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

Dear Editor,

below we respond to the additional comments from Reviewer #3, which we think we could mostly address in a constructive manner. However, we also feel that in two cases, we had deny the requests of the reviewer to expand the manuscript into what we consider data analysis and interpretation. We furthermore changed the geographic term "Lofoten" to the correct regional description "Vesterålen", as Lofoten denotes only the southern branch of the Lofoten-Vesterålen archipelago.

On behalf of all authors,
Harald Sodemann

Reviewer #3:

The authors have responded constructively to the first-round reviews. The additions of Fig. 14, the revised uncertainty table (Table 5), the corrected $\delta^{18}O$ uncertainty for ALOMAR, and the new spatial representativeness discussion in Section 7 are all welcome improvements. However, several issues remain that require attention.

1) The R^2 values reported in Figs. 14c-d are derived from only five data points. For n = 5, R^2 = 0.60 corresponds to a p-value of approximately 0.13, which is not statistically significant at the conventional α = 0.05 level. The authors must report p-values and confidence intervals for both regressions, and soften the conclusion regarding a "local INP source" if the relationship is not significant. Additionally, alternative explanations for the anticorrelation (e.g., air mass origin shifts, wind-driven resuspension) should be briefly acknowledged.

Reply: *We would like to thank the reviewer for this comment and support their intent for rigorous statistical significance testing. While it is true that the p-value for n=5, R2=0.60 is approximately 0.12 at -15°C and thus the null hypothesis (H0: There is no statistically significant relationship between dD and the INP concentration at this temperature; Fig. 14c) cannot be rejected on a 0.05 level, we recommend being rather cautious interpreting these results quantitatively given the low sample size of 5. P-values can be highly unstable at low sample sizes, and this is also the reason why we have chosen to not report any p-values nor statistical significance testing at -17°C, where we in fact would be able to reject the null hypothesis on a α = 0.05-level (n=5, R2=0.88 leading to a p-value of 0.02 at -17°C; Fig. 14d). However, we believe it is more honest to acknowledge the low sample size and not report any quantitative significance levels but instead cautiously conclude that our data "suggests a local source of INPs throughout the warm air intrusion of IOP2" (Line 667). Thus, we focus on the exploratory nature of this analysis and report descriptive statistics instead (scatter plot and R2).*

We would also like to point out that we have already extended our dataset description with this case study in response to the first round of reviewer comments, and we feel no further in-depth analysis needs to be added as in ESSD "any interpretation of data is outside the scope of regular articles" (https://www.earth-system-science-data.net/about/aims_and_scope.html).

However, we do agree with the reviewer that alternative explanations for the anti-correlation might exist. While the wind-driven resuspension of aerosols is consistent with a more local source in INPs, we have added shifts in air mass origin or modification during transport as alternative explanations in the manuscript:

"However, further analysis is needed to investigate the influence of other explanations, such as shifts in air mass origin or the modification during transport for the observed anti-correlation. Nevertheless, these findings exemplify the added value of combined SWI and aerosol INP measurements pertaining to, for example, INP source attribution."

2) The +1.65‰ bias correction applied to site Coast $\delta^{18}O$ (Section 5.1) carries its own statistical uncertainty, the authors should either incorporate this uncertainty into the budget or provide a brief quantitative justification for why it is negligible.

Reply: *We determine the bias from the median difference in $\delta^{18}O$ from site Coast and site ALOMAR. The uncertainty of a median can be estimated from the standard error of the mean, scaled by $\epsilon = \sqrt{\pi/2} \approx 1.2533$. We then use the squared sum of the uncertainty of the medians of the two time series (about 0.04 ‰ for each) to estimate the uncertainty of the bias correction:*

$$\sigma_b = \sqrt{\frac{\epsilon^2 \sigma_A^2}{N_A} + \frac{\epsilon^2 \sigma_C^2}{N_C}} = 0.06\ ‰$$

Here, subscripts A and C denote sites ALOMAR and Coast, $\sigma_{A,C}$ is the respective standard deviation, $N_{A,C}$ is the number of samples at the respective station, and σ_b is the uncertainty of the bias.

When this bias uncertainty σ_b is combined with the total measurement uncertainty using squared sums, the combined uncertainty would change slightly from 0.22 ‰ to 0.23 ‰ for $\delta^{18}O$. In the revised manuscript, we now state the value of the bias including the uncertainty, and note that the uncertainty of the bias estimation only has a very minor impact on the uncertainty budget.

3) Section 5.4 notes a ~20‰ positive d-excess bias in surface snow samples relative to box samples but dismisses the cause as "currently unclear." For a data description paper, this is insufficient. The authors should clarify whether the bias is consistent across all transects, add an explicit cautionary statement for data users, and briefly discuss plausible physical mechanisms.

Reply: *The full quote of this section in the current version of the manuscript reads as follows:*

"*The cause of this positive bias of about 20 ‰ or more in the surface snow samples is currently unclear, and may be related to mixing and exchange processes with the snow pack. Thus, while these transect samples deserve to be investigated more on a case-by-case basis, the fact that inter-event variations of up to 80 ‰ in δD are substantially larger than for the samples from a given transect (2-20 ‰) clearly indicates the possibility to determine the representativeness of Coast precipitation isotope measurements for the 100 km long transect across the Lofoten archipelago.*"

We respectfully disagree that we here dismiss the cause of this difference, and think that these sentences do clearly provide a mentioning of possible physical causes, as the reviewer wants to see in a data paper. The reviewer asks in addition for an investigation to assess to what degree the bias is consistent across all samples. In our understanding, and based on the guidelines provided by ESSD (aims and scope, see also Carlson and Oda, 2018), this is beyond the scope of a ESSD data paper, as it would lead us to venture into exploring processes affecting the data by means of a scientific analysis. We therefore chose to refrain from further extending the analysis here.

4) Fifty-seven radiosonde ascents are included in the PANGAEA dataset, yet neither Table 1 nor Section 2.4 provides any description of the instrument type, measured variables, or vertical resolution. A brief description should be added.

Reply: *We added a description of the variables measured by the radiosondes:*

"*The radiosondes measured wind speed and direction using GPS, as well as air pressure from a silicon capacitor, air temperature from a resistive sensor, and relative humidity from a humicap sensor. All data is reported at a 2 s time interval.*"

The dataset metadata contains details about the sounding system and type of sondes in standard BUFR format used by WMO.

5) The distance to Bergen is stated as "1100 km" in Line 142 but as "1000 km" in line 688 and in the abstract. Please use a consistent value throughout.

Reply: *The exact distance between Bergen and Andenes is 1100 km, and 120 km between Andenes and Tromsø. We now use these numbers throughout the manuscript.*

6) Line 685 refers to "W-shape variations during IOP5" without a specific panel or time reference. Please add a pointer to help readers locate this feature.

Reply: *We have added a reference to Fig. 6, noting the date 24 March 2021 when the W-shape variations were recorded by the measurement network.*

7) The archived PANGAEA data should clearly state whether the +1.65‰ Coast $\delta^{18}O$ bias correction has been pre-applied to the deposited data or must be applied by users. The current manuscript text is ambiguous on this point.

Reply: *We now explicitly state in Sec. 5.1 that the correction has been included with the dataset:*

"*The dataset on the data repository includes this bias correction and does not need to be applied by the data users.*"

The dataset description on the pangea data repository will also be updated to clearly specify that this correction has already been applied to the dataset. Time delays apply when editing dataset descriptions on pangea.

8) Line 411: "obtained by from the squared sum", please delete "by."

Reply: *This has been corrected in the revised manuscript.*

9) Please confirm that the correction to Fig. 13 panel labels (a-e) noted in the response to Reviewer 1 has been fully implemented, including a complete caption description of all five panels.

Reply: *Thanks for spotting that the revised figure had not been picked up when compiling the manuscript, this has now been corrected.*

References:

Carlson, D. and Oda, T.: Editorial: Data publication – ESSD goals, practices and recommendations, Earth Syst. Sci. Data, 10, 2275–2278, https://doi.org/10.5194/essd-10-2275-2018, 2018.