results have been added to the

Supplementary Material (Figs. S5 and S6).

First, to evaluate the sensitivity to endmember selection, the isotopic compositions of both local and remote source endmembers were perturbed by ±20%, based on the observed variability of the endmember isotope compositions during the campaign. A total of 1,000 randomized endmember sets were then generated and applied within the MixSIAR framework (Fig. S5). The results indicate that the inferred source contributions remain within the uncertainty range of the original estimates and do not substantially alter the overall conclusions.

Second, to evaluate the sensitivity to the Bayesian mixing model configuration, we tested alternative prior distributions within the MixSIAR framework (Figs. S6). Specifically, we compared a uniform prior ($\alpha = [1,1]$), a weakly informative prior ($\alpha = [2,2]$), and two biased priors favoring one endmember over the other ($\alpha = [3,1]$ and $\alpha = [1,3]$). The resulting posterior source contributions and their 95% credible intervals showed substantial overlap, suggesting that the inferred source contributions are relatively insensitive to the choice of prior assumptions.

The results indicate that uncertainties associated with both endmember selection and Bayesian model configuration have limited influence on the inferred source contributions, and the posterior estimates remain stable across all sensitivity tests. In contrast, a simple linear mixing model cannot explicitly propagate uncertainties in endmember definitions and observational errors, nor provide posterior probability distributions for the inferred source contributions.

We thank the reviewer again for this insightful suggestion and have revised the wording accordingly to better represent the uncertainties associated with the source apportionment results (Lines 15–16，183-188，386-387).

2. The authors have used the global closure equation of Merlivat and Jouzel (1979) in calculating the surface vapour, which is only valid if precipitation balances evaporation (Jouzel and Koster, 1996). The authors acknowledge that their results are biased due to this choice, but instead the evaporation flux should be calculated using a more complete model which takes mixing with local vapour into account (Craig and Gordon, 1965). This is important because the Bayesian source appointment will be affected by this choice.

We thank the reviewer for this valuable suggestion. We agree that the Craig–Gordon model provides a more comprehensive representation of isotopic evaporation by accounting for

isotopic exchange between the evaporating surface and ambient atmospheric vapor.

However, the objective of our study is to define source endmembers for Bayesian source apportionment rather than to quantify the isotopic composition of the net evaporative flux. The Craig–Gordon framework estimates the isotopic composition of the net evaporation flux, which depends not only on local surface water but also on the isotopic composition of ambient atmospheric vapor (Horita et al., 2008). In the Arctic, ambient vapor is strongly influenced by moisture transport and mixing from multiple remote source regions. Consequently, the resulting isotopic composition incorporates information from the surrounding atmospheric reservoir and does not represent an independent source endmember.

By contrast, the closure formulation of Merlivat and Jouzel (1979; MJ79) assumes that newly evaporated vapor is removed from the system without subsequent mixing with the local atmospheric vapor reservoir, thereby providing an idealized isotopic composition of freshly evaporated vapor derived from the local source water (Dar et al., 2020). We acknowledge that the closure assumption introduces uncertainty due to its requirement of precipitation–evaporation balance. However, for the purpose of defining source endmembers, this limitation is more consistent with the assumptions of the Bayesian mixing framework because it preserves the independence of endmembers and avoids incorporating non-local atmospheric influences into their definition. We therefore adopted the closure-based formulation and clarified this rationale in the revised manuscript, and included a discussion of the uncertainty associated with the use of the MJ79 model (Line 456-459).

3. The precision of the isotope species is given with a number of digits that is not possible to achieve with available analytical methods. δ18O is commonly given with 2 decimals, and δD with 1 decimal in liquid analysis, but the total uncertainty has not been quantified here.

We thank the reviewer for this helpful comment. We agree that the precision and number of significant digits reported for stable isotope measurements should be consistent with the analytical uncertainty.

We acknowledge that a full propagation of total analytical uncertainty was not previously provided. In the revised manuscript, we have now included estimates of long-term analytical uncertainty based on repeated measurements of internal laboratory standards over the study period, expressed as long-term reproducibility derived from normalization residuals ($\delta^{18}O$: ±0.06‰, δD: ±0.3‰, 1σ) (Lines 78-80). This uncertainty reflects the combined effects of instrumental precision, calibration to the international VSMOW scale, and day-to-day

analytical variability under identical measurement conditions.

Following the reviewer's suggestion, all isotope values reported in the manuscript (tables and main text) have been rounded to appropriate significant figures ($\delta^{18}O$: two decimal places; δD: one decimal place) to ensure consistency with the reported analytical uncertainty. This rounding does not affect the main interpretations or conclusions of the study.

4. The δD data are never shown. It would be useful to show a short time segment where both δD and δ18O are seen at high time resolution for a period where strong variations are present to ascertain that the two main isotope co-vary on the same time scales and are not affected differently by inlet memory effects in the unheated inlet.

We thank the reviewer for this helpful suggestion. Following the reviewer's suggestion, we have added an additional high-resolution time-series figure showing both $\delta^{18}O$ and δD throughout the observational period in the Supplementary Material (Fig. S7). The two isotope species exhibit generally consistent temporal variability and co-vary on similar time scales during periods of both relatively stable and rapidly changing atmospheric conditions, with no systematic lag or differential response observed between $\delta^{18}O$ and δD, suggesting that inlet memory effects in the unheated inlet do not differentially affect the two isotope measurements.

5. Some more details of the methodology are missing: what was the flow rate in the inlet, what was the uncertainty of secondary standards, what tubing material was used, what brand/type of filter was used, at what delta value was correction determined?

Thank you for this helpful suggestion. We have revised the Methods section to provide the requested methodological details. These include the inlet flow rate (0.8 L/min), the uncertainty of the secondary standards (±0.15‰ for $\delta^{18}O$ and ±1.0‰ for δD, 1σ), the tubing material (PFA), the brand of filter used (Nanjing Liebo), and the two internal standards ($\delta^{18}O$ = –11.02% and –29.86‰, δD = –78.1‰ and –222.9‰) used for correction. Please see Lines 71-73, 100-101 for details.

6. Some of the findings remain overstated or do not seem to follow as the only possible conclusion (see detailed comments), these should be modified. The significance of the uniqueness and novelty are also overstated. There have been extensive cruises with isotope measurements in the Arctic before, including R/V Polarstern (Bonne et al., 2019), but also during the MOSAiC drift (Brunello et al., 2023, 2024). In these papers, the interpretations put

forward here in terms of sea ice influencing the vapour already exist in the literature. This manuscript confirms previous findings with a new dataset.

We thank the reviewer for this valuable and constructive comment. We agree that some statements in the original manuscript may have overstated the level of novelty and the strength of the conclusions. In the revised manuscript, we have carefully moderated the statements regarding novelty and significance and revised the text to properly acknowledge previous work. Please see the detailed responses to the specific comments below.

7. I find the interpretation of the data in terms of sea ice categories questionable. The data does not separate well according to the 3 categories of ice free, transitional, ice covered. In particular the transitional category looks just like the sea ice category. Instead, it seems that air temperature or SST would be the more stringent separator, that divides the data into two categories (Fig. 4). And since SST is available, RH(SST), which is indeed most influential for the kinetic fractionation during evaporation, can be calculated from the dataset. It would be interesting to see if RH(SST) stratifies the data, such as SST itself does. A critical rethinking and discussion of the categories that are now used for analysing the data is recommended.

We thank the reviewer for this valuable comment. We agree that air temperature, SST, and RH(SST) are important physical controls on Arctic water vapour isotopic variability and may provide complementary perspectives on surface exchange processes.

In this study, we focus on isotope variability under distinct sea-ice surface conditions, for which sea-ice concentration (SIC) provides a physically based descriptor of surface–atmosphere exchange regimes. We acknowledge that SIC categories do not always represent sharply separated regimes, particularly between transitional and ice-covered conditions. We therefore interpret the SIC-based results in a regime-dependent rather than strictly discrete sense, and note that SST and related thermodynamic variables are strongly coupled with SIC and may provide alternative perspectives on the same underlying gradients. We have added a discussion of this limitation in Section 4.1 (Lines 414–420).

8. In the writing, a 'framework' is used that deals with the opposing pairs 'Rayleigh fractionation' and 'kinetic fractionation'. I am not convinced that this dichotomy is really matching to the processes at hand. Rayleigh models are an entire family of models, that describe isotope fractionation with or without removal of condensate during a continuous cooling process. Depending on how the removal is simulated, there could be super-saturation and thus kinetic fractionation in a Rayleigh model. Is not the main difference between

(super)saturation/condensation and sub-saturation/evaporation? Maybe the authors wanted to emphasise that there is predominantly equilibrium fractionation in the long-range transport? In any case it should be justified better that the chosen antipoles are indeed meaningful opposites.

Thank you for this insightful comment. We agree that Rayleigh fractionation and kinetic fractionation are not strictly opposing concepts. As the reviewer noted, Rayleigh distillation represents a broad class of isotope fractionation models describing progressive phase changes during transport, and kinetic effects may also occur within Rayleigh-type processes under supersaturated conditions.

Our original intention was not to contrast Rayleigh fractionation against kinetic fractionation as mutually exclusive mechanisms, but rather to distinguish between isotope evolution dominated by near-equilibrium condensation during large-scale transport and isotope modifications associated with non-equilibrium surface evaporation processes. We recognize that the original terminology may have oversimplified this distinction.

Following the reviewer's suggestions (see detailed responses below), we have revised the manuscript to avoid implying a strict antipole relationship between Rayleigh and kinetic fractionation, and instead emphasize differences in dominant controlling regimes rather than a simple equilibrium versus kinetic dichotomy.

9. The description of the choices of the Bayesian model is now much more detailed, which is good. However, the choices are not justified, or supported by references. The impact of these choices on the source appointment remains unclear, and the workings of the method itself is still opaque. Some sentences explaining the working concept would be useful here.

We thank the reviewer for this constructive comment. We agree that additional explanation of the Bayesian framework and model parameter choices would improve the clarity of the manuscript.

In this study, we used the standard MixSIAR implementation following the framework described by (Stock and Semmens, 2016; Stock et al., 2018). We adopted a Dirichlet prior with $\alpha = 1$ (uninformative prior) as the default setting in MixSIAR when no strong prior knowledge of source contributions is available. This choice avoids imposing strong a priori constraints on source proportions and allows the posterior estimates to be primarily informed by the isotope observations through the likelihood function. To assess the robustness of this configuration, we further tested alternative prior specifications within the MixSIAR framework (Figs. S6). The

resulting source apportionment showed little sensitivity to prior choice, indicating that the posterior estimates are strongly data-constrained.

We also used the recommended Markov Chain Monte Carlo (MCMC) configuration (chain length = 100,000; burn-in = 50,000; thinning = 50; 3 chains) as suggested in the MixSIAR documentation to ensure adequate posterior sampling. Convergence diagnostics using the Gelman–Rubin statistic (< 1.05) confirm satisfactory mixing of the chains.

Conceptually, the Bayesian mixing model combines isotope mass balance constraints with uncertainty in endmember signatures and observational error to infer posterior probability distributions of source contributions. In practice, this is achieved by iteratively sampling source proportion combinations and retaining those that best reproduce the observed isotope composition within uncertainty bounds.

We have added a more detailed conceptual description in the revised manuscript to improve clarity. Please see Lines 145-148, 155–160, 183-188 for details.

10. The "inverse temperature effect" is proposed as a terminology. I think it is not beneficial to add another term to the set of established terms describing precipitation isotopes, also since this study deals with water vapour and not with precipitation. It becomes more confusing to a reader, also since the proposed term is the negation of another "effect".

This is a good point. We agree that the term “inverse temperature effect” may be confusing and potentially misleading in the context of water vapour isotope studies. To avoid ambiguity and improve conceptual clarity, we have removed this term from the revised manuscript.

11. The cruise period needs to be specified in the methods section and in the abstract. This is essential information missing from the manuscript. A short description of the cruise with harbour times should also be included.

Thank you for this helpful suggestion. We have revised both the Abstract and the Methods section to include the cruise period. In addition, a brief description of the cruise route and observation period has been added to the Methods section to provide a clearer overview of the measurement campaign (Line 66-69).

12. Please make the dataset available to the reviewers before manuscript publication.

Thank you for this suggestion. In accordance with the data policy of the Polar Research Institute of China, all datasets must be archived in its official data repository before release. The dataset used in this study has been submitted to the repository (https://www.chinare.org.cn/) and is currently undergoing review and approval. Once the deposition is approved, the dataset will be publicly released with a persistent DOI. We will update the Data Availability Statement with the DOI and access link prior to the final publication of the manuscript.

Detailed comments:

L. 43: I don't think this is an accurate representation of Klein et al (2015). They observed a short spike in water vapour isotopes over land which they interpreted in terms of a cyclone signal. You might rephrase as "In contrast, Klein et al., (2015) proposed that local evaporation..."

We thank the reviewer for this comment. We have revised the wording accordingly (Line 44).

L. 54: "diverse" -> "contrasting" or "different"

We thank the reviewer for this comment. We have revised the wording accordingly (Line 55).

L. 55: "unique": This data set is not unique, given previous Arctic cruises where similar observations were made and are described in publications (Bonne et al., 2019; Brunello et al., 2023, 2024). Maybe rephase as "new".

We thank the reviewer for this comment. We have revised the wording accordingly (Line 56).

L. 74: The time interval for which this measurement precision is valid needs to be specified. How was this determined?

Thank you for this suggestion. The reported measurement precision was taken from the Picarro instrument specifications. We have clarified in the manuscript that the precision is better than 0.25‰ (0.08‰) for $\delta^{18}O$ and 1.6‰ (0.5‰) for δD at 10 s (100s) averaging intervals, respectively, according to the manufacturer's manual. The corresponding information has been added to the manuscript (Line 78).

L. 75: It is recommended to publish the dataset with 1 min resolution. The analyzer should have sampled at even higher frequency of 1 Hz or so. Was the d-excess calculated from the hourly data, or from the 1 min data?

We thank the reviewer for this comment. The original isotope measurements were recorded at 1 min resolution, and d-excess was calculated directly from the 1 min isotope data. In this study, hourly data were used for the subsequent analyses to match the temporal resolution of the reanalysis and to facilitate comparison among different variables.

L. 81: What were the delta values of the standards, was it actual VSMOW?

Thank you for this comment. The standards used in this study were not VSMOW itself, but secondary laboratory standards calibrated against VSMOW. Their isotope compositions were $\delta^{18}O$ = –11.018‰ and δD = –78.051‰ for Standard 1, and $\delta^{18}O$ = –29.860‰ and δD = –222.860‰ for Standard 2. We have added this information, together with the corresponding standard deviations, to the revised manuscript (Lines 100-101).

L. 90: Uncertainties of the calibration standards need to be provided. Check number of digits and justifiable precision.

Thank you for this comment. We have added the standard deviations of the calibration standards to the revised manuscript (Lines 102–103) to document their analytical uncertainties. We have also reviewed the reported numerical precision throughout the manuscript and adjusted the number of significant digits where appropriate to ensure that it is consistent with the measurement uncertainty.

L. 97: Is the weather station data included with the isotope dataset? At which height was the weather station acquiring data?

Thank you for this comment. The meteorological data were collected simultaneously with the isotope measurements and are included in the dataset. The weather station was installed close to the isotope inlet at a height of approximately 10 m above sea level. This has been clarified in the revised manuscript (Lines 108).

L. 107: The ice-free category may include extensive regions with drift ice. This should be considered in the interpretation/discussion of the results.

This is a valid point. We acknowledge that the ice-free category may include regions with drift or fragmented sea ice, particularly under marginal ice conditions, which introduces some ambiguity in the surface classification.

We have added a discussion of this limitation in Section 4.1, and clarify that this potential mixing of surface types may affect the interpretation of the associated isotope signals, but does not alter the main conclusions of the study. Please see Lines 414–420 for details.

L. 118: The assumption of δD = δ18O = 0 permil for arctic ocean sea water may be violated frequently, for example in regions influenced by Siberian river outflow, or where brine rejection/sea ice melt take place. What uncertainty is added by this assumption?

Thank you for this insightful comment. We acknowledge that the assumption of $\delta^{18}O = 0‰$ and $\delta D = 0‰$ for Arctic surface seawater may not be valid everywhere in the Arctic Ocean. This choice was based on published observations indicating that Arctic surface seawater isotope compositions are generally close to 0‰ (Namyatov et al., 2023). Regional deviations may occur due to river runoff, sea-ice melt, or brine rejection, but the isotopic variability of Arctic surface seawater is typically within approximately 1‰.

This level of variability is smaller than or comparable to the perturbations explored in our endmember sensitivity experiments (±20%; Fig. S5). The sensitivity tests show that such deviations have only minor effects on the calculated vapor isotope composition and do not alter the main conclusions of this study.

L. 129: Were all levels used in the subsequent analysis? How much difference is there actually between the different elevations? If the trajectories are calculated using pressure level output from GDAS, there are not enough constraints in the boundary data to justify 10, 20, 30, 40 and 50 m trajectories to contain independent or additional information.

We thank the reviewer for this insightful comment. Yes, trajectories initialized at all five arrival heights (10–50 m) were included in the subsequent analysis for the lower-latitude moisture endmember. While slight spatial deviations were observed among trajectories initialized at different arrival heights, their overall transport patterns, moisture histories, and resulting isotopic signatures remained highly consistent.

We agree that the vertical resolution of the GDAS meteorological fields does not support interpreting these trajectories as independent vertical layers. Therefore, the multiple initialization heights are used as a small ensemble to test the robustness of the inferred transport pathways rather than to represent distinct physical atmospheric layers.

L. 141: What is N?

"N" denotes the normal distribution. We apologize for the confusion and have clarified this point in the revised manuscript (Line 155).

L. 145: What does Rhat stand for in this context?

We apologize for the confusion. "Rhat" refers to the Gelman–Rubin convergence diagnostic, with values lower than 1.05 indicating that the model has converged. To improve clarity and avoid ambiguity, we have removed it in the revised manuscript (Line 160).

L. 159-161: Make consistent with measurement precision

Thank you for your suggestions, we've adjusted the manuscript accordingly (See lines 173-174).

L. 178: "The results suggest...": This strong statement is not justified given Fig. 2. There is some degree of non-overlap, but predominantly the PDFs overlap. Adjust the statement to reflect what is seen in the figure.

Thank you for this helpful comment. We agree that the original wording was too strong given the degree of overlap shown in Fig. 2. The statement has been revised in the manuscript to better reflect this feature (Line 198-199).

L. 185: "This distinct d-excess pattern...": I don't see how this statement can be derived from Fig. 2b. The PDFs overlap strongly, except for a separate peak for the transition region. Adjust the statement to reflect what is seen in the figure.

We apologize for the confusion. We agree that the probability density functions in Fig. 2b show substantial overlap among the three sea-ice regimes, with only a modest separation associated with the transition regime.

Our original intention was to highlight that the spatial pattern of d-excess differs from that of $\delta^{18}O$, which show a clear gradient, rather than claiming the distributions across the three sea-ice regimes are completely distinct. The statement has been revised in the manuscript to avoid overstatement (Line 205).

L. 193: "Highlighting the growing role of...": The statement suggests some linear relation

between SIC and kinetic fractionation, but what the figure shows is that there is a more binary distinction between the part of the cruise around 29. August and the rest of the cruise. It would be useful to show the δD as well for maybe a limited period in the Appendix. The co-variation between δ18O and d-excess is quite strong, which possibly suggests a surprisingly flat δD. Showing the δD would clarify that aspect.

Thank you for this insightful suggestion. We apologize if our original phrasing implied a linear relationship between sea ice concentration (SIC) and kinetic fractionation. By 'growing role' we intended to describe how kinetic fractionation gradually increases with temperature in the ice-free region, rather than a linear response to SIC. We have adjusted the expression in the revised manuscript to avoid this confusion and better reflect the binary-like distinction you noted (Lines 213-214). Furthermore, as suggested, we have added a high-resolution time series of δD in the Supplementary Material (Fig. S7), which helps clarify the co-variation between between $\delta^{18}O$, δD, and d-excess.

L. 210: "Above this threshold, δ18O ...": Rephrase "equilibrium-driven fractionation" as "equilibrium fractionation". It would be useful to clarify that you talk about a water vapour mixture here with different contributions, one of them being local evaporation under sub-saturated conditions.

Thank you for your suggestions, we've adjusted the manuscript accordingly (See lines 230-232).

L. 213: Please provide the p-level or avoid using "significant"

Thank the reviewer for this comment, we have now explicitly added them to the main text whenever 'significant' is mentioned to ensure clarity (Lines 234-235).

L. 216: not clear what exactly is meant by "dominant fractionation regime"

Thank you for pointing this out. To clarify this, we have changed this phrase to "suggesting a shift in the dominant controls on isotopic variability" (Line 236).

L. 220: "Collectively, these patterns indicate...": Again, this indicates a gradual transition, which is not shown by the data. The pattern is quite clearly bi-modal according to Fig. 4. The claimed control by the sea ice extent is not convincing.

Thank you for the suggestion. We have adjusted the wording accordingly to avoid confusion (Lines 240-241).

L. 222: "further modulates": This statement is confusing and probably wrong. RH quantifies the deficit above the surface, but does not control it. Sub-saturation leads to ("controls") the degree of kinetic fractionation during evaporation. RH(SST) more exactly quantifies the sub-saturation during evaporation, and is negatively correlated to d-excess in the evaporation flux. Rephrase for clarity.

Thank you for the suggestion. We have adjusted the wording accordingly (Line 242-243).

L. 264: "Rayleigh equilibrium pattern": Unclear what this means, rephrase. The proposed antipoles "Rayleigh distillation" and "local kinetic fractionation" might be better framed as long-range transport and condensation and local evaporation.
Thank you for the suggestion. We have adjusted the wording accordingly (Line 286).

L. 281: "organized Rayleigh behaviour": unclear what is meant by this

We apologized for the confusing phrases and we've adjusted the wording accordingly (Line 303).

L. 285: Again, some degree of influence is proposed that is not backed up by the mostly bi-modal variation seen in the dataset, stemming from differences in the ice free Barents Sea and elsewhere during the cruise.

Thank you for the suggestion. We have adjusted the wording accordingly (Line 307-308).

L. 291: "directly implicates": This confirms previous findings and the theoretical understanding of isotope fractionation.

Thank you for the suggestion. We have adjusted the wording accordingly (Line 313).

L. 293-298: "distinct processes that shift with the state of sea ice", "sea ice ... shapes the balance": These formulations imply a control of sea ice that is not backed up by the data. No convincing evidence is provided for a gradual influence of sea ice state. Rephrase to be in line with the presented evidence, or clearly identify these statements as speculative and move to the

discussion section.

Thank you for the suggestion. We have adjusted the wording accordingly (Lines 315-320).

L. 300: Unclear what is meant by "anomalous isotopic behaviour", rephrase

Thank you for the suggestion. We have adjusted the wording accordingly (Lines 322-323).

L. 313: The deficiencies of MJ79 are known and the model is not valid for local evaporation calculations.

Thank you for this important comment. We agree that the MJ79 closure formulation has known limitations and is not strictly valid for representing local evaporation processes in a physically explicit sense.

In this study, however, MJ79 is not used to explicitly model local evaporation fluxes. Consistent with our response to Major Comment #2, it is applied only to provide an idealized estimate of the isotopic composition of freshly evaporated vapor associated with a given source water, which is then used to define source endmembers for the Bayesian mixing framework. We have clarified this distinction in the revised Discussion to avoid possible misinterpretation (Lines 456–459).

L. 330: Why sublimation?

Thank you for the question. This extended residency suggests atmospheric conditions conducive to non-equilibrium ice-phase processes under cold and frequently supersaturated conditions, such as crystal formation and dew deposition.

Sublimation of snow or sea ice has been proposed in previous studies as a potential additional source of vapor with elevated d-excess in ice-covered regions (Kopec et al., 2016; Bonne et al., 2019). However, it is not directly diagnosed from the trajectory analysis and should therefore be regarded as a possible contributing process rather than a confirmed mechanism. In the revised manuscript, we have rephrased this sentence to clarify the role of sublimation within the trajectory-based interpretation and to ensure consistency in describing it as a possible contributing process rather than a directly inferred mechanism (Lines 352).

L. 348-354: Make consistent with actual measurement precision.

Thank you for this suggestion. We have revised the manuscript to ensure that all reported values are consistent with the actual measurement precision (See lines 371-377).

L. 363: Without a sensitivity analysis, these numbers can hardly be seen as robust. The error bars overlap for all but the last category. A systematic sensitivity analysis is needed, taking the different assumptions of end member delta values into account.

Thank you for this important suggestion. A sensitivity analysis has been conducted by varying the end-member δ-values (Figs. S5–S6). The results show that the main conclusions and source contribution patterns remain stable across the tested ranges, indicating robustness to reasonable uncertainties in end-member assumptions (Lines 386-387).

L. 371: "resolves a longstanding Arctic paradox": This is clearly an overstatement of the presented results. I think the measurements and interpretation are useful, but not novel to the extent that this statement is justified given other published work in the region.
Thank you for pointing this out. We agree that the original statement was overstated and have rephrased the sentence in a more cautious manner to better reflect the observational constraints provided by our dataset and the associated interpretation (Lines 395–397).

L. 373: What is meant by "boundary-layer thermodynamics"? Maybe "surface fluxes" or "surface evaporation" would fit better here?

We apologize for the confusing wording. We have adjusted the corresponding phrasing to improve clarity (Line 397).

L. 375: Samuels-Crow et al. 2014 did not make measurements in the Arctic, please remove citation. The sentence lumps together precipitation and vapour measurements, which skews the comparison. Peng et al (2019) reports measurements from Antarctica which seems out of context here.

Thank you for your suggestion. We have adjusted the citation here (Lines 399-402).

L. 376: "These apparently conflicting results...ice-controlled humidity": This sentence again implies a continuity/control of sea ice that is not made evident in the data.

Thank you for the suggestion. We have adjusted the wording accordingly (Lines 400-402).

L. 386: Rephrase this taking into account that your dataset is also limited in terms of time and extent: "During the cruise period/for our dataset, we find that..."

Thank you for the suggestion. We have adjusted the related phrases as suggested (Line 409).

L. 400: "Nevertheless ... remain robust": Near surface temperature can be much colder than SST in near the ice edge on short time scales. Unless the authors have investigated this, the claim remains without evidence.

Thank you for the suggestion. We have revised the related statements to adopt a more cautious interpretation and reduced the strength of the wording accordingly (Line 431).

L. 403: The definition of an "anti-temperature effect" does not seem to add value, but seems rather confusing.

Thank you for your suggestion. We agree that the term 'anti-temperature effect' is confusing and have removed it accordingly.

L. 410: "spatial fractionation over the heterogeneous ice surface": Unclear what is meant with this statement.

Thank you for pointing this out. We agree that the original phrasing was unclear. We have revised the text to clarify that “spatial fractionation over the heterogeneous ice surface” refers to spatial variability in isotopic fractionation associated with differences in surface conditions (Lines 440–441).

L. 412: Warm-air intrusions may be more of a spring phenomenon. How much of a limitation is it for your study/interpretation that it only covers the summer season?

Thank you for this comment. We clarify that our intention is not to attribute the observed isotope–temperature relationships to warm-air intrusions. While previous studies have interpreted similar relationships in terms of warm-air intrusions, our analysis focuses on summer Arctic conditions and highlights moisture transport and surface exchange processes as the dominant controls. The comparison with previous studies is intended to emphasize differences in interpretation rather than to invoke warm-air intrusions as a driving mechanism in this study.

L. 430: "intrinsically tied to sea-ice state": The evidence provided here is not sufficient/consistent with this claim. Rephrase as speculation, and propose what is needed to more firmly make this claim.

Thank you for your suggestion. We have adjusted the this sentence as suggested (Lines 462-464).

L. 433: "These results provide the first in-situ isotopic observational constraints for evaluating ...": This sentence is a clear overstatement of the results from this manuscript, and not in line with the fact that there are other publications and datasets available that presented similar results. The sentence should be removed.

Thank you for the suggestion. We have adjusted the wording accordingly (Line 465).

L. 441: "We advocate a regime-informed dual-proxy framework": Unclear what is meant by that, simplify/clarify sentence.

We apologize for the confusion. Our intention was to emphasize that the co-variation between $\delta^{18}O$ and d-excess should be interpreted within the context of different sea-ice regimes, in order to help disentangle past temperature and sea ice signals and to link modern process understanding with paleo-isotope interpretation. To improve clarity, we have revised the sentence accordingly (Line 473-475).

L. 445-451: This paragraph would benefit from some moderation of the impacts of this study, given all limitation indicated so far.

Thank you for this suggestion. We agree that the implications of the study should be presented more cautiously in light of the limitations discussed above. Our intention in this paragraph was to highlight potential directions for future research rather than to imply that these advances have already been achieved by the present study. We have therefore revised the wording to moderate the strength of the statements and to emphasize the potential contributions of future work in these areas (Line 476-482).

L. 452: "systematically regulated by sea ice regime": This claim is unfounded. What the presented evidence shows, is that there is a bimodality in the measurements in clearly ice-free and ice influenced regions.

Thank you for this comment. We agree that the original phrasing overstated the strength of the evidence. Our intention was to highlight the contrasting isotopic characteristics observed under ice-free and ice-covered conditions, rather than to imply a systematic regulation by sea-ice regime. In the revised manuscript, we have reworded this sentence to more accurately reflect the observational evidence and to avoid overinterpretation of the underlying controls (Lines 483–484).

L. 459: "These results establish": Again, this claim is not backed up by the results.

Thank you for your suggestion. In the revised manuscript, we have rephrased this sentence to moderate the strength of the conclusion and to more accurately reflect the evidence provided by our analysis (Line 490-492).

We sincerely thank the editor and the reviewers again for their careful evaluation and constructive comments. We believe that all concerns have been adequately addressed in the revised manuscript, and we hope that the revisions meet your expectations. Please do not hesitate to contact me if you require any additional information to evaluate our contribution.

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