The Cryosphere Discussions

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Short Comment

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Community comment on "The role of sublimation as a driver of climate signals in the water isotope content of surface snow: laboratory and field experimental results" by Abigail G. Hughes et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-87-CC1, 2021

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.

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 $\delta^{18}O$ 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 $\delta^{18}O$ and sublimation rate to drive a model of surface snow $\delta^{18}O$ (+ 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.

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.

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 ‰ $\delta^{18}O$ to a seasonal amplitude of □8 ‰ $\delta^{18}O$ (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.

Again, thanks for this interesting contribution to a better understanding of the ice core signal.

Kind regards,

Thomas Münch and Thomas Laepple