Dear Editor,

We thank you and the reviewers for the valuable time and constructive feedbacks on our manuscript. This manuscript received two detailed and very constructive reviews, which have been carefully considered in preparing the revised manuscript. In almost all cases, we were able to make substantial improvements based on the reviewer comments. Among these revisions, the most substantial changes include 1) correcting the isotopic end-member of advected moisture and re-running the MixSIAR model, 2) improving the HYSPLIT configuration by computing ten-day backward trajectories at five vertical levels, 3) providing a more detailed description of the data and methodology, and 4) expanding the comparison with previous studies to better contextualize our study within the existing literature. Although these edits and additions have not altered the results or key conclusions of our work, we believe they have greatly strengthened the manuscript. Below is a point-by-point response to each reviewer comment.

Response to Reviewer 1

Comment 1.1: 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.

Reply: 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)

Comment 1.2: 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).

Reply: 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).

Comment 1.3: 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.

Reply: 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.

Comment 1.4: 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.

Reply: 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).

Comment 1.5: 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.

Reply: 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.

Comment 1.6: 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.

Reply: 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.

Comment 1.7: 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.

Reply: 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.

Comment 1.8: 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).

Reply: 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.

Comment 1.9: The description of the use of MixSIAR is insufficient and currently not reproducible.

Reply: 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.

Comment 1.10: 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.

Reply: 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.

Comment 1.11: 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?

Reply: 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.

Comment 1.12: Line 12: 'ice-phase processes'. You mean 'in cloud ice phase processes'?

Reply: 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.

Comment 1.13: Line 21: Change 'These parameters' to 'these isotopes fractionate'

Reply: Thank you for your suggestion. We have revised the wording accordingly in the updated manuscript (Line 21).

Comment 1.14: Line 31: Add reference to these studies after the words 'ice-free ocean'

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

Comment 1.15: Line 35: Which sources are meant here, local or lower latitude sources?

Reply: 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.

Comment 1.16: Line 42: This is not consistent with Thurnherr and Aemisegger (2022) who observed negative d-excess in extra-tropics.

Reply: 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.

Comment 1.17: Line 130: What sources were used as input.

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

Comment 1.18: 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?

Reply: 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.

Comment 1.19: 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…..

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

Comment 1.20: Line 179: no more so on relative humidity refer to Pfahl and Sodeman (2014)

Reply: 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).

Comment 1.21: Line 204: The word 'trajectory' is confusing in this context, you mean 'curve'?

Reply: We apologize for the confusion caused by the original description. Accordingly, we have replaced "trajectory" with "curve" in the revised manuscript (line 243).

Comment 1.22: Line 231: you mean d-excesss in surrounding vapour

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

Comment 1.23: Line 236: supersaturation was only mentioned in relation to cloud processes?

Reply: 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).

Comment 1.24: Line 278: supersaturation is not a process but a condition? You mean crystal formation under supersaturated conditions?

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

Comment 1.25: Line 281: It is usually the opposite, air masses from the south are warm and moist in general, please explain.

Reply: 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.

Comment 1.26: Line 301: (single) 10m trajectory arrival does not necessarily represent the moisture source.

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

Response to Reviewer 2

Comment 2.1: The key motivating point of the manuscript is the discrepancy between different studies that show divergence relationships between d-excess and sea ice coverage. This is a good question (the introduction is very well written), and one that can be addressed with this dataset. However, the analyses as presented do not directly address this problem.

Reply: We thank the reviewer for this constructive comment. We acknowledge that the original manuscript did not sufficiently articulate how our analyses address this discrepancy. To clarify, we have now revised the relevant text to explicitly link our findings to this question. Specifically,

our results show that d-excess does not respond monotonically to sea ice concentration; instead, it exhibits a bimodal distribution (Fig. S1), indicating that high d-excess values can arise from two distinct processes: local evaporation and Rayleigh distillation during long-range transport. This observed bimodality helps explain why previous studies have reported divergent relationships, depending on the relative contributions of local evaporation from sea ice loss versus transport-related distillation. In the revised manuscript, we have expanded the Discussion to elaborate on this point (see Lines 379-389 for details).

Comment 2.2: The authors simply use the Merlivat and Jouzel 1979 (MJ79) model for predicting the local evaporation signal in the MixSIAR model (section 3.6), despite this being the focus of the initial questions of the manuscript. They do examine MJ79 results compared to the observations, but only for air temperatures over 5 C. Why not try this with lower temperatures? I suspect it breaks down against the observations, which then brings into question why it is used in the mixing model. These

Reply: We thank the reviewer for this constructive suggestion and acknowledge that the original description lacked clarity.

Vapor isotopic behaviors at temperatures below approximately 5 °C can be reasonably interpreted within a Rayleigh distillation framework. In contrast, at temperatures above ~5 °C, d-excess shows an opposite response to temperature compared to those at lower temperatures, and the data progressively deviate from the Rayleigh line (Figs. 4 and 6). To interpret the enhanced variability observed at these higher temperatures, we applied the MJ79 model as an illustrative framework to explore the potential role of local evaporation over open water. MJ79 is used here as a tool to estimate the isotopic composition of local evaporation, rather than for formal model validation, whose applicability has been discussed in previous studies (e.g., Bonne et al., 2019).

In response to the reviewer's suggestion, we have revised Fig. 7 to include MJ79 simulations at temperatures below ~5 °C. The model successfully captures observed d-excess variability at temperatures above ~5 °C, but fails at lower temperatures, where moisture advection dominates and the closure assumption is invalid. The breakdown of MJ79 at lower temperatures provides further support for our interpretation, indicating a substantial contribution from non-local vapor sources in these conditions.

In the revised manuscript, we have explicitly clarified these points to ensure a clear understanding of the MJ79 application, its limitations, and the rationale for its use in interpreting local evaporation signals (see Lines 313-321 for details).

Comment 2.3: In addition to the local signal question above, there are other questions with how the mixing model is designed to address the key questions. There are very few details presented on how the various end members are defined; this needs to be clearer. As described

many times previously in the manuscript, there are isotopic changes from the source to the observation site during advection of the different air masses. If is unclear how, if at all, Rayleigh distillation is dealt with in this model. There will be considerable changes to those lower latitude end member air mass isotopic values by the time they reach the Central Arctic.

Reply: We thank the reviewer for these detailed and constructive comments. We agree that clearer documentation of endmember definitions and isotopic modification during transport is essential for clarifying the assumptions and improving the transparency of the mixing model.

In the revised manuscript, we have expanded Section 2.5 to provide a detailed description of each endmember, including the data sources and assumptions used to represent local evaporation and low-latitude air masses (Lines 142-168).

We also recognize the importance of accounting for Rayleigh distillation during transport for low-latitude moisture sources. To address this, we have incorporated an additional Rayleigh fractionation correction for the low-latitude endmembers. Specifically, we applied a simple Rayleigh distillation model to adjust the initial isotopic composition of the low-latitude source, using the average specific humidity and temperature observed in the central Arctic lower troposphere during the study period as the final state of the distillation process. The fraction of remaining vapor was estimated from the ratio of Arctic to source-region specific humidity. This correction was applied to both $\delta^{18}O$ and δD to generate a "transport-corrected" endmember for use in the MixSIAR model. The procedure, including the equations and parameters used, is described in detail in Section 2.5.

We then re-ran the MixSIAR model with the updated setting and present the results in revised Fig. 9. This transport-corrected approach allows us to more realistically account for isotopic modification during long-range advection, improving the physical interpretation of the MixSIAR results.

We sincerely appreciate the reviewer's insightful comments, which have helped us clarify the scope, assumptions, and limitations of our mixing model, substantially improving the robustness and transparency of the analysis.

Comment 2.4: More details and justification on HYSPLIT methods needed. With the broad goal of defining where the moisture is coming from, the selected approach here does not seem to match the project needs. A simple 5 day back-trajectory from a single height only tells us where the air at that height came from over that period. Finding the source of moisture with back-trajectories requires looking at humidity changes (moisture uptake) and/or other property changes over the trajectory (e.g., Sodemann et al. 2008). 5 days has been shown to not be long enough at times in the Arctic with significant recirculating in the central Arctic basin and/or long transport trajectories from lower latitudes. Also, small differences in initialization height are known to, at times, result in considerably different trajectories depending on the wind

distribution above the surface as represented by the reanalysis data product. It would be ideal to initiate the trajectories from at least several heights near the surface to ensure that appropriate air mass transport is captured.

Reply: We thank the reviewer for these constructive comments regarding the HYSPLIT analysis. Our original aim in using the HYSPLIT model was to identify the pathways and origins of air parcels and to examine their meteorological properties—such as 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.

We acknowledge the reviewer's concerns that a 5-day, single-height trajectory without moisture tracking is limited for this purpose. To strengthen the qualitative interpretation while remaining consistent with our original aim, we have revised the trajectory configuration in three complementary ways. First, to better capture the longer residence time of water vapor in the Arctic and the possibility of recirculation or long-range transport, we extended the back-trajectory calculations from 5 to 10 days (Gimeno et al., 2021; Thurnherr et al., 2020). Second, to reduce sensitivity to the choice of initialization height, we performed additional simulations from three starting heights (10, 30, and 50 m). As shown in the revised Figs. S2–S4, the inferred transport pathways are qualitatively consistent across these heights. Third, to better link the trajectory information to moisture transport, we revised the weighting scheme to incorporate specific humidity along each trajectory (see Section 2.5), providing a more representative basis for comparing air-mass characteristics across source regions than trajectory frequency alone.

Together, these modifications allow a more robust qualitative assessment of air-mass origins and characteristics, while explicitly acknowledging that a full moisture-uptake analysis remains beyond the scope of this study. The revised Discussion now explicitly discusses the remaining uncertainties associated with the HYSPLIT approach and clarifies that the trajectory analysis is intended as a qualitative, supporting tool rather than a primary basis for the moisture-source attribution (see Lines 423-425 for details).

Comment 2.5: One other area that could benefit the manuscript is putting the observed relationships into context of prior observations. These observed relationships are the strength of this study, and should be emphasized. The suggestions here are not to take away from or question the analyses presented, but could provide important context to these new observations. For example, see how the spatial patterns compare with other cited studies, e.g., Brunello et al. (2023) in the Central Arctic basin. Examine how the slopes of d-excess relationships with various parameters (e.g., RH) compare with those observed in similar regions, e.g., Bonne et al. (2019). How do the fractions of local evaporation contributions compare with other observational or modeling studies (done with or without water isotopes)?

Without having to do any major new or overhauled analyses, my suggestion would be to really

focus in on these observed relationships, compare them with other studies, and highlight the consistencies and discrepancies found here. The uniqueness of this large spatial observational assessment gives an important opportunity to do this effort, and, to me, would be a quite valuable contribution from this effort.

Reply: We highly appreciate the reviewer's constructive and thoughtful comments. In response, we have strengthened the Results and Discussion sections by explicitly placing our observed relationships in the context of previous observational studies. Specifically, we now compare the spatial patterns of the observed d-excess relationships with those reported for the central Arctic Basin in earlier work (e.g., Brunello et al., 2023; Section 4.1). We also discuss how the slopes of the d-excess relationships with key controlling variables (such as relative humidity) compare with those reported in similar Arctic and polar environments (e.g., Bonne et al., 2019; Section 4.2). In addition, our estimated fractions of local evaporation contributions are now discussed in the context of previous observational and modeling studies, both isotope-based and non-isotope-based (Section 4.3).

These comparisons highlight consistencies and discrepancies with earlier studies, emphasizing the value of our large-scale spatial observations. We thank the reviewer for this suggestion, which helped us better contextualize and underscore the relevance of our results.

Comment 2.6: Line 63. More details needed on the sampling setup. For example: where on the ship was the inlet located; was the inlet tube heated?

Reply: We thank the reviewer for this helpful comment. In the revised manuscript, we have expanded the Methods section to provide a more detailed description of the sampling setup (see Lines 64-70 for details).

Comment 2.7: Line 97. Consider changing the name of 'Melt Region' to something like 'Open Water Region' as there is not necessarily any melt associated with the given region.

Reply: This is a good point. In the revised manuscript, we have replaced "melt regions" with "ice-free regions" throughout the text and figures.

Comment 2.8: Figure 1 and 3. What is the temporal resolution of the datasets?

Reply: We apologize for the confusion. The measurements were recorded at 1-second intervals, and for Figures 1 and 3 we extracted one representative data point per hour. This has been clarified in the revised manuscript (see Line 74).

Comment 2.9: Figures 4 and 5. Consider adding a panel showing all of these together, and/or make the axes the same for each panel. It is not straightforward to compare the relationships as plotted other than comparing slopes.

Reply: Thank you for this suggestion. In the revised manuscript, we have combined the related

panels of Figures 4 and 5 into a single multi-panel figure to facilitate direct comparison of the relationships. The axes are now consistent across panels, improving interpretability.

Comment 2.10: L228 and Figure 6. 'Steeper than predicted'…with a single Rayleigh curve. But the air masses are a mixture of moisture sourced from many locations (with different temperatures, etc). I would suggest including several Rayleigh curves starting from different initial air masses. This might better show the mixing the authors are trying to show with their mixing model.

Reply: Thank you for this thoughtful suggestion. We agree that using multiple Rayleigh curves with different initial conditions could help illustrate mixing among air masses with diverse source characteristics.

We tested $\delta^{18}O$ Rayleigh curves using a range of plausible starting temperatures and humidity conditions representative of potential source regions (e.g., North Atlantic open water, marginal ice zones). As shown in Figure 6, these curves reasonably bracket the observed $\delta^{18}O$ values, supporting the interpretation of progressive fractionation during transport.

For d-excess, however, even multiple starting points cannot capture the full observed variability, because d-excess is highly sensitive to non-equilibrium processes such as ice crystal formation (Samuels-Crow et al., 2014), dew deposition under supersaturation (Thurnherr et al., 2022), or sublimation from snow and sea ice surfaces (Bonne et al., 2019). Small contributions from these processes can produce substantial deviations from ideal Rayleigh behavior. Therefore, we present Rayleigh curves modified for supersaturated conditions to illustrate the potential influence of non-Rayleigh processes, while acknowledging that their exact combination and magnitude remain uncertain.

These clarifications have been incorporated into the revised manuscript, along with a discussion of the limitations of interpreting d-excess solely within a Rayleigh framework (see Lines 278-280).

Comment 2.11: Figure 7. Why only look at conditions greater than 5 C (see 1a above for more)? Also, both panels of the plot seem like they are showing the same d-excess data, not d18O and d-excess data.

Reply: We apologize for the errors in the original Figure 7, in which both panels incorrectly displayed d-excess data. The figure has been corrected in the revised manuscript to show $\delta^{18}O$ and d-excess separately.

The choice to focus on conditions above 5 °C is based on the patterns shown in Figures 4 and 6. Above this temperature, the relationships between d-excess, $\delta^{18}O$, and temperature are reversed compared to those at lower temperatures, and these data points generally lie above the

Rayleigh curve. This subset likely reflects a transition from Rayleigh-dominated processes to additional influences, motivating our focus on these data

Comment 2.12: Line 335. Should this say 'producing lower d-excess' instead of higher?

Reply: Thank you for the reminder. We carefully re-examined the manuscript and confirm that "higher" is appropriate in this context. Our measurements show elevated d-excess values in both ice-covered and ice-free regions (Fig. 2); however, the underlying fractionation pathways differ.

In ice-covered regions, high d-excess is mainly due to equilibrium fractionation under very low specific humidity, consistent with previous studies (Samuels-Crow et al., 2014; Galewsky et al. 2016). In contrast, in ice-free regions, elevated d-excess primarily reflects local evaporation under non-equilibrium conditions. This distinction also explains why previous studies reported contrasting relationships between d-excess and sea ice (Bonne et al., 2019; Klein and Welker, 2016).

We have revised the manuscript to clarify this distinction (see Lines 333-335). Thank you again for your insightful suggestion.

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.

Reference

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