The manuscript by Zhang et al. uses a new set of cruise-based water vapor isotopic observations to examine Arctic evaporation over a broad spatial scale and range of sea ice conditions. The manuscript presents a valuable observational dataset. Pan-Arctic observations from a single cruise campaign are challenging to conduct, where measurements are collected in a relatively short time window, providing important like-for-like comparisons. The dataset on its own is a valuable contribution, and leads this manuscript to having value as a publication. However, there are some potentially questionable analyses and conclusions drawn that warrant further examination before publication.

In general, the observations presented in this study are generally sound and important, but the interpretations and conclusions drawn do not always fit the analyses. I describe some of these below.

Major comments:

1. Motivation of paper vs analyses presented.

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.

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).

a. Defining the local evaporation signal

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

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).

b. Use of mixing model

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.

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.

c. Back-trajectory analyses.

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.

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).

2. Context of analyses

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.

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.

Minor comments.

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

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).

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.

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

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

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).

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.

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.

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.

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).

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.

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

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

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.

References

Bonne, Jean-Louis, et al. "Resolving the controls of water vapour isotopes in the Atlantic sector." Nature communications 10.1 (2019): 1632.

Galewsky, Joseph, et al. "Stable isotopes in atmospheric water vapor and applications to the hydrologic cycle." Reviews of Geophysics 54.4 (2016): 809-865.

Gimeno, Luis, et al. "The residence time of water vapour in the atmosphere." Nature Reviews Earth & Environment 2.8 (2021): 558-569.

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.

Samuels-Crow, Kimberly E., et al. "Deuterium excess in subtropical free troposphere water vapor: Continuous measurements from the Chajnantor Plateau, northern Chile." Geophysical Research Letters 41.23 (2014): 8652-8659.

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, 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.

References

Bonne, J. L., Behrens, M., Meyer, H., Kipfstuhl, S., Rabe, B., Schönicke, L., et al. (2019). Resolving the controls of water vapour isotopes in the Atlantic sector. Nature communications, 10(1), 1632.

Brunello, C. F., Meyer, H., Mellat, M., Casado, M., Bucci, S., Dütsch, M., & Werner, M. (2023). Contrasting seasonal isotopic signatures of near-surface atmospheric water vapor in the central Arctic during the MOSAiC campaign. Journal of Geophysical Research: Atmospheres, 128(24), e2022JD038400.

Sodemann, H., Schwierz, C., & Wernli, H. (2008). Interannual variability of Greenland winter precipitation sources: Lagrangian moisture diagnostic and North Atlantic Oscillation influence. Journal of Geophysical Research: Atmospheres, 113(D3).