Dear reviewer,

Thank you very much for your insightful comments on our article. We have taken care to revisit all the points you raised and to address them as precisely as possible. Please find our answers below.

Major comments:

1. *The abstract would benefit from further revision to improve the logical flow, as the connections between different parts currently feel somewhat disjointed. In particular, too much emphasis is placed on the objectives and significance of the study, while the description of the results remains relatively brief. Additionally, although some key conclusions are mentioned, several valuable findings—such as the influence of storage processes on ice core isotopes, as well as the potential effects of surface mixing and stratigraphic noise—do not appear to be adequately reflected. A more balanced presentation of results and conclusions would strengthen the abstract.*

Many thanks for these suggestions. We will make sure that these different aspects of the results are listed in the revised version of the abstract.

2. *The two ice cores were processed using different sample segmentation methods (one was directly segmented, while the other was analyzed by CFA continuous flow analysis). Could this difference introduce measurement errors for water stable isotopic profile? How did the authors consider the potential impact of such methodological differences on the model evaluation?*

The two ice cores were actually processed the same way for the top 3 m, i.e. through discrete sampling. We thus do not expect any influence of the analytical methodology for the observed differences between the two ice cores which is largely performed over this depth range. Then, for deeper sections on which the ICORDA core has been analysed through CFA and the Subglacior through discrete measurements, we refer to the comparison between CFA and discrete measurements described in Petteni et al. (2025). Such a comparison was performed on the Paleo core, drilled on a site with similar surface characteristics (temperature, accumulation) than the LDC site. In the Petteni et al. (2025) study, a good agreement is shown between the discrete and CFA measurements both on high frequency variability and amplitude of the isotopic record. We will provide these explanations in the revised manuscript.

3. *The description of the VFC model lacks clarity regarding how the different post-depositional processes are specifically coupled—for example, whether these processes are linked through a particular parameter. The description remains too general to allow a thorough evaluation of the model's operational mechanism, which in turn compromises the completeness and reproducibility of the methodological workflow. This is especially important given that the authors claim to have improved upon the original VFC model by incorporating surface mixing and the stratigraphic noise. Could the authors provide a more detailed explanation in this section, or perhaps include a detailed explanation in the appendix to ensure completeness and reproducibility?*

Thank you for this question. Our study is not a process study and we do not point to a specific mechanism for explaining the mixing in the top layer. We simply invoke snow reworking and/or convective mixing of the water vapor in the top of the firn. The surface wind may be a key driver for the mixing processes but to parameterize its effect, we would need to perform the study on different sites characterized by different wind speed which is beyond the scope of our study. We thus stick to the suggestion of Ollivier et al. (in rev) that a mixing effect occurs on a few centimetres at the surface of the firn in the neighbouring Dome C site and propose to include surface mixing in a very simple way, i.e. averaging the isotopic values over a few centimetres at the surface. The next step would be to study if sites with higher surface wind speed have smoother variations in the water isotopic records to further test the process of mixing through surface wind.

L174 – 177, We will improve our description of the VFC model as:

"Stratigraphic noise has been introduced in the classical VFC model to represent in a very crude way the effect of sublimation, condensation from the atmosphere or subsurface layers, or freezing. However, such post-deposition effects alone are probably not enough for explaining the enhanced diffusion observed in the water isotopic record on the top of the firn which we think is primarily explained by mixing of the snow and/or water vapor. In this study, we thus propose a very simple way to include the effect of surface reworking caused by wind, or the 'ventilation' effect caused by variations in surface pressure or convection due to temperature gradient i.e. different processes which lead to the mixing of the isotopic signature over a few centimetres at the surface. According to Ollivier et al. (in review), surface layers at Dome C are indeed affected by the mixing of the water isotopic signal through water vapour circulation or snow reworking in the top few centimetres in addition to sublimation effects. To account for this mixing effect, we average the isotopic profile using a sliding window of a few centimetres, thereby applying a spatial moving average with all points evenly weighted to the isotopic profile of the uncompacted snow. The mixing that occurs at the surface is thus already propagated along the entire length of the virtual firn core."

4. *Some of the model parameters used by the authors are derived from Dome C rather than from the LDC site. This choice may introduce some bias, and the authors should provide a justification for this in the discussion section.*

The LDC site is not much documented while many studies of surface snow and firn were performed at the Dome C site, 35 km away. Moreover, altitude, temperature and accumulation rate are very similar on both sites. DC and LDC sites are also located within the same grid cell of the LMDZ6-iso grid. As a consequence, in the absence of any better alternative, we derive the parameters for LDC from DC. We agree that we need to provide these explanations in the revised manuscript.

5. *When generating synthetic monthly data of precipitation δ18O, the origin and applicability of the temperature-isotope conversion coefficient are questionable. The gradient of 0.5‰·°C^{-1} may only be derived from the results at Dome C over the past decade. Whether this coefficient is applicable to the LDC region and to the past two thousand years remains unclear and should be addressed*

You are right to raise this point. There are no published studies on the exact slope to use for LDC, neither for short or long scales. For this reason, we have chosen to use the commonly accepted value of 0.5‰·°C^{-1}. However, we can carry out a sensitivity test on our synthetic data generator by varying this slope. We compared the spectra and the profile obtained for synthetic data with a slope of 0.5‰·°C^{-1} and with a slope of 0.755‰·°C^{-1} (Figure R1). The spectra are very similar at low frequencies and differ slightly at high frequencies; however, this difference does not affect our conclusion when comparing the VFC results over 84 m with the observations (ICORDA-LDC). The reconstructed profiles are also very similar. We therefore conclude that we can use the slope published for Dome C, pending better values for LDC, and for long timescales, without this affecting our conclusions. We propose to add this figure in the Appendix.

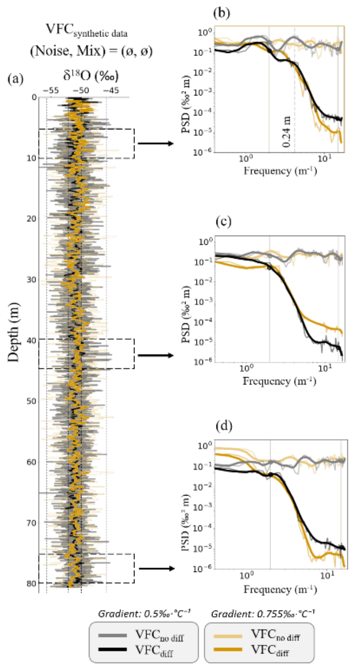

Figure R1 – VFC $\delta^{18}O$ profile before ($VFC_{no\ diff}$) and after (VFC_{diff}) diffusion, built from monthly synthetic climatic data with a slope of 0.5‰·°C⁻¹ (in grey before the diffusion, in black after) and with a slope of 0.755‰·°C⁻¹ (light and dark yellow), on (a) on the 80 m long-scale. No noise nor mixing has been added to the model (Noise, Mix) = (ø, ø). We display three profile sections between (window 1, b) 5-10m, (window 2, d) 40-45m, (window 3, f) 75-80m depth and their spectra on the right (c, e, g, respectively). The two solid lines in grey delimit the spectral domain where we evaluate the attenuation of the high frequencies after the inflection point (corresponding to Frequency = 2m⁻¹).

6. *The identification of sulfate peaks should be described in more detail. The identification of significant peaks would normally require an iterative method, but in this study, the description only states that sulfate peaks were determined based on Dome C results, without providing a detailed explanation. Additionally, differences in sulfate peaks between the two ice cores are observed, but no detailed explanation is given. A more thorough description would improve the reliability of the dating results.*

Indeed, in Gauthier et al. (2016), an iterative method has been proposed for the peak detection. As an answer to this comment, we also tried the same algorithm as in Gauthier which confirms the position of the peak as identified in the first version. This will be explained better in the new version of the manuscript as:

Section 2.3, L 156:

"To identify the sulphates peaks in the record, we use the peak detection method described in Gautier et al. (2016), based on an iterative method. The algorithm works by detecting high sulphate concentrations relative to a baseline noise level. To determine this baseline level, that could have varied in time, the algorithm scans the ice core in 1-metre sections and calculates the mean concentration (m) and standard deviation (σ). Values above m + 2σ are subtracted at each iteration, and m and σ values are recalculated, until the background noise level is isolated. The details of the algorithm are described in Gautier et al. (2016)."

About the presence/absence of some peaks according to the core, we agree that we lack an explanation for this in the text. We will comment it as:

Section 3.2, L 252:

"As in the study conducted at Dome C by Gautier et al. (2016), it is noticeable that some peaks appear in one of the cores but are absent from the other, despite the cores proximity. Gautier et al. (2016) suggest that snow drift and surface roughness may explain the absence of certain peaks in firn cores that are, nevertheless, very close to one another. "

7. *The calculation of the RMS score is based only on the differences in PSD at two specific frequency points (the inflection point A and the high-frequency upper limit point B), rather than on the entire spectrum or the whole depth profile. This approach provides very limited information and may not adequately reflect how well the model matches the observations across the full frequency spectrum or the entire depth range. Additionally, is it reasonable to use average weighting in this calculation? Could the authors provide a detailed explanation of the considerations behind this formula?*

Several reviewers noted this inconsistency for the RMS score calculation. We thus modified our method for calculating the RMS score. We now calculate the RMSD score across a wide continuous range of the spectrum, i.e. between the start of the frequency's PSD drop and the very high frequencies, stopping at twice the resolution of the widest samples to ensure that at least two points are included in the spectrum calculation. We will change this criterion in the text as:

Section 3.3.1, L 313 – 315:

"We then define the spectral slope as the attenuation slope of the high frequencies between the inflection point and a high frequency upper limit set at twice the resolution of the widest discrete sample for the top part of both cores. For the top of ICORDA-LDC, the widest samples are between 3 and 4 cm, so we decide to set the upper limit at frequency = 15 m^{-1} (corresponding to ~7 cm). For the top of Subglacior-LDC, the samples minimal resolution is between 4 and 5 cm, then we set the limit at Frequency = 10 m^{-1} (corresponding to 10 cm)."

We will change the values and the figures according to the new spectral limit on the right.

We also carried out around a hundred simulations for each combination, in order to derive the average spectrum and eliminate the random component in the noise implementation, as it became apparent to us that 10 cores were insufficient to eliminate random variability in the VFC spectrum due to the addition of noise.

The section 4.2 will be updated by adding the following elements in the revised version:

New figure (N1) in Section 4.2:

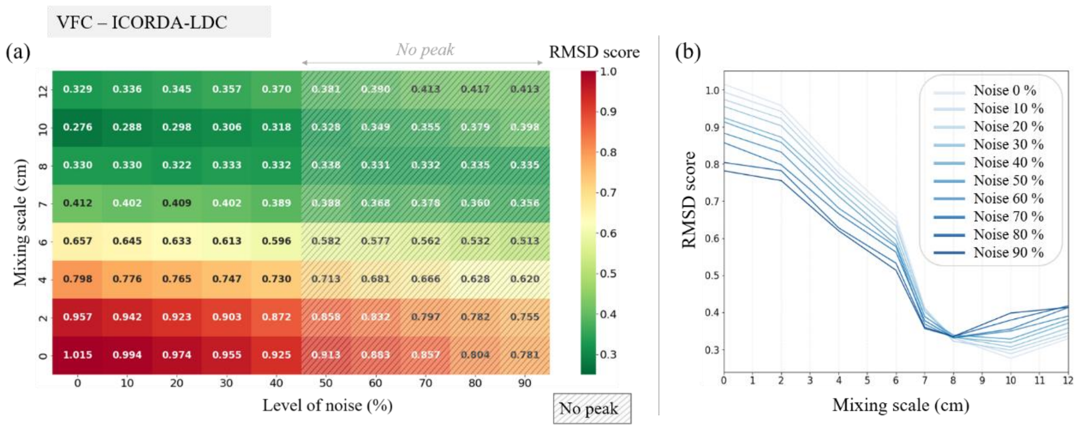

Figure (N1) – (a) Model-observations RMSD score for the various sensitivity tests of the VFC model, with a noise level varying from 0 to 90 % and a mixing scale ranging from 0 to 12 cm. The hatched grey area corresponds to simulations where the frequency peak at 18-19 cm does not appear. (b) Evolution of the RMSD score along with the mixing scale for each level of noise. A low RMS score indicates a good match between the model and the observations, while a high RMS score indicates that the model-observation fit is poor.

<u>Comment on Figure (N1):</u>

"With ICORDA-LDC, this score reveals two main findings:

(1) At least 7 cm of surface mixing is required to achieve RMS scores < 0.5 and thus bring the model closer to the observations. Above 6 cm, the RMSD score drops to reach its minimum values whatever the noise level. It appears that the mixing is the main parameter controlling the model convergence towards the observations.

(2) Above 50% noise, the peak observed around 18 cm in ICORDA-LDC is no longer visible. See Fig (N2) for a mixing scale of 7 cm."

<u>New figure (N2) in Section 4.2 for a mixing scale of 7 cm:</u>

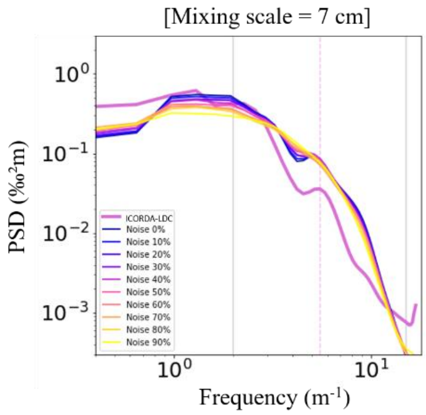

Figure (N2) – Comparison of ICORDA-LDC spectrum and VFC spectra for a range of noise levels between 0 and 90%, and a mixing scale of 7 cm. The dashed line highlights the frequency peak in the observations and the grey solid lines delimit the spectral domain to evaluate the model.

<u>General comment on Figures (N1) and (N2):</u>

"We conclude that to make the model match the observations (and thus, in practice, bring the two spectra closer together), we must add a mixing layer of at least 7 cm at the surface and limit the addition of stratigraphic noise to below 50 per cent. This allows to preserve the presence of the frequency peak at 18 cm, which has been documented in several Antarctic ice cores (Laepple et al., 2018).

Based on surface and sub-surface water isotope data (top 6 cm), Ollivier et al. (in review) proposed a preferred mixing layer of 4 cm at DC and imputed it to changes in wind strength and snow properties. Our study suggests that 4 cm is not sufficient to explain the observed water isotopes variability over the top 3 m at LDC. However, the authors suggest that a mixing layer of 7 cm is also possible (Section 3.1) which agrees with our result.

Neither any of the combinations of noise and mixing can reproduce well the profile and spectra of Subglacior-LDC, as shows by the RMSD score (Fig. N3 a, b). In contrast to ICORDA-LDC, Subglacior-LDC displays a particularly flat signal from 0 to 1.5 m depth."

<u>New Figure (N3) in Secion 4.2:</u>

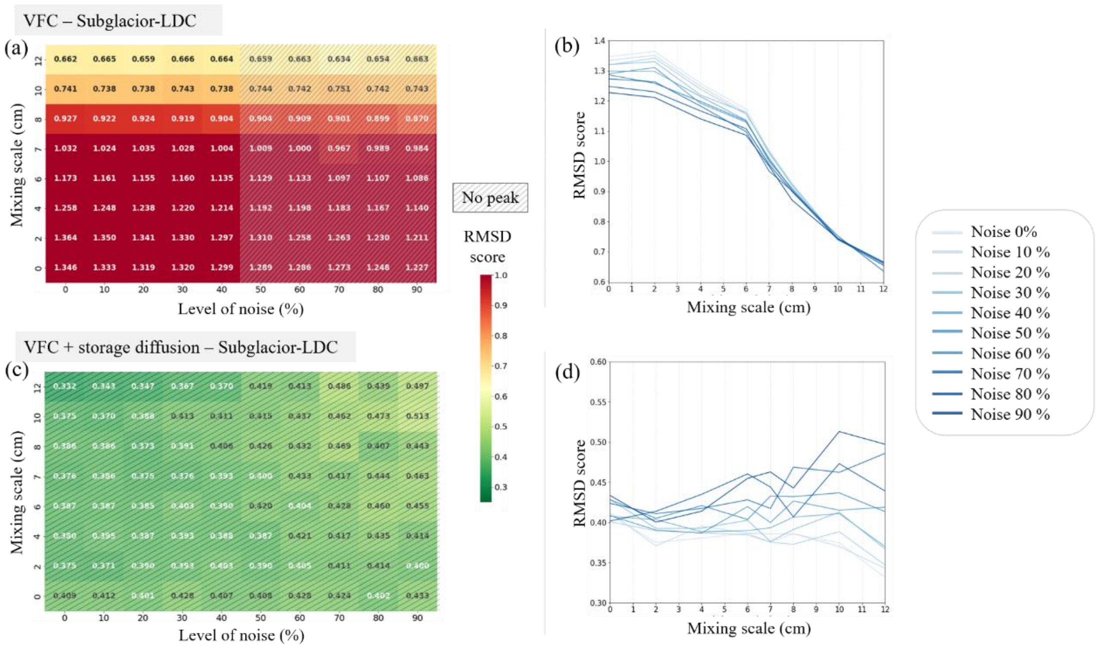

Figure (N3) – Model-observations RMSD score for the various sensitivity tests of the VFC model, with a noise level varying from 0 to 90 % and a mixing scale ranging from 0 to 12 cm, (a) before and (c) after implementing the storage diffusion. The hatched grey area corresponds to simulations where the frequency peak at 18-19 cm does not appear. Evolution of the RMSD score along with the mixing scale for each level of noise, (b) before and (d) after adding the storage diffusion. A low RMS score indicates a good match between the model and the observations, while a high RMS score indicates that the model-observation fit is poor.

Comment on Figure (N3):

"Taking storage diffusion into account clearly improves the RMSD score (Fig. 13 c, d), with values below 0.5, although it does not perform as well as the best scores for the ICORDA-LDC reconstruction when mixing is included. It should be noted that, with the addition of this storage diffusion, the effect of mixing and noise is no longer visible in the RMS score. Moreover, the peak corresponding to a 19 cm periodicity disappears from the VFC spectrum (Fig 14), in agreement with the observations in Subglacior-LDC. When a firn core is stored for too long before being analysed, diffusion caused by storage erases the isotopic signal in the upper part whatever its origin (deposition or post-deposition effect)."

New figure (N4) in Section 4.2 for a mixing scale of 7 cm:

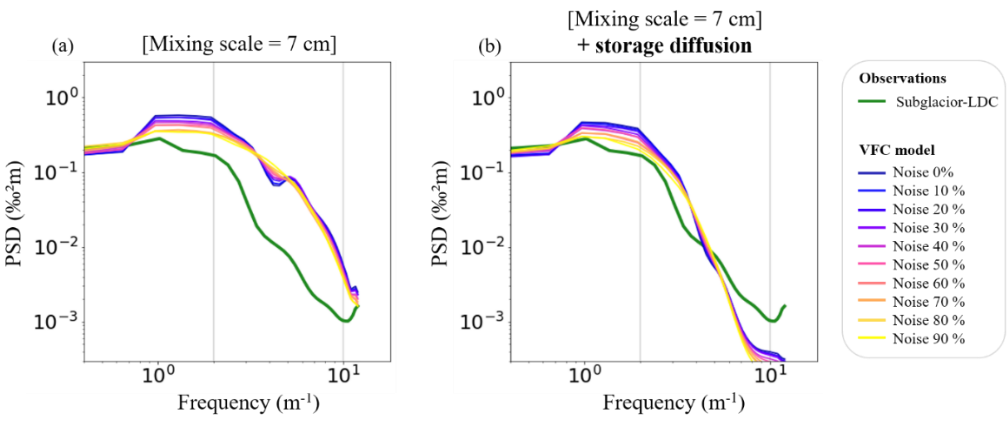

Figure (N4) – Comparison of ICORDA-LDC spectrum and VFC spectra for a range of noise levels between 0 and 90%, and a mixing scale of 7 cm, (a) before and (b) after implementing the storage diffusion. The dashed line highlights the frequency peak in the observations and the grey solid lines delimit the spectral domain to evaluate the model.

General comment on Figures (N3) and (N4):

"Adding the storage diffusion brings the spectrum of the model closer to that of the observations, but cuts off the high frequencies too sharply (Fig. 14). The storage effect as modelled in Dallmayr et al. (2025) cannot fully reconcile the model with the observations, but still clearly improve the RMSD score and the VFC spectrum, as observation match over the top part. We note that Dallmayr et al. (2025) estimated $\sigma_{storage}$ at a depth between 2.40 and 3.40 m, i.e., shifted slightly deeper than our 0-3 m depth interval. Because open porosity is larger close to the surface, we probably underestimate the diffusion due to storage effect in the Subglacior-LDC core. This storage effect is minimized for the ICORDA-LDC core due to the short storage time (a few months) prior to isotopic measurements."

8. *The authors explain that even when incorporating storage effects, the influence on ice core isotopes still cannot effectively reconcile the differences between the VFC model simulations and the Subglacior-LDC observations in terms of power spectral density. They suggest that adding storage effects would improve the agreement. However, as shown in Figure 13, at high frequencies, the VFC model simulations already fall below the actual observations, while at low frequencies, the simulations remain higher than the observations. This pattern suggests that simply increasing the storage effects may not necessarily lead to a better match between the model and the observations. Other factors might be at play. In this regard, the authors should provide further explanation and clarification.*

We should reformulate this indeed. We agree with the reviewer that our conclusion is that the storage effect as modelled in Dallmayr et al. (2025) cannot fully reconcile the model with the observations. Still, it was worth trying it since it improves the VFC - observation match over the top 1.5 m and also improves the RMSE when comparing the VFC spectrum and the spectrum of observations. We propose to include this discussion as written in the suggestion of modified text in the answer to the previous comment.

9. *The conclusions might be better restricted to the LDC site rather than being generalized to broader contexts. Limiting the scope of the conclusions to the specific study site would help avoid overextrapolation.*

We will modify the conclusion accordingly.

Minor comments:

1. *The writing in the main text and the abstract is sometimes too casual, and some sentences are hard to follow, especially in the Discussion. To improve overall clarity and readability, it might be helpful to have the manuscript carefully checked and revised by a native English speaker.*

We will perform a careful check of the revised manuscript to improve clarity and readability.

2. *Multiple references are separated by commas; these should be changed to semicolons.*

Thank you for pointing that out. We will separate the references with semicolons in the revised version.

3. *Section 2.1: Please state the depths of the two ice cores in this section*

The total depths of the two firn cores is given just before, at the beginning of section 2 (L 94-95: 120 m for Subglacior-LDC, 130 m for ICORDA-LDC).

4. *Section 2.2, second paragraph: The tense is inconsistent. Please revise.*

Thank you for pointing that out. We will revise the tense in the revised version.

5. *Section 2.3: The description of the sulfate measurement method using the Subglacior-LDC is missing. Please provide the method and a reference citation.*

Thank you for pointing that out. We will add this sentence in the revised version: "Subglacior-LDC sulphate profile was analysed with a resolution of 2 cm, following Gautier et al. (2016)."

6. *Section 2.3: Can the dating accuracy or error be provided?*

Thank you for this question, it is indeed interesting to consider the uncertainty in the dating. In our calculation of ages from tie points, the only uncertainty relates to accumulation. In Ooms et al. (2026), the accumulation rate at Dome C over the last decade is estimated at 24 ± 6 mm.w.e/year, corresponding to an uncertainty σ_{accu} of 0.25 years over 1 year. We apply this uncertainty to the LDC site, as we have no better estimate. By incrementing over several years and referring to the nearest tie point ($\Delta t_{nearest}$), we calculate the age uncertainty $\sigma_{accu,age}$ as (Eq. R1):

$$\sigma_{accu,age} = \sqrt{\Delta t_{nearest} * \sigma_{accu}{}^2} \quad \text{(Eq. R1)}$$

The Figure R2 shows the evolution of the uncertainty over the age in between the tie points:

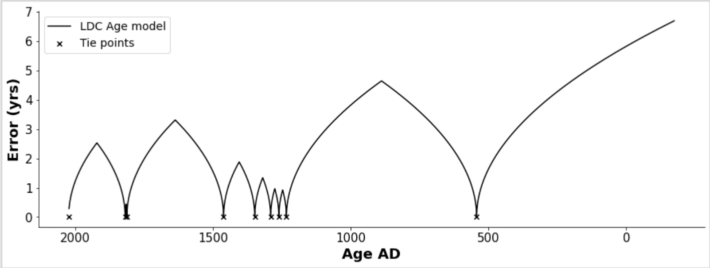

Figure R2 - Calculation of the error propagation for firn cores dating between each tie point. Volcanic horizons are marked by black crosses, where the dating error is minimal. This error increases with distance from the nearest tie point and reaches its maximum values midway between two tie points.

We will provide the age error in the revised version.

7. *Section 2.5, Line 183: Does the calculation of diffusion length require the atmospheric pressure parameter? Please clarify.*

To reply to this question and clarify our calculation following your comment below, we propose to add this text to better explain our calculation of the diffusion length:

"In the VFC model, firn diffusion represents the progressive smoothing of the original water-isotope signal after snow deposition. In the upper firn, the snow is still porous, so water vapour can move through the air-filled pore space and exchange isotopes with surrounding snow grains. This process gradually reduces small-scale variations in $\delta^{18}O$, in the same way that a moving average or low-pass filter removes the shortest wavelengths from a signal. We describe this smoothing using the classical firn-diffusion approach of Johnsen (1977), Johnsen et al. (2000), and Gkinis et al. (2014), in which the strength of diffusion is expressed by the diffusion length, σ. The diffusion length can be understood as the typical depth scale over which the isotope signal has been blurred: the larger σ is, the more strongly short-scale variability is attenuated. In the model, σ is calculated as a function of depth because diffusion evolves as the firn becomes denser and less porous. It also depends on local temperature, accumulation rate and atmospheric pressure, which control how efficiently water vapour can move through the firn. More details on how this parameter is calculated can be found in Gkinis et al. (2021)."

8. *Appendix B: The content is too brief, only showing results. Appropriate text and necessary formulas should be provided to explain the calculation process.*

We replied to this comment above (question 7).

9. *Section 3.1: After removing the top 3 m, the standard deviations of the isotope data in both two cores become larger. This is unreasonable because diffusion should reduce high frequency isotopic fluctuations with increasing depth (as stated in the introduction). Such an effect will decrease the standard deviation. So, please check the accuracy of the data and provide a reasonable explanation for this result.*

We provide below some explanations for this apparent mismatch.

For Subglacior-LDC, the low standard deviation at the surface corresponds to the surprising flat isotopic profile measured in the upper part of the core. We suggest that the disappearance of the isotopic variability is at least partly linked to strong storage diffusion in this section of very low density (very high porosity).

For ICORDA-LDC, the lower standard deviation on the top 0-3 m is due to differences in resolution between the upper part (analysed as discrete samples) and the rest of the core (analysed using CFA). This is the reason why we have distinguished between the [0–3 m] and [3–30 m] lines in the table. If we reduce the resolution of the entire core to that of the widest sample (4 cm), we obtain std [0 – 84 m] = 1.217; std [0–3 m] = 1.314; std [3–84 m] = 1.212, which is consistent with the diffusion increase with depth. We will insist on these differences of resolution for the different series of measurements and implication for the presented values of the standard deviation. We thus confirm the consistency of the data with these explanations.

10. *Table 1: Units are missing, and the necessity of this table is not clear. Please revise or justify.*

Thank you for pointing the lack of unit, we will correct it in our table. As our data will be published on Pangaea, you are right to point out that this table is not necessary in the appendix; we therefore propose to remove it.

11. *Figure 3: The y-axis mark in the second subplot is incomplete, and the label for sulfate lacks the ion symbol (SO_4^{2-}).*

Thank you for pointing that out. We will revise the figure.

12. *Section 4.2: The authors state that a 6–8 cm mixed layer is "consistent with Ollivier et al.". However, Ollivier et al. reportedly proposed a 4 cm mixed layer. Unless Ollivier et al. also considered 6 – 8 cm as possible or within their uncertainty range, this is a logical contradiction. It seems the intended meaning might be that the 6–8 cm mixed layer is consistent with Ollivier et al.'s interpretation that the mixed layer is driven by wind and snow properties, rather than consistent with the numerical value of 4 cm. However, the current wording is misleading. Please clarify explicitly what "consistent" refers to.*

Thank you for your comment. We propose to qualified this statement as:

"Based on surface and sub-surface water isotope data (top 6 cm), Ollivier et al. (in review) proposed a preferred mixing layer of 4 to 8 cm at DC depending on the parameterization of the fractionation during sublimation. Our study is in better agreement with a 8 cm mixing layer."

13. *Numerous spelling errors and inappropriate word usages are found throughout the text. The following list provides only some of the visible problems, but these are by no means exhaustive. The authors should carefully check and revise all language issues in the manuscript.*

Thank you very much for listing these spelling errors, we will be careful to correct all of them in the revised version.