## Review of "Baffin Bay sea ice extent and synoptic moisture transport drive water vapor isotope ( $\delta^{18}$ O, $\delta$ D, d-excess) variability in coastal northwest Greenland " by Pete D. Akers et al.

This paper presents a two-year time series of high time resolution water vapour isotope measurements from Thule in the northern Baffin Bay with the aim to investigate the synoptic drivers of the isotope variability measured in the region of the Greenland high Arctic. Five interacting factors are presented, that are thought to determine the isotope signals' variability at the daily to annual timescales. These factors include mainly local environmental conditions (temperature, marine moisture availability, surface winds, NAO and the contribution of land evaporative sources). The relative contribution of the different factors is thought to change with the seasonal cycle and in the coming years with the interannual variability in the extent of the sea ice. Overall, I found this well-written paper inspiring to read, it presents good quality measurement data, shows carefully compiled figures and several interesting analyses. I found the discussion related to the role of sea ice particularly interesting. I have three major comment on the science as well as a few minor comments listed below.

My major comments are:

- 1) The analysis on the five factors determining the stable water vapour isotope variability measured at Thule at different timescales is very interesting. However, I had difficulties to evaluate the independence of these five factors and also found them to be chosen in a subjective way. The authors give no motivational framework of the basic physical mechanisms that would justify choosing these 5 factors as basic variables that determine isotopic variations. Could the authors provide a more thorough introduction into why they think these five factors are the relevant ones to be studied? Others would be just as relevant such as e.g. the relative humidity with respect to sea surface temperature, cloud condensation temperature or sea surface temperature, which are the traditional variables that are studied as environmental controls of stable water isotope variability. For these traditional variables, physical frameworks exist that explain why they are relevant: e.g. the formation of clouds during moist adiabatic ascent of air parcels (Rayleigh distillation framework, Dansgaard 1964) for cloud condensation temperatures and the Craig and Gordon 1965 ocean evaporation model for SST and the relative humidity with respect to SST.
- 2) To me highlighting the importance of the atmospheric circulation and at the same time underlining the relevance of local environmental conditions is somewhat contradictory. Many previous studies have used trajectory analysis to show the relevance of environmental conditions at the moisture source for the variability of stable water isotope measurements in water vapour (e.g. Pfahl and Wernli 2008; Aemisegger et al. 2014, Aemisegger 2018; Thurnherr et al. 2020). Here the authors say the circulation and the local conditions are key. I would find it useful, if there was a comment on this apparent contradiction in the paper. Or if it is not a contradiction, then to resolve the misunderstanding and explain why the results of this paper are in agreement with these previous studies.

3) My third major comment is a more technical one: the presentation of the calibration and postprocessing framework of the exceptionally long and very valuable Arctic water vapor isotope time series lacks some details in particular on the total uncertainty of the measurements (see also my minor comments 5-11, below).

Minor comments:

- P. 1, L. 1: The sea ice extent seems to come out as the most important factor controlling if moisture is mainly sourced from the local environment or if it is transported from further away. This could be mentioned more clearly in the abstract before the five controlling factors. In my opinion it comes a bit late in the current version.
- 2) P. 2, L. 50: There were many studies investigating the quality of laser spectrometric measurements in the early 2010s, add "e.g." and maybe Sturm and Knohl 2012 and Aemisegger et al. 2012 could be cited as well, since the latter study particularly focused on the capability of laser systems to resolve the synoptic timescale variability of water vapour isotopes.
- 3) P. 2, L.50: The Yale database could be cited here, since it groups most of the already published water vapour isotope data: Wei et al. 2019.
- 4) P. 3, L. 75 "critically giving a second set of observations to derive annual patterns and anomalies" not sure if I understand this correctly. What do the authors mean here?
- 5) P. 5, L. 140: Did the authors test the response times of their system using "Tygon tubing". Several early studies (e.g. Sturm and Knohl 2010; Tremoy et al. 2011; Aemisegger et al. 2012) showed that certain tubing materials induce very large residence times and unwanted strong interactions between the tubing wall and the sample gas.
- 6) P. 5, L. 140: What was the residence time of the sampled air in the tubing, how long was the tubing, was it heated, was the inlet shielded? These are all very important points for performing high quality stable water vapour isotope measurements especially in extreme environments such as in northern Greenland.
- 7) P. 5, L. 151: Introduce the delta notation and the normalisation to the international VSMOW-VSLAP scale.
- 8) P.6, L. 155: The standards' isotopic composition does not bracket the measured isotope signals. The authors should explicitly mention this and comment on the expected impact of this extrapolation on the total uncertainty of their measurements.
- 9) P. 6, L. 175: Even though the drift of the Picarro laser spectrometers is limited regular calibrations should be carried out to 1) survey the good functioning of the system and 2) to provide a long term assessment of the total uncertainty of the measurements (see, Aemisegger et al. 2012; Thurnherr et al. 2020). In particular, Thurnherr et al. 2020 shows that different post processing procedures lead to substantial changes in the isotope data, in particular, with respect to the treatment of the water vapour mixing ratio dependent isotope bias correction.
- 10) P. 6, L. 181: The precision (Allan variance, or standard deviation of a constant water vapour isotope signal) strongly depends on the water vapour mixing ratio (see Aemisegger et al. 2012 and Sodemann et al. 2017). Please indicate the total uncertainty of the measurements as a function of water vapour mixing ratio. This is very important, given the very low levels of humidity observed at Thule in winter.

- 11) P. 7, L. 195: Was the water vapour mixing ratio of the L2130 calibrated using a dew point generator or another humidity sensor installed in parallel? Without calibration the reading of the laser spectrometric volume mixing ratio may be biased.
- 12) P. 8, L. 225: Please indicated the horizontal and vertical grid resolution of the MERRA-2 reanalysis data. Note that HYSPLIT is not a model but a post-processing tool, thus it cannot be "forced". I would suggest to write: "... with air parcel backtrajectories calculated based on three-dimensional MERRA-2 wind fields...". I am not convinced that choosing only 10 days per months produces a robust two-year climatology. But given the limited use that is made of the trajectory climatology in this paper, the approach is ok.
- 13) P. 9, L. 254: "The magnitude of irregular hourly to weekly variations" do you mean synoptic timescale variations?
- 14) P. 10, L. 291: What is meant by "temperature-driven equilibrium fractionation"? I think the cited literature is a bit misleading. Dütsch et al. 2017 shows that the condensation temperature indeed has a certain impact on the deuterium excess. Pfahl and Sodemann et al. 2014 discuss the effect of the SST.
- 15) P. 10, L. 293: Interesting that a negative correlation between the deuterium excess and the temperature is found! Did the authors also look at the correlation with nearby SSTs? In climate reconstructions based on ice cores a positive relation between the deuterium excess and moisture source SST is assumed (e.g. Johnsen et al. 1989; Vimeux et al. 1999; Stenni et al. 2001). However, in a detailed analysis of the correlation behaviour between the deuterium excess and SST a recent study (Aemisegger and Sjolte 2018) found different regions (in particular at high latitudes) that are expected to exhibit a negative correlation based on the Craig Gordon model and the closure assumption. It is thought that this negative correlation arises from a positive feedback mechanism between the SST and the relative humidity with respect to SST. Such a negative correlation regime is expected to be dominant particularly in regions where the variability in air-sea interactions is mainly driven by variability in atmospheric circulation and not primarily by variations in ocean circulation. If the deuterium excess also shows a negative correlation to the nearby SST at Thule this would be evidence for such a behaviour. Of course, the time series at Thule is too short to look at this in detail. But still, I find the negative correlation between air temperature and deuterium excess that is found here very interesting and since it is of opposite sign with respect to the traditional interpretation of the deuterium excess in ice core studies, I would find it worthwhile to shortly discuss this.
- 16) P. 11, L. 320: The relative humidity is very low above forming ice? Can the authors show some evidence or cite some literature?
- 17) P. 11, L. 323: Phase changes in the NAO might be confusing in the context of isotopes and water phase changes. Is there another way to formulate the change in the NAO sign?
- 18) P. 12, L341-L356: Very interesting discussion on the role of the NAO and the sea ice extent!
- 19) P. 14, L. 394: I think the relations to the "traditional driving variables" SST and relative humidity with respect to the SST should at least be shortly mentioned.
- 20) P. 14, L. 396 and 399: "southerly" flow instead of "southern" flow.

- 21) P. 15, Section 6.3 How large is the influence of more distant land sources of e.g. northern Canada? From Fig. 3 one might think that they may play a significant role.
- 22) P. 18, Section 7: Additionally, I suspect that Arctic anticyclones would play a key role in the synoptic timescale variability at Thule (in Greenland blocking situations). A case study of an Arctic Blocking event (over northern Russia) is shown in Schneider et al. 2019, which highlights the very large horizontal gradients resulting from the subsidence induced drying at the core of the anticyclone and the progressive atmospheric moisture uptake along the anticyclone edges.
- 23) P. 18, L. 542: I doubt that in the case of sea ice much water vapour at Thule originates from the deep tropics or subtropics. The analysis shown in Fig. 3 does not support such a statement. Here I think one can safely write "advection from the midlatitudes".
- 24) P. 21, L. 609: I don't understand why an input of low  $\delta^{18}$ O/high dxs from evaposublimation results in a  $\delta^{18}$ O maximum and dxs minimum at midday.
- 25) P. 21, L. 627: It would be great to clearly mention that evaporation of meltwater and sublimation of snow might not carry the same isotopic composition (see e.g. Christner et al. 2017).
- 26) P. 22, L. 664: Maybe not single extreme events but years with high frequency of occurrence of warm advection events, e.g. due to a northward shift of the storm track?
- 27) P. 23: In the conclusion it would be very nice to mention the great use of this dataset for the validation of high-resolution isotope-enabled simulations in the Arctic to study the importance of air-ice and air-sea interaction processes in more detail. Here the great value of the data is their long temporal coverage, thanks to which model-based sensitivity experiments could be performed to test the importance of different driving factors (similar to the climatological sensitivity study over Europe by Christner et al. 2018).

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