Response to anonymous referee #1

We thank Reviewer 1 for their time and effort to provide detailed and constructive feedback on the manuscript, which has improved the quality of the study. We have addressed all comments below and propose to implement the changes in a revised version of the manuscript.

Black: reviewer comment
Blue: author's response
Green: revised text (**bold: new text**)

General comments

The effects of post-depositional processes (water vapor exchanges between surface snow and atmosphere, diffusion of water vapor in the firn, and wind redistribution) on water stable isotopic composition in surface snow in the inland of polar ice sheets strongly limit the quantitative paleo-temperature reconstructions by ice-core stable isotopic records from these regions. In this study, the authors attempted to evaluate the mean effect of post-depositional processes at Dome C using a combination of existing and new dataset of precipitation, surface snow and subsurface isotopic compositions over 2017-2021. It is noted that the sampling schemes are elaborate and the sampling temporal resolution is high, which is not easy to perform at very harsh meteorological conditions such as at Dome C but is very necessary to address the problem of post-depositional processes. I appreciate the authors for their hard work at Dome C and their persistent efforts on this complex issue on the post-depositional processes. Although I agree with the authors for their most interpretations of the observations, I think some explanations should be added and the different post-depositional processes should be clarified.

Specific comments

I note that precipitation, surface snow and subsurface snow show similar $\delta^{18}O$ seasonal cycle, but the maximum (minimum) value of $\delta^{18}O$ occurs in different month, with highest $\delta^{18}O$ value in January and lowest value in May for precipitation, a maximum of $\delta^{18}O$ in February and a minimum in October for surface snow, and a maximum of $\delta^{18}O$ in March and a minimum in November for subsurface snow. I think it must be due to the post-depositional processes, but why the post-depositional processes can lead to the $\delta^{18}O$ shift in time?

We agree with the reviewer that there is a shift in time of the monthly mean maximum $\delta^{18}O$ in the precipitation, the snow surface and the snow subsurface. However, we argue that this is not only due to post-depositional processes but also because of the time period that the samples represent or integrate.

For precipitation, if we consider the arithmetic monthly means, the maximum $\delta^{18}O$ occurs in January and the minimum in June (Fig. 6b, l.462-463). This is explained by the fact that the precipitation $\delta^{18}O$ follows nicely the seasonality of the atmospheric temperature (Fig. 6a), which has a monthly mean maximum in December and minimum in June (Genthon et al., 2021, also valid for the 2017-2021 period). Now considering the precipitation-weighted monthly means, the minimum and maximum $\delta^{18}O$ occur in May and January, respectively, which also relates to the seasonality of atmospheric temperature.
Now the snow surface (here defined as the upper first centimetre of the snowpack) integrates several precipitation events, up to 2 months before the sample collection (Fig. S3). This explains why the highest $\delta^{18}O$ in the snow surface is found in February: the snow surface at this time integrates the precipitation events fallen in December and January with the highest $\delta^{18}O$. It is less clear for the minimum $\delta^{18}O$ in the snow surface (October) because it occurs more than 2 months after the minimum in precipitation (May). This could be due for example to snow erosion and redistribution in winter that mixes winter precipitation with already deposited (and more enriched) snow, as discussed l.703-706.
The same integration effect applies to the snow subsurface (1 to 4 cm deep): its maximum in $\delta^{18}O$ is shifted later in the year compared to the snow surface because it integrates the precipitation events fallen onto the ice sheet up to 6 months prior to sample collection (Fig. S3), so that the high $\delta^{18}O$ summer precipitation events are only "recorded" in the snow

subsurface later in the year. The minimum $\delta^{18}O$ in the snow subsurface also occurs about 6 months later than the minimum $\delta^{18}O$ in the precipitation (November and May, respectively).

We explained this integration effect for the vertical difference between the snow surface and subsurface in Section 4.2.1. To make this more clear and include the explanation of the shift in the $\delta^{18}O$ monthly mean maximum, we have improved the subparagraph l.680 forward in the manuscript as follows:

"[...]. The higher $\delta^{18}O$ and lower d-excess in the summer precipitation compared to the winter precipitation (Fig. 6) explains why the mean summer snow surface is enriched in $\delta^{18}O$ and has a lower d-excess than the snow subsurface (Fig. 4). **This integration process can also explain, at first order, why the maximum and minimum monthly mean $\delta^{18}O$ in the precipitation, the snow surface and the snow subsurface do not occur at the same time (January and May for the precipitation-weighted, Fig. 6c; February and October for the snow surface; and March and November for the snow subsurface, Fig. 3b).**"

In section 3.2.2, the authors find that the surface snow is relatively enriched in $\delta^{18}O$ during warm season (from November to April) due to the sublimation of surface snow. Indeed, the calculated surface moisture fluxes also indicate sublimation occurs mainly in summer (Fig. 2). Theoretically, stronger sublimation should cause more enrichment of $\delta^{18}O$ in surface snow. As a result, I would like to know if there exists a correlation between the surface snow $\delta^{18}O$ and the water vapor flux during the summer. Additionally, the slope between δD and $\delta^{18}O$ in surface snow should be lower than the δD-$\delta^{18}O$ slope of precipitation. I suggest the authors to add above relevant information to further validate the sublimation process because it is a key post-depositional process in inland of Antarctica.

We partly agree with the reviewer's comment. In section 3.2.2, we indeed find that the snow surface is relatively enriched in $\delta^{18}O$ from November to April. However, in Section 4.2.1, we discuss that the mean seasonal difference between the two snow layers is most likely explained by the integration effect of the samples (see comment above and l.677 forward in the manuscript), and that it is the inter-annual variability in the difference between the two snow layers that might, on the other hand, be explained by sublimation fluxes (l.666 forward). We therefore only hypothesise on the effects of sublimation on the snow surface.

Nevertheless, we performed a correlation analysis between the snow surface $\delta^{18}O$ and the daily mean water vapor flux during the summer months (November to January) and found that they correlate positively with a coefficient of 0.41 (Spearman's correlation coefficient, $p\text{-value} < 0.001$). We also find a negative correlation between the snow surface d-excess and the daily mean water vapor flux of -0.49 (Spearman's correlation coefficient, $p\text{-value} < 0.001$). We argue that these moderate correlations don't confirm nor rule out the effect of water vapor fluxes, because the snow surface samples cover different time periods than the days of sample collection (and therefore the daily mean water vapor flux). In addition, we expect the snow surface $\delta^{18}O$ to be correlated to the water vapor fluxes due to their individual correlation to the atmospheric temperature (precipitation $\delta^{18}O$ correlated to temperature that contributes to the signal in the snow and water vapor fluxes dependent on the humidity gradient between the surface and the atmosphere, thus also dependent on the atmospheric temperature).
Similarly, because the precipitation and snow surface samples don't cover the same time period, we argue that comparing their $\delta^{18}O$-δD slopes will not necessarily provide additional information on the effect of sublimation. If we look at the precipitation and snow surface samples from December and January (summer months) in the $\delta^{18}O$ vs δD domain, we find that the $\delta^{18}O$-δD slope for the snow surface is higher than the slope for precipitation (see Figure A below). However, we see that most of the snow surface samples are below the precipitation samples (i.e. with a lower d-excess), which indicates sublimation in the summertime.

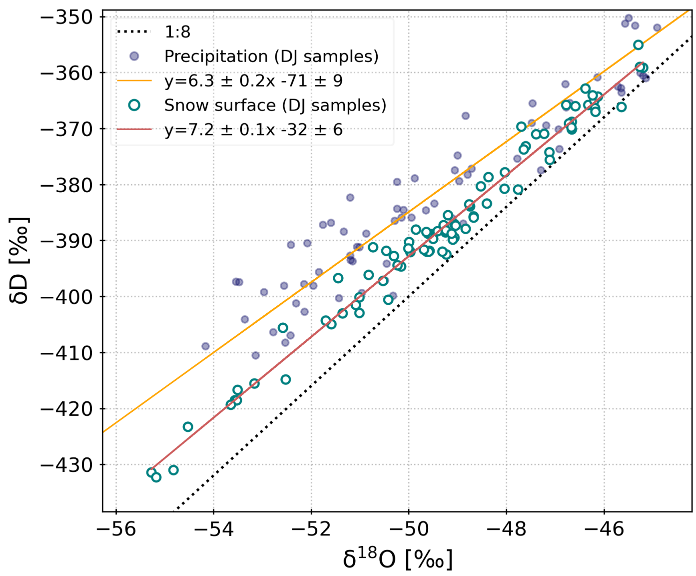

Figure A: Precipitation and snow surface $\delta^{18}O$ versus δD during summertime (December and January). All precipitation samples with a negative d-excess were discarded to exclude any imprint of sublimation on the precipitation isotopic composition (see Section 4.2.2 of manuscript).

We have included additional information on this point in the discussion, improving the text l.666 forward as follows:

"We showed that at Dome C, sublimation occurs during the summertime (Fig. 2a) and that there is a net annual sublimation of snow (Fig. 2b), which means that the surface snow isotopic composition must have been affected by these fluxes. **We find that in the $\delta^{18}O$ vs δD domain, most of the snow surface samples collected in December and January are below the precipitation samples (i.e. with a lower d-excess) collected in the same months (not shown), indicating an effect of sublimation on the snow surface isotopic composition in the summertime. In addition,** we observe some inter-annual variability in the vertical difference between the snow surface and subsurface isotopic compositions (Fig. 4). [...]"

In section 3.3.2, the authors compared precipitation isotopic composition ($\delta^{18}O$ and d-excess) with the simulations of ECHAM6-wiso. Although the daily $\delta^{18}O$ modelled by ECHAM6-wiso shows a good agreement with the observations, the ECHAM6 overestimates (underestimates) the $\delta^{18}O$ (d-excess) likely due to the warm bias of the model. In addition, the daily d-excess is poorly simulated by the ECHAM6-wiso (Fig. 7b). The possible deficiency of the ECHAM6-wiso for the mismatch between the simulations and observations should be brief explained for guiding scientists to better utilize GCMs-wiso.

We acknowledge that we only briefly touch upon the origin of the observed biases between the observations and ECHAM6-wiso (l.760-766) but don't suggest any improvements for the representation of isotopes in the GCM. As also suggested by reviewer 2, we have now included in the manuscript some suggestions for the modelling community (l.763 forward):

"A thorough investigation of the biases in ECHAM6-wiso is beyond the scope of this study, but they might arise from different processes in the model, such as the environmental conditions at the moisture source region, the moisture transport and pathway, the supersaturation parametrization or the condensation height and temperature at Dome C. **The fourteen-year record of the precipitation isotopic composition (Dreossi et al., 2023 and this study) gives the opportunity to evaluate isotope-enabled GCMs and can be used to improve the tuning of the empirical parameterization of supersaturation in polar clouds (e.g. Risi et al., 2013).**"

The authors conclude that the mean effect of post-depositional processes enriches surface snow $\delta^{18}O$ by 3.3‰ to 6.6‰ and lowers the snow surface d-excess by 3.5‰ to 7.6‰. As the authors analyzed, summertime sublimation should contribute significantly to the mean effect. After comparing Fig. 3b and Fig. 6c, I found the $\delta^{18}O$ values of surface snow during wintertime are also higher than the values of precipitation. This suggests that the post-depositional processes rather than sublimation are also significant in winter. So, what kind of post-depositional processes lead to the enrichment of surface snow $\delta^{18}O$ in winter? Is it the diffusion of water vapor? The authors should discuss the post-depositional processes in wintertime.

On the seasonal scale, we are careful to not overinterpret the difference between precipitation and snow surface due to several processes that are difficult to disentangle, specifically sublimation/condensation, interstitial diffusion, wind erosion and mixing and snow surface integration (see comment 1 for the latter). We therefore mainly make a statement on the overall mean difference (5 years) between the precipitation and the snow surface and are cautious to attribute this difference to the effect of one process only.
However, the reviewer's comment is a very interesting point that we briefly discuss in Section 4.2.1 (l.683 forward), but we agree that we miss a discussion on the post-depositional processes in the wintertime. We have improved the text in Section 4.2.1 as follows:

l.687 forward
"Indeed, the isotopic composition of the snowpack is affected by diffusion of water molecules along isotopic and temperature gradients (Johnsen et al., 2000; Gkinis et al., 2014). Implementing this process in the model would possibly reduce the too large amplitude of the seasonal cycles in the synthetic snow layers to match the observations (Fig. 8b and d, 9b and d). **Diffusion might also partly explain the difference observed at the seasonal scale between the precipitation and the snow surface isotopic compositions (Fig. 3b and d, Fig. 6c and f), smoothing the incoming high-amplitude seasonal signal of precipitation and leading to a snow surface enriched in $\delta^{18}O$ (and with lower d-excess) in the wintertime compared to precipitation.** In addition, some of the short-term variations [...]."

l.704 forward
"[...]. This process was proposed by Casado et al. (2018) to explain the slow decrease in the surface snow $\delta^{18}O$ during the winter, a pattern observed in our study (Fig. 3a and b) and that cannot be explained by precipitation only (Fig. 8a and b). **A recent laboratory study also suggests that snow metamorphism during wind transport ("airborne snow metamorphism") has the potential to impact the snow isotopic composition in both $\delta^{18}O$ and d-excess (Wahl et al., 2024). This could be an additional process occurring at Dome C explaining some of the differences between the precipitation and snow isotopic signals, including in the wintertime.**"

References

Risi, C., Landais, A., Winkler, R., and Vimeux, F.: Can we determine what controls the spatio-temporal distribution of d-excess and ^{17}O-excess in precipitation using the LMDZ general circulation model?, Clim. Past, 9, 2173–2193, https://doi.org/10.5194/cp-9-2173-2013, 2013.

Wahl, S., Walter, B., Aemisegger, F., Bianchi, L., and Lehning, M.: Identifying airborne snow metamorphism with stable water isotopes, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2024-745, 2024.