Munch and Laepple short comment:

Dear Authors,

Let us first congratulate you on this interesting and elaborate study, combining lab experiments, modelling, and field studies, to advance our knowledge concerning one of the pressing questions in current ice core research which is the role of sublimation for the isotopic signal formation in polar snow and firn.

In the following we would like to comment on two aspects of your study: the use of the mass balance model and the interpretation of the field study data.

The lab experiment quite convincingly shows how under these controlled conditions sublimation goes along with isotopic fractionation in the surface snow and how this change propagates with depth due to diffusion. You compare your results to the output of a simple model which you drive with the observed isotopic composition of the vapour from your lab experiment together with a mass balance equation. Given this model setup, it is however not surprising that the model qualitatively reproduces the isotopic change in the surface snow, since you directly feed the model with the observational data that inherently includes already the fractionation effect from the sublimation. In our opinion it would be more convincing to use some fractionation model and the mass turnover to dynamically model both the evolution of the snow and the vapour isotopic composition.

One of the advantages of the laboratory experiment is the compliance with the closure assumption. We therefore argue that it is reasonable to use the measurements directly rather than some kind of fractionation model that might or might not be tunable to fit these observations. As you point out in your first sentence of the above paragraph, the observations of the enrichment of the snow under pure sublimation conditions serve the purpose of clearly demonstrating fractionation during sublimation and consequential enrichment in the snow. Albeit it would be easiest if traditional fractionation (i.e. Craig-Gordon) models could reproduce the experimental results, we find it not surprising that established evaporation models are unable to reproduce our observations, as evaporation from a water surface and sublimation from a porous snow surface show quite different characteristics.

In a further step of your study, you compare the hourly evolution of atmospheric vapour isotopic composition at EGRIP to isotopic data from surface snow samples and conclude that the atmospheric vapour isotopic composition drives the surface snow isotopes on these time scales. Here we are a bit puzzled about the assumed causal relationship, especially concerning the results from the lab experiment. In the lab experiment, sublimation "creates" vapour δ18O from initially dry air. In nature, i.e. for the field data, you seem to assume it the other way around? Can you comment on this? If so, why do you not use the measured atmospheric δ18O and sublimation rate to drive a model of surface snow δ18O (+ diffusion) and compare this to the observations? This would be a real test of your hypothesis. If we understand your simple mass balance model (your Equation C2) correctly, to do so one could model the temporal evolution of R_S (surface snow) given the measured time series of R_E (atmospheric vapour) and latent heat flux LHF. The latter would provide the sublimation rate and thus the mass change per unit time of the surface snow.

We do find that the atmospheric vapor and surface snow isotopic composition co-vary, but we agree that we cannot determine a causal relationship at this time. As we state in L337 of the

discussion, "At this stage it is unclear whether LHF, vapor d18O, or another factor is influencing the snow surface, or whether the snow surface composition is driving vapor d18O."

However, our laboratory experiments do demonstrate that the snow surface influences the atmospheric isotopic composition during sublimation, and so we have now included a model run with field observation data as input for a very short time period in which sublimation is occurring. As the model is only designed to simulate the sublimation process, we are constrained to periods of continuous positive latent heat flux. Additionally, the field vapor isotopic composition that we use to drive the model is not only created solely from the snow but also subject to synoptic meteorological conditions. It would be interesting to use a C&G type model for the field experimental conditions but we argue that this would go beyond the intended scope of this paper. A larger data set will be necessary for this.

The following is implemented in the paper in L312 of the discussion:

"Figure X [shown here below] shows the mass balance model output for 6:00-18:00 on July 25. The starting snow isotopic composition is the same as the starting composition of the FB4 samples, and model input includes vapor measurements for d18O, d-excess, and humidity, as well as latent heat flux measurements and a linear estimate of temperature (as temperature data is not available for this period). We find that the surface d18O value increases by approximately 0.6 per mil, and the d-excess value decreases by approximately 1.3 per mil. The change propagates only 0.5 cm below the snow surface.

In comparison, the surface (0-0.5 cm) FB4 sample during this time period increases by approximately 1 per mil, and d-excess decreases by approximately 4 per mil (see Fig. A13). There is also a greater change in the sample from 0.5-1 cm, which increases by approximately 0.7 per mil in d18O, and decreases by approximately 2.5 per mil in d-excess in FB4 samples. The larger change observed in the field samples may be due to several factors, including the possibility that the measured vapor is a combination of sublimated vapor from the samples and vapor sourced from elsewhere. Additionally, wind pumping in the field setting may increase the rate of isotope change in the FB samples. As expected from the lab experiments, we see a similar trend towards enriched snow when running the model with actual field observations."
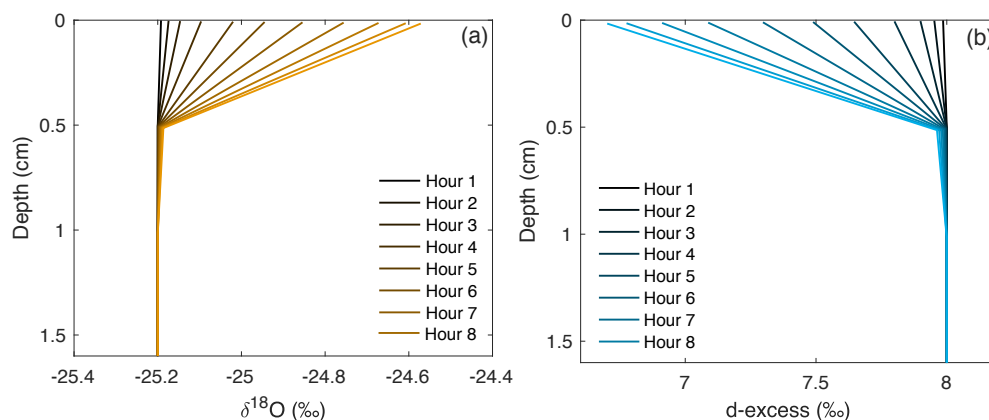
Concerning the field surface samples we would also welcome some more thoughts on the impact of isotopic spatial variability on your results. You already noted and partly accounted for the spatial variability by averaging across three samples. What are the horizontal variations in your data compared to the observed temporal change? One way to show this would be to add standard error estimates (from the 3 replicates) in Figure 5. Depending on the outcome it might

be useful to provide some suggestions on how to improve the field study setup in order to rule out confounding spatial with temporal changes.

The spatial variability of the field snow is one reason why the FB experiments with mixed snow were conducted as an intermediate version between laboratory and field.

We also try to keep spatial variability influences small by taking snow samples directly next to each other (see Fig. A1) and averaging across the three sample sites. In Fig. A12 you can see the spatial variability between the three sites and a clear offset in the mean is noticeable. Nevertheless, the individual sites can all experience a similar temporal evolution.

In future studies we would suggest to either take more samples from various locations to take into account the spatial variability OR exclude spatial variability concerns by digging a large pit (like 20cm x1m x 10m) and fill it with well-mixed snow. That way the starting isotopic composition would be the same for all samples (even in the vertical) but it would represent natural conditions closer than our field box experiments.

Finally, we would like to see some more elaboration in the discussion section of the impact of your results on longer time scales: How can you rule out that sublimation and subsequent deposition not just counteract and cancel each other on longer (seasonal or interannual) time scales? On the other hand, the effect could be relevant on palaeo time scales due to a stronger difference in relevant environmental conditions. In this view, the current comparison of your sub-diurnal change of 3 ‰ δ18O to a seasonal amplitude of ~8 ‰ δ18O (Renland) seems to be too simplistic. As an example, the temperature change in Berlin today between 4 am and 1 pm was 17 K which is the same amplitude as the annual cycle of temperature; still, it is unclear if the day-to-night changes have a significant impact on the summer-to-winter changes.

We have added a speculation of the snow-vapor exchange effect on monthly timescales based on the measured NET sublimation flux in July. This is a very simple mass balance calculation that finds a 5 cm snow layer is enriched by 4‰ in d18O during July. As you point out, whether there is a net sublimation or deposition flux is dependent on the local climatology. We mention this now also in the context of paleo timescales.

You are correct that no direct interpretation can be drawn from the comparison between the sub-diurnal change to seasonal change. However, it gives the reader a feeling for the magnitude of the observed changes that they would otherwise only have if they were very familiar with the topic. To elaborate on this, we have revised the paragraph from L358-370:

To assess the relevance of our results on longer timescales, we make use of the simple mass balance model and an observed mean latent heat flux in July of 3.1 W/m2, indicating a net removal of snow from the surface due to sublimation. By assuming equilibrium fractionation during sublimation (Wahl et al2021) we can calculate the isotopic composition of the humidity flux and the associated removal of isotopologues. When considering reasonable values of a 5 cm layer of snow, a snow density of 300 kg/m3, an initial isotopic composition of -20‰ d18O and a surface temperature of -9 C for the month of July, the snow would be enriched by ~4‰ due to the net humidity flux which is substantial. For comparison, the seasonal amplitude (i.e. summer peak to winter trough) at the Renland Ice Cap, for example, is about 8‰ in d18O (Hughes2020). We acknowledge that this is a highly simplified mass balance calculation without taking into account the vapor isotopic composition or precipitation inputs. However, since vapor exchange is a continuous process it will continuously affect the layer of snow that is in contact

with the atmosphere, and will therefore imprint on the snow isotopic composition with a general net daily sublimation signal during months with a net sublimation flux.

Which months show a net sublimation flux is dependent on the geographical location and general climatology of the area. Especially in the context of paleoclimatological interpretation of ice cores, this cannot be assumed to be constant in time. If the vapor-snow exchange imprints on the seasonal snow isotopic composition as indicated in the result of the mass balance calculation, one would need to take changes in seasonality into account when making assumptions about vapor-exchange effects on paleo timescales, as has been previously demonstrated for precipitation seasonality (Werner et al. 2000).

On shorter timescales in our laboratory experiments we observe changes of up to 8 per mil d18O and 20 per mil d-excess over time periods of several days, and in FS field experiments we find an average change of 2.09 per mil d18O and 3.78 per mil d-excess on very short (sub-diurnal) timescales. This observation, in combination with our mass balance calculation of 4 per mil change in d18O over the month of July, suggests that under typical natural conditions, changes in the surface isotope value occurring on a short timescale may have an impact on the mean seasonally-recorded isotope signal. Previous studies have addressed the effect of seasonally-biased accumulation rate on diffusion and the recorded d18O isotope signal (Persson et al., 2011; Casado et al., 2020; Hughes et al., 2020) and the effect of physical modifications and snow redistribution of the snow surface on the accumulation intermittency (Zuhr et al., 2021), but the effect of sublimation driven changes in surface snow isotopic composition between precipitation events has not been quantified previously. Whether the magnitude of the mean isotope change due to sublimation and snow-vapor exchange outweighs the effects of snow redistribution, accumulation bias, and diffusion has yet to be determined.

Citation:

Werner, M., Mikolajewicz, U., Heimann, M., & Hoffmann, G. (2000). Borehole versus isotope temperatures on Greenland : Seasonality does matter. *Geophys. Res. Lett.*, *27*(5), 723–726.

Wahl, S., Steen-Larsen, H. C., Reuder, J., & Hörhold, M. (2021). Quantifying the Stable Water Isotopologue Exchange between Snow Surface and Lower Atmosphere by Direct Flux Measurements. *Journal of Geophysical Research: Atmospheres*, 1–24. https://doi.org/10.1029/2020jd034400