Dear Editor,

We have now revised the manuscript, with some of the more substantial changes being:

- Data example figures (previously 9 and 10) have been promoted to earlier in the manuscript in Section 3 (3 and 4)
- Some of the details given in Section 2 have been relocated to more fitting subsections in Section 3
- Section 6 “Data Availability" has been moved to the end of the manuscript (Section 8)
- Section 1 “Introduction” has been extensively reworked.
- The two appendices have been combined into a single appendix

Please find our responses to the specific comments of Referees given below in blue text, first for the comments of referee #1, followed by #2.

With best regards, on behalf of all authors

Andrew W. Seidl

Referee #1 Review of Seidl et al. “The ISLAS2020 field campaign: Studying the near-surface exchange process of stable water isotopes during the arctic wintertime” submitted to ESSD (ESSD-2024-293)

This paper describes an extensive effort of water sample collection for isotope analysis in the Arctic during an exceptionally cold period in late winter 2020 with 11 near-surface profiles over a snow covered and an open ocean surface in Ny Ålesund as well as many precipitation samples from different location in Svalbard and along the coast on the Norwegian mainland. To me the near-surface profile data is truly innovative and interesting and the whole dataset provides exciting new insights into moisture cycling in the Arctic. I find the paper well written and very well documented. In some instances, the descriptions are a bit too detailed and lengthy, but in general I found the paper to be well-balanced. I would suggest providing an earlier overview figure of the sampled data together with the meteorological documentation at the beginning of the paper. Otherwise, the data and methods section is very challenging to read. But this can be easily solved by placing Fig.9 and 10 much earlier and relating them to Fig. 2 with the meteorological overview. I also recommend some more in-depth discussion of the isotope observations in the profiles to

provide some physical consistency checks since the deuterium excess observations are indeed far from the normal range of expected measurements. Below you find some more detailed comments.

In the revised manuscript, Figures 9 and 10 have been moved to Figures 3 and 4 to act as a reference point for the reader through earlier sections. We have also included more references to existing literature to better familiarize the reader with some of the processes that can impact the d-excess. We refrain from in-depth discussion of the observations themselves as this is a paper for ESSD, and the results are interpreted in a forthcoming manuscript. Please find our responses to the minor comments below.

Minor comments:

- 6: after “primary” one expects to find a “secondary”, but was the precipitation sampling campaign really a secondary component of the field experiment? Or just at a larger scale?

We have removed the phrase “primary field experiment”, instead opting for the more descriptive “Ny-Ålesund field experiment”. However, when describing the campaign network, we do describe this as “main location” or “localised experiment” to better relate the concentrated focus of the field experiment.

- 12: meteorological station data?

Changed to “ambient conditions from nearby meteorological stations”.

- 36: put Pfahl et al. behind regional models and Brady et al behind earth system models.

These references have been moved accordingly.

- 39-40: “as well as the conservation of the isotopic imprint during further airmass transformation over open waters” what do you mean exactly by this? At different stages of the airmass transformation process? Or why “conservation”? Do you expect the isotopic imprint to be conserved during airmass transformation over open waters? I think this is a bit unclearly formulated. Also, to me it seems not so clear how you want to address this question. I really like the profiling aspect, but it’s definitely very local and I also like the distributed precipitation sampling, but to me it’s not so clear how you want to link them.

Indeed, the word "conservation" was unclear/misleading in this context. These lines now read “However, high-resolution in-situ measurements of water isotope composition during

both condensation and evaporation in Arctic conditions remain severely lacking. Furthermore, coordinated measurement efforts enabling analysis of the isotopic imprint of airmass transformation as it is transported southward from the High Arctic to mainland Norway over the open waters of the Norwegian Sea are scarce."

- 40: I would not write "validate" but evaluate. Otherwise, you start with a biased way of looking at the model assuming it does well in simulating the processes you are interested in.

This sentence now states: "Such observational data are highly needed to test and evaluate different phase change processes in isotope-enabled models."

- 44: d-xs is a strange notation, I would either opt for d or dexc, or dxs but the "-" is confusing. After introducing it, the abbreviation should be used consistently (e.g. at L. 47 you write d-excess).

All occurrences of "d-xs" notation have been removed and replaced with d-excess

- 45-46: You use first non-equilibrium fractionation and then kinetic, that's a bit confusing. Very likely the negative deuterium excess is also due to non-equilibrium fractionation but due to water vapour deposition in supersaturated conditions with respect to surface temperatures (Thurnherr and Aemisegger, 2022). You even write it yourself later at L. 54 ("freeze dried air masses"). This should be made clearer already here.

This passage has been reworded to more clearly differentiate between equilibrium and non-equilibrium fractionation, while also explaining that the d-excess quantifies kinetic fractionation.

- L 53: Here maybe a reference to AC3 and Kirbus et al. 2024 ACP would be good.

We have now included this reference in Section 1

Kirbus, B., Schirmacher, I., Klingebiel, M., Schäfer, M., Ehrlich, A., Slättberg, N., Lucke, J., Moser, M., Müller, H., and Wendisch, M.: Thermodynamic and cloud evolution in a cold-air outbreak during HALO-(AC)^3: quasi-Lagrangian observations compared to the ERA5 and CARRA reanalyses, Atmos. Chem. Phys., 24, 3883–3904, https://doi.org/10.5194/acp-24-3883-2024, 2024.

- L45: "The HDO molecules are more likely to evaporate than the $H_2{}^{18}O$ molecules, a process known as kinetic isotope fractionation, which results in a positive d-excess

signature" That's not exactly true. HDO molecules are less likely to evaporate than $H_2{}^{18}O$ molecules. HDO has a lower saturation vapour pressure, and this effect dominates both the $d^{18}O$ and $d^{2}H$ signals. In equilibrium the $d^{18}O$ and $d^{2}H$ are related by a factor of about 1 to 8. It's just that there is an excess of HDO in the vapour compared to the equilibrium case during subsaturated conditions.

Along with the above point made regarding L 45-46, we have removed phrases such as "more likely" instead presenting the fact that the ratio of HDO to $H_2{}^{18}O$ is larger under non-equilibrium conditions than compared to equilibrium conditions.

- 42: I am not sure I can follow here: I don't know of any theory that would have the ambition to predict the Arctic dexcess surface flux as such. Do you mean that we have an idea about the range of expected dexcess in the evaporation flux for a given range of temperatures and near-surface humidity gradients over the ocean? But there is still ongoing important controversy about the influence of snow metamorphism and what the isotope composition of the net snow sublimation flux is (see Wahl et al. 2024 TC).

We have now more clearly differentiated that the theory previously mentioned referred to determining the flux during evaporation and have now included a short sentence with some of the suggested existing literature that points to snow-air exchange, as also fits with the point below regarding L 42-50.

- 42-50: Here I miss a statement that clearly summarises the literature already available on relevant processes. It has been shown in several recent studies (e.g. Thurnherr and Aemisegger, 2022, Brunello et al. 2024 GRL) that snow/ocean-air exchange is the key process that impacts dexcess in the mid- to high latitude marine boundary layer. The large-scale drivers of this dexcess variability has been linked to warm vs. cold air advection over the mid- to high latitude oceans. What is challenging in the Arctic and around Svalbard is that there are very large inhomogeneities in the surface conditions and both air-sea and air-snow interactions matter. In addition, not only airmass transport and temperature advection play a role such as within the core of the storm track regions, but also prolonged longwave cooling during stable anticyclonic conditions. During these conditions air-snow exchange can be enhanced due to snow metamorphism either within the surface snow (Casado et al. 2021 GRL) or during transport of blowing and drifting snow (Wahl et al. 2024, TC). So, I would say that there are already several literature building blocks available that provide the physical basis for establishing a more nuanced view for reading the Arctic dexcess signals. I suggest merging the paragraphs at L. 42 and L. 51 and discussing the large-

scale drivers and physical processes directly with the associated known dexcess signals (near-surface vapour dexcess during upward vs. downward net fluxes, role of snow metamorphism).

These two paragraphs have been reworked to now include more of the available literature in order to give the reader a better overview of studies working to study the relevant processes, as well as studies that observe the complicated pattern of stable water isotope compositions in the Arctic.

- 59: Here the reader needs to know why this reconciliation between lab and field studies is necessary and what it entails. What do lab and field studies not agree upon?

We have reworked this paragraph to better highlight the gaps between lab and field studies.

- 76: the the

This has been corrected

- Section 2.1: the deployment times don't become clear from this section. How close in time where the three sites visited? Does the free tropospheric site really give a representative observation of the weather situation in which the observations at the two profiling sites were done? Also I didn't find a figure showing these observations neither a discussion of how they could related to the observations at the profiling sites.

Section 2 has been shortened and generalized to summarize the campaign locations, give the climatology of the experimental site in Ny-Ålesund, and present actual weather conditions encountered. Specific deployment site details have been moved to the relevant subsections of Section 3. Subsequently, overview Figures (9 & 10) have not been included in Section 2 but instead put into section 3.

- Section 2.2: it would help to have the abbreviations of the sampling locations of Fig. 2 in the text as well. Furthermore, a timeline with an overview of the sampling periods for the samples taken at the different locations and sublocations would greatly help to get an overview of how much precipitation was sampled where and over which accumulation period. Figure 9 and 10 should be placed here and not in the results section.

These abbreviations have been given while presenting specific location in Section 3.5. Figures 9 and 10 (now 3 and 4) have moved to the start of Section 3, which has been renamed as “Campaign activities”.

- 149: A reference to the climatological work on Fram Strait CAOs and their preconditioning would be helpful here: Papritz et al. 2019

We have decided to include this reference in Section 2

Papritz, L., Rouges, E., Aemisegger, F., & Wernli, H. (2019). On the thermodynamic preconditioning of Arctic air masses and the role of tropopause polar vortices for cold air outbreaks from Fram Strait. *JGR*, 124, 11033–11050. https://doi.org/10.1029/2019JD030570

- 155: the COAi has units of K or °C I assume.

This has been corrected.

- 158: the periods of profiling and where the profiles were done should also be listed clearly in Fig.2: I suggest splitting Fig. 2 into two: one figure covering the timeline in terms of sampling and meteo (as is) and another figure with a synoptic overview (where maybe 2 additional timesteps could be chosen, which are more representative for the profile sampling).

Figure 2 (synoptic timeline) is now immediately followed by the overview Figures (previously 9 and 10), which should make connections between weather conditions and deployment sites easier for the reader. We decided to keep Fig. 2 in one to show the connection between sampling time and synoptic overview.

- 176: nearby the meteorological station

This sentence has been changed to “The instrument setup at Zeppelin involved only a CRDS analyzer measuring near to the meteorological station of the observatory.”

- Figure 3 and other locations: could the times be indicated in UTC or is there a reason for doing otherwise?

All times have been given as “UTC”.

- 193-201: This is a very detailed paragraph on the FODS the data of which is published elsewhere, is this really necessary in the text? The deployment dates are relevant for the reader of this paper though.

This paragraph has been shortened accordingly.

- 210: **the** isotopic evolution of the snow

This has been corrected.

- Table 2: times of sampling are missing

We have decided to keep specific times omitted, as the days covered are intended as a general overview (e.g. we sampled Fjord every day, even when not profiling, but only surface snow while at the Snow site).

- Table 5: what does this imply for the response time of the instrument?

Unfortunately, characterizing the total inlet response during ZEP1 and ZEP3 was not possible, due to the inaccessibility of the main sampling inlet on the observatory mast. We can only speculate that the response time is dominated by the flowrate through the ~21 m of the observatory inlet manifold. And we consider the response time during ZEP2 to be comparable to the response time at the profiling sites.

- 350: "for a sample at -10 ‰d18O and -100 ‰ is thereby estimated as 0.44 ‰and 1.5 ‰, respectively" space between ‰d18O and missing dD indication.

Thank you, these typos have been corrected.

- 374: at this time resolution the response time of the whole system is a key missing information.

We have added a reference to Seidl et al. (2023), which describes how the time resolution of the profiling system was calculated.

- 408: double 10 s information

This has been corrected.

- Section 5.1 I think the profiles are exciting and THE big innovation of this paper: I would find it very valuable to provide the standard profiling plots for all the sampled profiles at the two sites in the supplement. Furthermore, the specific humidity is missing in the profiles, as well as the relative humidity with respect to the surface temperature (key variable to understand air-surface water isotope fluxes) and I would also be curious to see the d18O.

Since this is an ESSD paper, we chose to only briefly exemplify the use of the dataset here. More detailed investigation of the profile data will be presented in a forthcoming

manuscript. However, the Python code for generating data example figures has been included in the Supplemental material, and this has been stated in the text.

- Section 5.1: here the information on the snow and ocean isotope composition would be very important to have together with the profiles. That's why they are useful, namely in combination with the surface profile observations.

We have provided the isotopic values of the underlying surfaces during the times covered in Figures 6 and 7 in the text of Section 5.1.

- 450-460: I think for quality check reasons, the low deuterium excess data in air found here should be discussed in terms of their physical plausibility. Clearly the temperature profiles shown for the snow site indicate very strong heat and moisture deposition fluxes to the surface. There is some literature available on this kind of phenomena and their impact on the isotope signature of water vapour in polar regions:

Negative values of the deuterium excess were also found in other studies e.g. during Mosaic and ACE and were associated with warm advection:

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

And in the Southern Ocean:

Thurnherr, I. and Aemisegger, F.: 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, Atmos. Chem. Phys., 22, 10353–10373, https://doi.org/10.5194/acp-22-10353-2022, 2022.

In their Fig. 2 Thurnherr and Aemisegger, 2022 illustrate and explain the large amplitude change in dvapour compared to d18Ovapour during the process of water vapour deposition to the surface, although over the ocean.

Other studies have discussed negative deuterium excess signals in polar regions as potentially due to sublimation:

Hu, J., Yan, Y., Yeung, L. Y., & Dee, S. G. (2022). Sublimation origin of negative deuterium excess observed in snow and ice samples from McMurdo Dry Valleys and Allan Hills Blue

Ice Areas, East Antarctica. *Journal of Geophysical Research: Atmospheres*, 127, e2021JD035950.

> Wahl et al. 2024 hint towards the fact that fractionation due to air-snow interactions is likely not due to the sublimation part of the flux but to the depositional part of snow metamorphism (ongoing transformation of the physical structure of the snow important in particular in environments with a strong vertical temperature gradient), and show some evidence for the fact that the dexcess is lowered due to this process during a controlled wind tunnel blowing snow experiment.
>
> Interestingly this profile was sampled during a CAO period over Fram strait. But the profiles with stable stratification for most of the time show that locally the site is influenced by a mesoscale wind system apparently advecting relatively warmer subsiding air over the snow site. I think these aspects should be highlighted because they matter for the credibility of the observations presented, which do deviate somewhat from the normal observational range for the dexcess.

Thank you for this detailed and well-presented point. At the end of the paragraph describing the Snow profiling site (~L.461), we have now included a paragraph that summarizes this information, and points to our forthcoming analysis:

"These low d-excess values may initially appear surprising and thus questionable in terms of data quality, however mass balance dictates that kinetic fractionation creates both positive and negative d-excess values as HDO molecules are removed preferentially from a reservoir, such as during condensation flux from the atmosphere to a cold surface. Negative or low d-excess values have, for example, been reported in water vapour measurements in the deep Arctic (Brunello et al., 2024), in the Iceland-Greenland Sea (Sodemann et al., 2024), and over the southern Ocean (Thurnherr and Aemisegger, 2022). Other studies have interpreted negative d-excess values as potentially being caused by sublimation effects (Hu et al., 2022). Wind-tunnel experiments with blowing snow have indicated that deposition onto blowing snow could lower the d-excess of the water vapour (Wahl et al., 2024). The d-excess measurements shown here have been made in an environment close to open water, where it is plausible that air masses advected over nearby cold surfaces can induce deposition fluxes to the

surface that lower the d-excess in atmospheric water vapour. A forthcoming study will investigate this hypothesis in more detail."

- 462-476: Very nice illustration of a situation in which the closure assumption (i.e. that the water vapour isotope signal is the same as the isotope signal of the flux) is far from being satisfied. This could be mentioned here.

We consider including such advanced interpretation beyond the scope of the journal. But we thank you for providing us with this very interesting thought!

- Section 5.1: It is interesting to note that slight variations in the wind direction lead to substantial changes in the vertical temperature structure going from moist plumes over the ocean and subsiding air pockets likely leading to enhanced vapour deposition to more well mixed conditions over the whole column. Therefore, from what I see the temporal variability at one location is at least as large as the vertical variability sampled with the profiling arm. So individual eddies really dominate the temporal variations and the measured isotope signals at different levels. The profiling system is not fast enough to give insights into the vertical structure of one single dynamical feature of vertical transport. This aspect ought to be actively mentioned and discussed.

While the rapid temperature fluctuations contribute to the noisiness of the signal-as illustrated by the variability scale added to the profiling example figures-we conclude that the overall gradient is resolved, since we sampled many heights and analyzed the averages over longer times than the lifespan of individual eddies. But we have also generally thought on how the high-resolution temperature profiles can be used to interpret our isotopic timeseries at a higher frequency; its potential is very exciting. We've added a sentence in Section 6 (Discussion) that states that the time of acquiring a profile does not allow to resolve individual eddies.

- Section 5.1: what does a wind direction change from 180° to 250° at the snow site imply for the air mass origin. How comes the vertical column is so differently stratified during these wind direction changes? Is there wind shielding or turbulence induced by the local infrastructure or by people?

In Section 2.1, we discuss the prevailing wind directions and their sources. Figure 1c also shows the topography associated with those directions relative to our profiling sites. There was no localized flow disturbance upstream. Additionally, changes in wind direction are

also associated with changes in wind speed, which is the most likely cause for the starkly different stratifications in the FODS column.

- [Figures] 6-8: error bars would be very helpful.

We have included a representative scale for the variability in the isotopic measurements in Figures 6 and 7.

- Section 5.2: given the results from the previous section surveying conditions at the moisture source and then presenting the precipitation isotopes: I think a short discussion on the importance of the transformation of the signal underway due to fractionation, in particular, related to cloud processing would be helpful to tie the paper together and provide a more coherent storyline to the reader.

We have promoted Section 7 to Section 6, immediately following 5.2, including a short word on synergy between our local and regional datasets.

- 530: post-depositional modification of the isotope signals has been discussed to be due to snow metamorphism and quantified in several recent studies (e.g. Casado et al. 2021, Aemisegger et al. 2022, Wahl et al. 2024)

Thank you for suggesting these works. We have now included references to them in the manuscript.

Casado, M., Landais, A., Picard, G., Arnaud, L., Dreossi, G., Stenni, B., & Prié, F. (2021). Water isotopic signature of surface snow metamorphism in Antarctica. *Geophysical Research Letters*, 48, e2021GL093382. https://doi.org/10.1029/2021GL093382

Aemisegger, F., Trachsel, J., Sadowski, Y., Eichler, A., Lehning, M., Avak, S., & Schneebeli, M. (2022). Fingerprints of frontal passages and post-depositional effects in the stable water isotope signal of seasonal Alpine snow. *Journal of Geophysical Research: Atmospheres*, 127, e2022JD037469. https://doi.org/10.1029/2022JD037469

- Good idea, one February 2020 event is described in Brunello et al. 2024.

We have decided to include this suggested reference in relation to the Warm Air Intrusions mentioned on L 546. Though unfortunately the timing of the cases put forward in that work are just beyond the timeline of ISLAS20220, a similar analysis as that put forward in this work could potentially be applied to the WAI observed from 3-8 March 2020.

Referee #2 Review of Seidl et al. "The ISLAS2020 field campaign: Studying the near-surface exchange process of stable water isotopes during the arctic wintertime" submitted to ESSD (ESSD-2024-293)

General comments:

This paper presents water isotope data collected during the ISLAS202 campaign. These stable isotope data include multiple different water phases (e.g., liquid, vapor, solid-snow, ice, etc.) from both inland and coastal settings. Overall, the study design and data collection methods are sound and the data seem of high quality, especially given the difficulties of doing this type of work (the water vapor data, in particular) in the High Arctic. The data presented by the authors is likely to be of use to various different disciplines from climate modelers to cryosphere scientists. The vertical profile data are particularly innovative and of interest. With some minor revisions, this manuscript could be acceptable for publication in Earth System Science Data.

Please find our responses to the minor comments below.

More specific comments:

- Lines 48-50: Adding some recent (existing) work showing how different process and locations influence variability in Arctic d-excess would be beneficial. While more Arctic d-excess information (as in this paper) would certainly be helpful, recent work reveals some of this nuance that the authors state as needed and should be included. This would also help place the contributions of this work in better context.

  For example:

Wahl, S., Walter, B., Aemisegger, F., Bianchi, L., & Lehning, M. (2024). Identifying airborne snow metamorphism with stable water isotopes. *The Cryosphere*, *18*(9), 4493-4515.

Indicates how water vapor d-excess can change with varying snow and (Arctic) atmospheric conditions (e.g., temperature, wind, etc.) in a laboratory setting.

Klein, E. S., Baltensperger, A. P., & Welker, J. M. (2024). Complexity of Arctic Ocean water isotope (δ18O, δ2H) spatial and temporal patterns revealed with machine learning. *Elementa: Science of the Anthropocene*, *12*(1).

Reveals nuance and new spatial patterns in Arctic d-excess values.

With this set up, the authors can then more specifically describe their new contributions to understanding d-excess variability (some of which begins at Line 59) and place them in better context. For example, the vertical profiles and quite creative and interesting.

We have rewritten this paragraph and the two following, also in alignment with the suggestions made by Referee #1. We now introduce more background literature to contextualize our measurements and try to emphasize their utility for finding consistency between the findings of previous field and laboratory studies.

- Line 127-128: The authors state that daily samples were taken, entirely of snow. What if there was not any fresh snow? Were samples collected from the existing surface? Was this done in the same spot (after several days of collection, samples would be further down the snow pack and not near the surface)?

This sentence now reads "once a day, given a sufficient amount of fresh snow, that snow was collected and homogenized, with an aliquot taken for analysis."

- Line 132: In this context, please explain high frequency. Once a day? Twice a day?

We have included that higher-frequency sampling was up to every 3 hours.

- Line 188: Is the tubing flow path length the same at 4 cm as 200 cm? Due to logistics, I suspect so, but this would be good to clarify. Were the flow rates the same at all heights?

We have clarified that the tubing length is around 4 m and given the flow rate through the inlet.

- Line 280: Why was the plastic tubing a combination of Bev-A-Line (~4m) and PTFE tubing (~6m)? I don't think this matters for data collection and I understand the challenges of working in the field, but I was just wondering if there was a particular reason for this.

We have now stated that this Bev-A-Line tubing was already existing tubing connected to the analyzer at the station.

- Lines 322-324: It looks like with the secondary standards used for water vapor isotope calibration, DI and GSM1, the most depleted (negative) value is -261 ‰ for δD. However, if I am interpreting this correctly, some of the values are far below

this (e.g., Figure 6 from the snow tundra site has values below -340). Is there a reason a standard with a lower value was not used? Table 6 lists GLW, which has a lower value, but it appears this was not used for vapor? Is there a reason a standard with a lower value was not used for calibration and how might this impact the values (e.g., potentially greater error with more depleted values)?

It is correct that in the field, the most depleted available standard was GSM1, and GLW became only available during the later characterisation of the analyzer. We have included multiple statements in Section 4 detailing that GLW replaced GSM1 as a working standard, and GLW was used for characterising the analyser's isotope-humidity offset.

- Also, this is somewhat subjective, but there are many uses of passive voice, which make the paper longer and more difficult to read. For example, in the first sentence of Section 2, the word "being" can be deleted between "site" and "in".

We have reviewed our use of active and passive voice in the manuscript and have adjusted where we thought such changes would fit best.