Revision of "Spatially distributed measurements of aerosols and stable isotopes in water vapour and precipitation in coastal Northern Norway during the ISLAS2021 campaign" by Dekhtyareva et al., submitted to ESSD

Dear Editor,

*we have now revised our manuscript based on the comments from three reviewers. We feel that we could address all comments constructively, even if on some occasions we chose to refrain from more detailed and extensive analysis to remain within the scope of a data description paper, rather than a more regular scientific analysis. There are already several publications in preparation that will start to explore the potential of the ISLAS2021 dataset, which we do not want to compromise by too extensive analysis in the data description paper. Our replies are included with the reviewer comments below in italics and marked by **Reply**.*

Reviewer #1:

We thank the reviewer for their helpful and constructive comments.

This manuscript presents a huge dataset obtained during an observationnal atmospheric campaign in March 2021 in Norway including a lot of complementary measurements (aerosols, water isotopes) at different locations using facilities at different sites. The description of the acquisition and calibration methods is done carefully (but could be sometime a bit reduced to avoid repetitions). While no interpretation of the data is presented (as expected for a ESSD paper), the complementarity of the measurements is presented which opens the way to future studies.

This manuscript is in the scope of ESSD, the data are usefull for further studies of the cloud microphysics and the data quality is well assessed so that future users can do the best of this dataset. The manuscript can thus be accepted with only minor revisions as detailed below.

- In the abstract and several times during the manuscript, the spatial representativeness of vapour isotopes measurements is mentionned. This is an important added value of this study with several instruments running in parallel (despite the leak on one of the inlet lines). It would be nice to have also a statement in the « discussion – conclusion » on the spatial representativeness to have an idea of the scale of the processes which are studied here.

***Reply:** Three points in the discussion and conclusion section have been rephrased to more clearly forward our findings regarding the spatial representativeness of the horizontal water vapour and precipitation isotope measurements, and the vertical gradients.*

- In the introduction, the potential complementarity between INP and water isotopes is strongly underlined for process studies. However, later in the text, the INP data are not much described and it also lacks from the last section. Also the fact that the INP data are not made publicly available (but upon request) rise question on the usefullness of this dataset. I encourage the authors to detail more in a revised version the potential use of the INP data and its link with water isotopes data, not only in the introduction.

***Reply:** We would like to thank the reviewer for pointing this out. We have now rephrased the description of the aerosol INP data to make it clear that in fact it is available and have now added the precipitation INP data to the repository as well. The Data availability section has been updated accordingly.*

To demonstrate the power of combining INP and SWI measurements, we have included the following figure (now Fig. 14 in manuscript) in Section 6 (Combined case study for IOP2), which shows the decrease in aerosol INP concentration at different temperatures with increasing delta D values in the vapor. The general expectation is that precipitation removes the most active INPs. However, the amount of precipitation removed from the airmass (more depleted delta D) does not reduce the INP concentration, pointing to a local INP source during the IOP2 warm air intrusion.

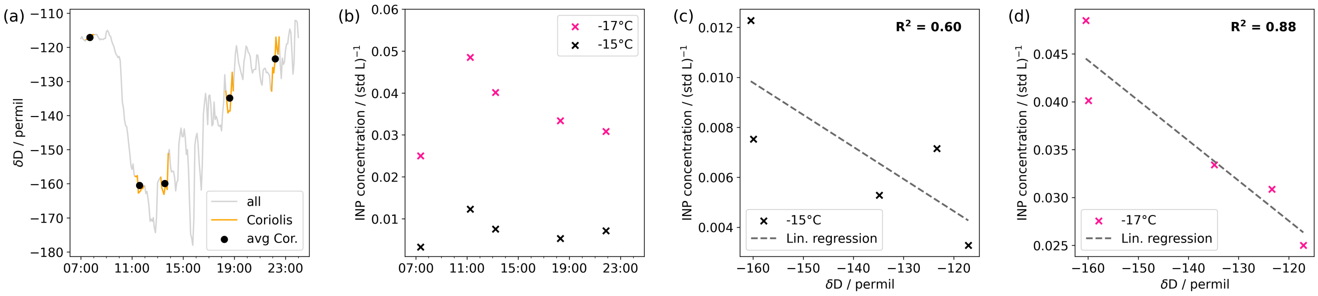

Figure R1*: Synthesis of water isotope and INP measurements at Andenes for a time period within IOP2 (20 to 21 March 2021). (a) water vapour delta D (permil) at site Coast (all data, grey line), during coincident INP measurements (Coriolis, orange line), and average over 40min of coincident INP measurements (avg Cor., black circle). (b) INP concentration (std L^{-1}) at -15°C (black crosses) and -17°C (pink crosses). (c) Dependence of INP concentration at -15°C on delta D (black crosses) and linear regression line (black dashed line). (d) Dependence of INP concentration at -17°C on delta D (pink crosses) and linear regression line (black dashed line).*

This figure is accompanied by the following text in Section 6:

“During IOP2, five aerosol INP measurements were conducted that coincided with the SWI measurements. While the vapor delta D values rapidly decreased from -120 permil to -175 permil in the beginning of IOP2 before increasing again to -115 permil at the end of IOP2 (Fig. 1a), the measured INP concentrations at -15°C and -17°C show the opposite behavior (Fig. 1b). Indeed, we find an anticorrelation between the INP concentrations and delta D measured in air throughout IOP2 (Figs. 1c and 1d, R^2 values of 0.6 at -15°C and 0.88 at -17°C, respectively). The general expectation is that precipitation removes the most active INPs from the airmass during transport. Consequently, as the amount of precipitation removed from the airmass (more depleted delta D) does not reduce the INP concentrations, this suggests a local source of INPs throughout the IOP2 warm air intrusion. This demonstrates the added value of combined SWI and aerosol INP measurements pertaining to e.g. INP source attribution.”

Furthermore, we elaborated on the synergy between INP and SWI measurements in the conclusions (Section 7, Point 6) as follows:

“6. Aerosols and INPs complement water isotope and precipitation sampling meaningfully, as both are related to microphysical processes within and below clouds. In particular, their synergy allows for INP source attribution (e.g. local vs. remote source regions) depending on how much distillation the SWIs indicate.”

- l. 52,you already write about the d-excess while it is only defined 5 lines below.

Reply: *We have removed the mentioning of d-excess from this sentence.*

- l. 101-105 and after. You describe very well the sampling strategy but the main scientific question addressed by this campaign is missing. It would be nice to explicit it.

Reply: *L. 103 states the main aim, but we agree that it is an advantage to also clearly state the main scientific question. We have revised this section to clearly state the overall scientific objective ("The main scientific aim of the ISLAS2021 campaign was to collect a dataset that would allow for the assessment of water turnover in arctic weather systems with a focus on cloud processes and precipitation."). We have also rephrased the specific objectives to highlight aspects such as representativeness of isotope measurements, paired vapour/precipitation measurements, and the combination of stable water isotope with aerosol/INP measurements to enable addressing the scientific aim.*

- Table 3. I was wondering why the data acquisition could not be run continuously.

Reply: *The aerosol lidar system at ALOMAR requires one to open the main hatch of the building, and thus a sufficiently long precipitation-free period to operate the laser system without risking that water enters the building, as well as restrictions due to air traffic at the nearby airport. This is now mentioned in the caption of Table 3 and mentioned in the revised manuscript.*

- Paragraph starting from l. 259 : it would help to have some ideas here on the sampling frequency and invoke the possibility of post-deposition effects (which are adressed later but it would be nice to introduce them here)

Reply: *Good point. The sampling frequency is visible from Fig. 5b. The potential for post-depositional processes depends both on the sampling frequency and the time of collection after the end of the snowfall event, which is not known at all box locations. In the revised manuscript, we added a sentence that specifies that the typical duration until sampling was 1 day or less for Transects T2-T5, and two days or more for T6-T8, increasing the potential for post-deposition effects to modify the precipitation signature.*

- l. 342-343 : I was wondering why the CRDS analysers could not operate continuously

Reply: *The CRDS analyzers were measuring ambient air continuously, except during short calibration periods, or when an analyzer was not measuring due to other issues. As is shown in Fig. 5, all 4 analyzers were operating for most of the campaign period, with the exception of the Tromsø installation, which was sampling room air until the installation was fixed. Unfortunately, there was a calculation error in the up-times in the previous version of the manuscript. We have corrected this mistake now, stating the correct up-times as follows:*

"At least two CRDS were operating during 99.2% (158.8h) of the campaign duration, at least 3 analyzers were measuring during 85.7% (137.2h), and all four analyzers were operating during 23.5% (37.6h) out of the 160.1 h of measurement time. When including the period of the inlet leakage in the calculation, the percentage when all four CRDS were measuring increases to 58.3 % (93.2h)."

- Caption of Figure 5. Can you explicit how you evaluate the data quality for the different instruments ?

Reply: *We amended the caption of Fig. 5 to spell out specifically what limits the data quality of the MRR at ALOMAR (height range) and the CRDS at Tromsø (inlet leakage) as indicated by the light blue and red shading.*

- l. 375 : Why did you change the standard on 21 March 2021 ?

Reply: *The reservoir bag for that water standard was running low and was therefore exchanged. The text has been updated accordingly.*

- l. 385-387 : Can you explicit why the analytical uncertainty for d18O is 1.25 permil on l. 386 and on l. 387, you mention that the total propagated uncertainty is 0.14 permil for d18O. In general, for this entire section, I had difficulty to understand how the number for uncertainties are evaluated. Since there are a lot of repetitions in this section (uncertainty calculation for each analysers), I would suggest to rewrite the section giving more details on the uncertainty calculation (or present some graphs) and then avoid repeating for all analysers except when there is a specificity as for the anlyser with a leak in the inlet line. A table to summarize the uncertainties for each analyser may also help reducing repetitions.

Reply: *Unfortunately, the wrong uncertainties were given for $\delta^{18}O$ in L. 386 (1.25 permil instead of 0.14 permil), this has been corrected in the revised manuscript.*

After reconsidering how to compute the uncertainty budget for vapour measurements, we have now adopted a more detailed approach that is also used for liquid sample analysis, and transferred this to the vapour measurements. The complete uncertainty budget is then obtained by calculating the combined uncertainty from the uncertainties of the calibration standards assigned value, the measurement of the calibration standards, and the uncertainty of the measured sample, each weighted by the sensitivity on the calibration slope (Sodemann et al., 2023). Thereby, these uncertainties are estimated from the standard deviation of an averaging interval. The sections of the manuscript where the vapour isotope measurement uncertainty is relevant, such as for the assessment of spatial representativeness, have been edited accordingly. In addition, we present the uncertainty calculations in a common section for all analyzers, and show the uncertainty budget components in the new Table 5.

- l. 453 – 454 : I do not understand the sentence – especially, I do not understand the use of the first sample. Probably rewriting a bit this sentence would help clarifying.

Reply: *We have rewritten and shortened this section, which contained to much detail and information that is available in the main reference for this section (Sodemann et al., 2023). The sentence referred to by the reviewer has been removed from the revised manuscript.*

- Figure 9 : It would help to have a label for the x-axis.

Reply: *A date label has been added to the x-axis of Fig. 9.*

- Caption of Figure 11 : not sure that it is clear for the reader that d_ALOMAR is the d-excess since it was not defined as such in the manuscript before.

Reply: *In the revised manuscript, we now introduce the symbols in the text explaining Figure 11. We also corrected ΔD to Δd in the caption to panel b.*

- l. 616-617 : can you better describe the « another step at about 12 :30 UTC »

Reply: *This refers to the step-like temperature change at about 12:30 UTC for the black line in panel b. The sentence has been rephrased for clarity.*

- l.645 – 651 : it would be interesting to further detail the processes at play when comparing dD in precipitation and dD « expected from equilibrium fractionaton ». I find this section lacks from explanations on what has been calculated and how we can interpret processes such as exchanges between snow and water vapor or vertical advection. No reference is given also to support this interpretation. Please complete this section.

Reply: *We have revised this section, as well as the description of Fig. 8, using the concept of equilibrium vapour as applied for example by Graf et al., 2019. The description also mentiones the role of below-cloud exchange processes between water vapour and precipitation in influencing the respective isotope composition. We however keep the description and discussion at a minimum to not venture too far into (forthcoming) data analysis in the present manuscript.*

- Figure 13 : I think that there are some problems of coherency between the figure and the caption (caption describes panels from a to d while there are panels from a to e on the figure).

Reply: *The panel labels for this figure have been corrected in the revised manuscript.*

References

Graf, P., Wernli, H., Pfahl, S., and Sodemann, H.: A new interpretative framework for below-cloud effects on stable water isotopes in vapour and rain, Atmospheric Chemistry and Physics, 19, 747–765, https://doi.org/10.5194/acp-19-747-2019, 2019.

Reviewer #2:

*We thank the reviewer for their helpful and constructive comments. Our replies are included with the reviewer comments below marked by **Reply**.*

This manuscript documents an extensive observational effort during the ISLAS2021 campaign in coastal Northern Norway, combining water vapour and precipitation stable isotope measurements with aerosol, INP, and meteorological observations. The paper is generally well-structured, and the dataset is of clear value to the community. However, I have several substantive concerns and recommendations that should be addressed.

1) The Introduction are presented largely in parallel rather than converging towards clearly articulated goals. It remains unclear what the central scientific questions motivating ISLAS2021 are. The authors should explicitly state primary scientific questions, and clarify how the observational strategy was designed to address them. This would greatly strengthen the coherence of the manuscript.

***Reply:** A similar remark has been made by reviewer #1. The scientific questions were entangled with the main objectives in the previous manuscript. We have now revised the introduction following the suggestion of the reviewer. In addition, we made a number of smaller edits to the introduction to clarify the narrative of the research gaps and how addressing those is enabled by the presented dataset.*

2) A key strength of ISLAS2021 is the simultaneous measurement of water isotopes, aerosols/INPs, and meteorology. However, beyond the Introduction, the manuscript largely treats these components separately, with limited synthesis explaining how they jointly constrain atmospheric processes. The authors should include a short synthesis section or paragraph explicitly describing: how the different measurement components complement each other; which combinations of variables are expected to be most informative; and which limitations currently prevent a full joint analysis.

***Reply:** In light of this and a comment from Reviewer 1, we have expanded on the link between INP and SWI measurements by including a new Fig. 14 and additional discussion in Section 6 and in the Conclusions.*

3) Spatial representativeness of vapour isotope measurements is repeatedly mentioned as a key motivation for the distributed network. However, the manuscript does not clearly summarize what spatial and temporal scales can realistically be probed with the available data. The authors should provide a qualitative (or semi-quantitative) assessment of the representativeness and scale of the observations, explicitly linking this to the network geometry and data availability.

***Reply:** We think that a detailed assessment of the spatial representativeness is beyond the scope of this data description paper, as such an assessment would involve, for example, Lagrangian transport model calculations to assess when airmasses from one station reach the next one. We want to point out that we already briefly discuss the horizontal representativeness of the water vapour isotope measurements in Sec. 5.5 and Sec. 7. In the revised manuscript, we have followed the reviewer's suggestion and added a separate, semi-quantitative discussion on the aspect of representativeness before the Conclusions section:*

"We assess the spatial representativeness from the ability of the network to detect co-variations in the vapour isotope signals with or without time offsets, while taking into account the combined measurement uncertainty of the CRDS analysers. The ambient variation of isotope composition reaches often values of 20 permil for δD and 2.5 permil for δ18O within 30 min, more than an order of magnitude larger than the respective typical combined uncertainties of 1 permil and 0.15 permil (Fig. 6 and Fig. 13c). The collocated installation of two CRDS at different elevations provides confirmation that these variations are due to meteorological phenomena, and that they are not fundamentally compromised by the larger uncertainty of the CRDS at site Coast. The detection of local-scale differences for the d-excess, however, is only possible in situations where the uncertainty is less than the signal, for example during situations of stable stratification (Fig. 11). By detecting both time offsets, but also modifications of signals in δD and δ18O over distance, the measurement network allows one to detect how atmospheric processes such as rain evaporation, mixing, and also horizontal variations of air masses have modified the atmospheric water vapour isotope composition over the time it takes an airmass to pass the 100 km distance (typically 2-6 h). Examples for periods where such horizontal representativeness is evident are the large W-shape variations of isotope composition during IOP5, which is apparent near-simultaneously at Coast and ALOMAR, and with a delay of 2 hours also at Tromsø. The network thus captures consistent isotope signatures that are associated with meso to synoptic-scale phenomena at this 100 km scale. The measurement station in Bergen is generally more disconnected from the weather evolution in

the northern locations, but captures for example similar signatures as the northern network during the mCAO period of IOP3 (Fig. 6). Interestingly, co-variations in the d-excess are sometimes more obvious across the entire network than for δD and δ18O, such as during IOP5 (Fig. 6d). A more detailed analysis using trajectory calculations or similar transport modelling tools are needed to better understand why such larger-scale co-variations are sometimes present, and sometimes not."

4) The manuscript applies four different calibration strategies for the four CRDS analyzers, with large differences in uncertainty, particularly for the Coast site. While the authors describe these strategies in technical detail, the implications for data consistency and inter-site comparability are not adequately addressed. The authors must provide a concise, synthetic comparison of all analyzers, ideally in a single table, including calibration method and uncertainty ranges. The manuscript must explicitly state whether inter-site vapour isotope gradients can be meaningfully interpreted given the differing uncertainty levels. If no cross-site harmonization or bias assessment was attempted, this must be clearly acknowledged and justified.

Reply: *The points raised by the reviewer here have been addressed by revising the description of calibration strategies and uncertainty assessment as detailed in the response to reviewer #1 (comment regarding l. 385-387). Unfortunaly, the previous manuscript contained a wrong uncertainty of $\delta^{18}O$ for site ALOMAR (1.25 permil instead of 0.14 permil), which may have prompted the reviewer's comment. We acknowledge however, that the writing of Section 4.1 could have been more clear and concise, as also pointed out by reviewer #1. We have followed the suggestion of both reviewers and use the new Table 5 to present the uncertainty of all analyzers in the revised manuscript. A discussion of the ability of comparing CRDS measurements across the network has been included in response to comment #3 above.*

5) Although uncertainty estimates are provided for individual instruments, the manuscript lacks a high-level synthesis that translates these uncertainties into practical implications for data use. The authors should include a synthesis section that explicitly addresses: Whether observed isotope variability typically exceeds measurement uncertainty; What temporal and spatial scales can be robustly investigated using this dataset; Which analyses are likely to be uncertainty-limited.

Reply: *Figure 6 where representative uncertainties are plottet along with the data series depicts that the atmospheric signals are most of the time much larger than measurement uncertainty. Otherwise, this comment addresses a similar point as in comment #3. We have included a discussion of the spatial representativeness in the Discussion section in the revised manuscript.*

6) A significant strength of the manuscript is the detailed calibration discussion (Section 4.1), but it also reveals substantial differences in uncertainty across sites. Please summarize the spatial consistency and reliability of the data more explicitly in the main text (e.g., Section 5), and guide users on how to interpret or filter the lower-quality measurements. Are there any recommendations for users on which periods or sites are most reliable for process studies?

Reply: *A brief discussion regarding inter-site comparison of the CRDS measurements has been included in response to Comment #3. Information about which periods are suitable for different analyses is partly contained in Sec. 3.1 and the corresponding Fig. 3. While it is difficult to give general recommendations, or in our opinion not very useful to describe details of hypothetical analyses, the IOPs with their respective characteristics are natural starting points for data users. We also now mention the period where all CRDS were operating simultaneously in Sec. 3.2. Several specific examples for how the dataset can be used are given in the Conclusions section (see also comment #5 by Reviewer #3).*

7) The case study is a key element to demonstrate dataset utility but currently lacks depth. Please expand the analysis to more fully demonstrate how the SWI and INP data co-evolve across multiple sites (e.g., Bergen–Tromsø–Andenes). Consider including a figure showing temporal evolution of δ18O, δD, and d-excess during a representative IOP, ideally aligned with meteorological context (e.g., pressure, IWV, radar reflectivity). Is there any evidence of isotope distillation or below-cloud processes that you can highlight?

Reply: *Unfortunately, the INP measurements were limited to the coastal site at Andenes. But to highlight the co-evolution of INPs and SWI during an IOP, we have included Fig. 14 in Section 6, which shows a decrease in INP concentration at different temperatures with increasing delta D values, suggesting a local INP source during the IOP2 warm air intrusion.*

We chose to limit the analysis to one case study period for this data paper to not overload this data description paper. A comparison of the vapour isotope and specific humdity at the different sites as requested by the reviewer is already given in Figs. 6 and 7. To give more concrete indication of below-cloud effects, we have redrawn Figs. 8 and 13 using the equilibrium vapour concept (Graf et al., 2019), rather than using a constant offset for the isotope composition.

8) The dataset includes a valuable INP record using the DRINCO method, but the presentation is minimal. Please expand on the sampling frequency, sensitivity, detection limits, and types of INPs targeted (biological vs mineral dust?). Indicate whether INP data have been used previously or how they compare to other Arctic datasets (e.g., Ny-Ålesund, ARM sites).

Reply: *Thank you for raising this point, we have now added some details regarding detection limit and INP types in the manuscript at the end of Sect. 4.3:*

"When accounting for the 12 m3 of air sampled with the Coriolis impinger (Sect. 2.2.2) and a residual cone volume of approximately 15 ml, the minimum detection limit of INPs was ~2.5x10-4 std L^{-1} at temperatures warmer than -17°C. Due to the low-end cutoff size of the Coriolis (~0.5 μm), it is expected that the INP concentration reflects both biological and mineralogical INPs that are typically larger than this size."

In addition, we have added details on the sampling frequency in the manuscript at the end of Sect. 2.2.2:

"On average, two routine samples were taken each day, except for during IOPs when we had additional sampling (up to 5 per day), resulting in 51 aerosol INP measurements during ISLAS2021."

Furthermore, a comparison with previous literature studies in the Arctic as detailed in Gjelsvik et al. (2025) is discussed on Lines 557-559 in the original manuscript. Generally, our observations fall in line with previous ground-based Arctic measurements.

Reviewer #3

This manuscript demonstrates a valuable multi-platform dataset of stable water isotopes in vapor and precipitation from a high-latitude coastal environment. The observational effort is important, and the dataset has strong potential for studies of air–sea interaction, mixed-phase cloud processes, and isotope-enabled model evaluation. However, the conclusions can benefit from further quantifications and more explicit details of measurement uncertainty, spatial representativeness, and event-to-event variability. Below are my comments:

1. Line 535: Fig. 8a shows notable discrepancies between precipitation rates from the Parsivel2 disdrometer and the Andenes AWS, particularly during high-intensity periods (IOP1 and IOP5). Please clarify the likely causes (e.g., wind-induced undercatch for the AWS gauge, phase-dependent biases during snow/graupel, disdrometer sampling/quality-control limitations, siting/height differences, and any post-processing corrections). If possible, quantify expected uncertainty ranges for each sensor and indicate whether the differences affect subsequent isotope interpretations.

Reply: *We agree that this is an interesting aspect that can be mentioned in the context of Fig. 8. We included a sentence that summarizes possible reasons for the different response, such as undercatch of solid precipitation for the AWS rain gauge, but also artifacts that are in particular during cases of snow for the Parsivel^2. The uncertainty of the precipitation amount does not induce uncertainty of the isotope composition of the precipitation, unless one is interested in the amount-weighted isotope composition, as for example in hydrological applications:*

"*According to the Parsivel^2 disdrometer, the most intense precipitation was recorded early in the morning of 19 March 2021 during IOP1 (12 mm hr^{-1}), followed by the evening of 24 March 2021 during IOP5 (7.2 mm hr^{-1}). Precipitation recorded by the rain gauge at site Andenes, 4 km from site Coast, records precipitation amounts that are generally similar in overall amount and timing. The disdrometer and the rain gauge have different uncertainties that can contribute to such differences. Wind-related undercatch is a common problem for rain gauges (Wolff et al., 2015; Nitu et al., 2018), whereas the Parsivel^2 overestimates precipitation rates for solid precipitation (snow and melting snow). Both effects are likely to contribute to discrepancies between the precipitation measurements, in particular during IOP1.*"

2. Section 5.4: The transect analysis is valuable, and the manuscript notes potential artefacts (e.g., exposure time/evaporation in boxes and spillover for T3–T4). However, the conclusion that Coast measurements are "representative across the Lofoten archipelago" would benefit from clearer quantification. Please (i) define representativeness (which isotope metrics, which conditions), (ii) provide summary statistics (mean bias/spread vs distance; results with/without T3–T4), and (iii) discuss how limited spatial sampling and event-to-event variability constrain generalization beyond the sampled transects, particularly when inter-comparison with other products (e.g., satellite- or reanalysis-based gridded datasets) is needed to assess spatial consistency.

Reply: *We have rephrased the text in this section by specifying that we consider representativeness in terms of the inter-event variations in δD being larger than the within-event variation. Figure 10 is meant to represent such summary statistics visually to indicate the potential for a more quantitative analysis. We are not aware of satellite or reanalysis-based datasets that would allow us to make statements about the spatial representative of water isotopes in surface snow or precipitation. We feel that while interesting, an extensive discussion of these aspects is beyond the scope of a dataset description paper, as it would venture into a quantitative evaluation and dataset interpretation, rather than indicating dataset potential:*

"*Thus, while these transect samples deserve to be investigated more on a case-by-case basis, the fact that inter-event variations in δD of up to 60 permil are substantially larger than the standard deviation of the samples from a given transect (2-20 permil) clearly indicates the possibility to determine the representativeness of Coast precipitation isotope measurements for the 100 km long transect across the Lofoten archipelago.*"

3. Line ~605: You state that the strongest negative vapor isotope gradients occur during mCAO periods when surface fluxes and non-equilibrium fractionation are strongest. This is plausible, but please add physical support: e.g., show/quote the coincident surface flux or stability indicators during mCAO and clarify the mechanism linking enhanced fluxes to the sign/magnitude of ΔδD and Δd-excess. Since you use 10-min averages due to a few-minute site time offset, please also provide a brief sensitivity check (e.g., 10 vs 30 min) to demonstrate the classification result is not an artefact of averaging choice.

Reply: *We have repeated the analysis in Fig. 10 using 30-min time averaging and find almost the exact same results (Fig. R2). We briefly state in the revised manuscript that the detected gradients are quantitatively very similar when averaging at a 30 min interval. The argument for the stronger gradient is that the near-surface vapour is actively influenced by surface evaporation flux, which takes place under stronger non-equilibrium conditions during mCAOs (e.g. Duscha et al., 2021; Sodemann et al., 2024). The positive d-excess signature is an integrated signature from water vapour advection over the open water upstream, and the CAO index means that the ocean is warmer than the atmosphere. In the absence of local surface flux and atmospheric profile measurements over the open ocean in the dataset, we use citations from relevant literature to support our claim here.*

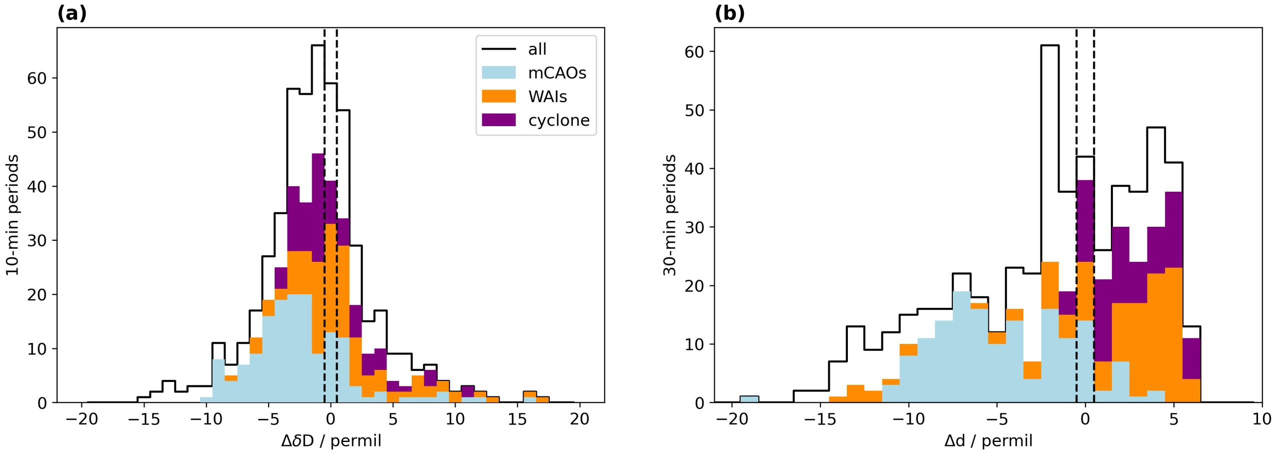

Figure R2: Vertical gradients in water vapour isotope measurements between sites ALOMAR and Coast of 30 min averaged measurement. Compare to Fig. 11 in the manuscript.

4. Section 6 (IOP2 case study): The IOP2 multi-instrument synthesis is very informative, including the period where the MRR stops recording precipitation while Parsivel/AWS indicate higher rates. Please add a brief cross-IOP context: are the key isotope–meteorology linkages seen in IOP2 also observed in other IOPs (especially IOP1 and IOP5)? Even a compact table/paragraph summarizing which features repeat versus which are event-specific would help users interpret representativeness.

Reply: *With regards to other IOPs, we note in the discussion of Figs. 6 and 8 that some similarities, but also differences between IOPs are present. It is difficult to fully address the reviewer's request without diving deeper into the scientific analysis of the dataset that we consider consistent with a data description paper. However, we now note in Sec. 6 commonalities between several IOPs, namely that the precipitation δD is more negative than vapour in IOP1 and IOP5, and that we see co-variations in the aerosol size distribution with δD. We state that these co-variations are not uniform across all events, but clearly motivate further investigation of the interrelation of aerosols and water isotopes.*

5. Discussion: The manuscript suggests the dataset can support improvements in NWP/model prediction for high-latitude mixed-phase precipitation. Please specify what aspects are most directly constrained by these observations (e.g., cloud liquid/ice partitioning, precipitation formation efficiency, low-level evaporation/sublimation, boundary-layer mixing) and outline a concrete example of model evaluation (which variables to compare, at what time/vertical resolution, and how isotope information adds value beyond standard thermodynamic fields).

Reply: *Thank you for this valuable suggestion. Some of the requested information is already provided in the introduction, and in the rephrased scientific objectives. The Conclusion section also provides an extensive list with specific examples suggesting where aerosols/INPs and water vapour isotope measurements can be used in combination to improve numerical models:*

"These include, for example, process studies and model validation of coastal mixed-phase clouds and precipitation in convective and stratiform cloud regimes, the understanding of INPs for sub-Arctic precipitation processes, improving Earth System Models for the present day Arctic climate (Gjelsvik et al., 2025), the assessment of the representativeness of stable water isotope measurements in water vapour and precipitation on a scale of up to 1000 km in different weather situations, the quantification of precipitation efficiency in high-latitude storms from stable water isotope measurements, and the analysis of the d-excess as a tracer of moisture source conditions."

We believe this already corresponds to what the reviewer requests, and therefore decided not to repeat some of same information in a Discussion section.

References

Wolff, M. A., Isaksen, K., Petersen-Øverleir, A., Ødemark, K., Reitan, T., and Brækkan, R.: Derivation of a new continuous adjustment function for correcting wind-induced loss of solid precipitation: results of a Norwegian field study, Hydrol. Earth Syst. Sci., 19, 951–967, https://doi.org/10.5194/hess-19-951-2015, 2015

Nitu, R., Roulet, Y.-A., Wolff, M., Earle, M., Reverdin, A., Smith, C., Kochendorfer, J., Morin, S., Rasmussen, R., Wong, K., Alastrué, J., Arnold, L., Baker, B., Buisán, S., Collado, J., Colli, M., Collins, B., Gaydos, A., Hannula, H.-R., Hoover, J., Joe, P., Kontu, A., Laine, T., Lanza, L., Lanzinger, E., Lee, G., Lejeune, Y., Leppänen, L., Mekis, E., Panel, J.-M., Poikonen, A., Ryu, S., Sabatini, F., Theriault, J., Yang, D., Genthon, C., van den Heuvel, F., Hirasawa, N., Konishi, H., Motoyoshi, H., Nakai, S., Nishimura, K., Senese, A., and Yamashita, K.: WMO Solid Precipitation Intercomparison Experiment (SPICE) (2012 - 2015), Tech. rep., WMO, Geneva, 2018.