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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Baffin Bay sea ice extent and synoptic moisture transport drive water vapor isotope (δ^{18}O, δD, d-*excess*) variability in coastal northwest Greenland" *by* Pete D. Akers et al.

Anonymous Referee #2

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General comments:
The paper introduces a new valuable data set of water vapor isotopic observations in the coastal high Arctic Greenland. Such a data set in a region where long term monitoring is still relatively scarce would be very useful to the community in particular to the understanding of the processes influencing water isotopic composition. The overall article is well written and clearly presented. The results are presented in detail, together with a precise analysis of the local meteorology and climate. The article presents a clear statistical analysis of the influences of multiple parameters and their interactions on the water vapor isotopic composition at multiple timescales and is therefore potentially beneficial to the fields of research of paleoclimates using water isotopic proxies

or of present day atmospheric moisture cycle in the Arctic region. However, some very important aspects of the calibration procedure of the water vapor isotopic observations on which all the analyses rely are not totally clear and need to be clarified in order to validate this data set. The calibration method description is sometimes a little enigmatic and some aspects should really be clarified as several of the recommendations for long term measurements of vapor isotopes do no seem to be totally respected (Bailey et al. 2015). Technical issues did not allow applying a normal calibration procedure and therefore choices have been made not to apply all the usually recommended corrections. These choices seem to have been made in favor of a larger temporal data coverage, but the data quality is probably affected. The deviations which could be introduced by a low quality calibration are not necessarily sufficient to invalidate the data set analyses, as the ambient air variability is very strong. But at the end, a clear overall estimate of the precision and accuracy of the data set is needed to justify subsequent analyses. If the precision is too low, some of the analyzed variations might not be significant. I believe that these questions must be answered before publishing these data set and analysis.

Specific comments:
L.29: "and past reductions should be similarly preserved in local glacial ice": this is based on the hypothesis that the local precipitation and vapor isotopic composition are similarly affected by the local sea ice cover and that the isotopic signal of precipitation is preserved in ice cores. This hypothesis is better detailed in the "implications" section, which is sufficient. I believe this affirmation is rather strong for an abstract, as this has not been proven, and should be removed here or expression with more caution.

L.38: Casado et al. 2018 is about the Antarctic environment, not the Arctic and should therefore be referred differently.

L. 140-142: What is the elevation of the inlet compared to the ground level? How long is the inlet tubing? Is the inlet tubing heated and if yes, at which temperature? This last information is important as there might be condensation occurring in a non-heated

tube, in particular in very cold environments. Does the setup include any protection at the inlet to prevent snow flakes or rain from entering the system?

L. 144 to 148: Part of this paragraph could be moved to the previous section describing the local climate.

L. 155 to 157: The two liquid water isotopic standard have isotopic values of -2.24 and -29.80 ‰in δ^{18}O, whereas your ambient air measurements have values ranging from around -20 to -50 ‰. If the calibration scale does not encompass the values which are measured, how can you be sure of the validity of this calibrationÂă?

L. 157 to 162: To justify the standard injection duration, which is lower than in many other studies, the stability of the isotopic values over the averaging period of the injection should be verified. A statistical analysis of the stability of δ^{18}O and δD values or a figure showing their evolution over the 10 minutes should be added to the supplementary materials at least.

L. 168 to 169: If I understand yell, you finally estimated the stability of the system over two months and did not apply any correction over the two years. However, you can hardly justify that there is no drift of the instrument sensitivity at time scales longer than a few months with these observations. The instrument sensitivity can also be different each time it is restarted or the monitoring program is launched and a new calibration scale should be calculated. Has the instrument never been restarted over two years, or can you justify its stability? I believe this is a very important point regarding the quality and accuracy of your dataset.

L.177 to 186: If the humidity sensitivity has been estimated based on a experiment performed in July 2019, the dry air system had not yet been installed. If the humidity response function has been estimated based on measurements performed with the DRIERITE as a dry air source, it is highly possible that the remaining moisture in the dry air source is strongly influencing the measurements at low humidity. This can lead to strong deviations at humidity values below a few thousand ppms and could

explain a very large part of the deviations depicted on Figure S1. It would be helpful to have an idea of the background humidity levels when injecting dry air produced via the DRIERITE without any liquid standard injection, in order to evaluate the validity of the humidity sensitivity corrections. If you apply a correction of the humidity sensitivity based on a experiment biased by a low quality air source, you will introduce this bias into your calibrated data set. Furthermore, more details should be given on the procedure to apply the humidity response correction has been applied.

L. 196 to 197: How did you add the CRDS mixing ratio values to the values recorded by the weather station? Did you use these measurements to fill gaps in the weather station records, or did you use any kind of average between the two sensors? Did you apply any calibration of the mixing ratio measured by the CRDS analyzer, or did you compare it with measurements performed by the weather station?

L. 580 – 604: The unique diel cycle observed in March in water isotopes is very similar to the observations of Bonne et al. 2020 in Siberia, which is cited in the previous section. The environment is a little different as there can not be any influence of katabatic winds in the Siberian sector, but this spring diel cycle is also attributed to the sublimation of the snow deposited earlier in winter, which has an isotopic composition different than the spring water vapour isotopic composition. Similarly, they don't see any significant diel cycle in other seasons. I think this would be worth including a short comment relating both situations.

Technical comments:
L. 122: Change "kmh^{-1}" into "km h^{-1}".

L. 139: "The L2130-i + SDM uses cavity ring-down spectroscopy". It does not seem necessary to add "+SDM", as the SDM is the calibration samples injection module, not the spectrometer itself.

L.140: Wouldn't "suited" be more suited than "amenable"?

L. 252: “over 30‰(-49.5‰to -17.5‰) 230‰(-377‰to -142‰), and 55‰(-7.6‰to 47.5‰)”: correct positions of the commas “over 30‰(-49.5‰to -17.5‰), 230‰(-377‰to -142‰) and 55‰(-7.6‰to 47.5‰)”

L. 271: “the effects of the these factors”

L. 307: “predominate” > “predominant”

L. 318: our analysis reveals that a large amount

L. 342: “data‘s” > “data’s”

L. 360-361: winds source have isotopically light vapor / winds source isotopically heavy vapor

L. 367: “on the L2130-i” is not necessary

L. 385: “composition, (Casado et al., 2018; Kopec et al., 2019)” : comma is not needed

L. 469: very warm observations at Thule come during this period

L. 590: “snow grains that have equilibrated”

L. 692: “adding additional”: formulation could be improved

References:

Bailey, A., Noone, D., Berkelhammer, M., Steen-Larsen, H. C., and Sato, P.: The stability and calibration of water vapor isotope ratio measurements during long-term deployments, Atmos. Meas. Tech., 8, 4521–4538, https://doi.org/10.5194/amt-8-4521-2015, 2015.

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