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. *Implementing a surface mixing is a great contribution to the discussion on the signal formation; however, the manuscript lacks any substantive discussion of other post depositional modifications, particularly sublimation, wind-driven erosion and snow redistribution. These processes are known to affect isotopic records in low accumulation regions such as Little Dome C. The authors mention these processes in the introduction and the title also suggests that these processes are addressed in the study. These processes should be at least discussed regarding their potential influence on the presented records (if not modelled). Furthermore, I am not an expert in simulating long time scales; therefore, I am unable to assess whether the assumption regarding the input climate for the 80m simulation is accurate, and I hope that another reviewer will be able to address this issue.*

We agree that we did not detail enough the other post-deposition studies. These processes are not explicitly represented in the VFC model. Actually, they are represented through the noise term but as we show in this paper, using intermittency of the precipitation and noise does not match properly the observations at LDC so that an improved representation of the processes is needed. We propose here to add the mixing in the top which is a simplified way to represent the forced exchange of water vapor through pressure or temperature gradients and mechanical snow reworking.

We will explain this strategy better in the new version of the manuscript:

"The mixing of the isotopic signature of the surface layer – whether due to wind or to the ventilation of the firn caused by barometric pumping or to convective mixing due to temperature gradients – is not the only post-depositional process affects the water isotopes in the snow surface. Vapour exchange between the snow surface and the atmosphere (Ritter et al., 2016; Wahl et al., 2022; Dietrich et al., 2023) has an impact on the isotopic composition of snow (Casado et al., 2021, Ollivier et al., 2025). The effect of surface sublimation is currently being investigated and may bias the mean isotopic composition of the profile (Ollivier et al., in revision). It will be considered in a future optimisation of the VFC model. Condensation of water vapor from the atmosphere on surface or subsurface cold snow and surface freezing can also affect the isotopic signature of the surface layers (Craig and Gordon, 1965; Merlivat and Jouzel, 1979; Cappa et al., 2003; Casado et al., 2021). In the present VFC model, these effects are all included in the noise term but future developments may include a more physical representation of these post-deposition processes."

2. *It remains unclear whether the implemented surface mixing is an analogue to diffusion and the manuscript does not sufficiently explain how mixing is calculated. In line 176 it is written that the mixing is a "spatial moving average", but it is not mentioned if this is, for instance, a weighted average, and during which step of the modelling this mixing is applied. A more explicit description of the underlying assumptions and of the parameterisation would strengthen the methodological transparency of the study.*

Thank you for this comment. The simplest way to include the surface mixing is to average the isotopic values over a few centimetres at the surface. We will extend our description of the VFC model as:

<u>L174 – 177:</u>

"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."

3. *The authors acknowledge that one core was stored for some years and that Dallmayr et al. (2025) reported isotopic changes associated with storage. Given this, a direct comparison of both cores without applying a correction is not well justified. If storage effects are analogous to diffusion, back-diffusion algorithms may offer a viable correction approach and should at least be considered and discussed.*

We do not feel that back-diffusion is useful in this study in which we do not aim to reconstruct the initial signal and prefer to stick with forward diffusion within the VFC model following the general flow of the manuscript. Moreover, the diffusion length estimated by Dallmayr et al. (2025) for storage diffusion was performed for firn densities significantly higher than for the surface firn (2.40 to 3.40 m depth in Dallmayr et al., 2025) so that any back-diffused reconstructed signal will be associated with large uncertainties. We will however add this comment in the revised manuscript for the future uses of these records for paleoclimatic reconstructions.

4. *The manuscript relies on SO_4 concentrations alone for core dating. The authors might consider calculating non-sea-salt sulphate (nss-SO_4) concentrations and using a robust threshold criterion. This would yield more reliable stratigraphic tie points and improve comparability with other records and studies.*

Following a comment of reviewer 1, we have improved the detection of sulphate peaks using the iterative method described in Gautier et al. (2016) for the peak detection. We use now the same algorithm than in Gautier et al. (2016) and describe the new version of the method as:

<u>Section 2.3, L 156:</u>

"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)."

Following your suggestion, we have also tested the use of nss-SO_4. We compared our raw sulphate data with the data obtained by applying

$[SO_4^{2-}]_{nss} = [SO_4^{2-}]_{total} - k * [Na^+]$ (from Ishino et al., 2019)

with k = 0.16 +/- 0.09 for Dome C (Legrand et al., 2012), the $[SO_4^{2-}]/[Na^+]$ mass ratio in sea salt particles.

The results are very comparable when looking at the SO_4^{2-} or nss-SO_4^{2-} records; we observe only very slight differences in the intensity of the peaks, which are not significant. Furthermore, in his thesis manuscript, Gautier argues that the use of nss-SO_4^{2-} has no impact on the detection of volcanic peaks at distant sites such as Dome C (and therefore also Little Dome C).

(*Reference:* Section I.1.3.3, Contribution du bruit de fond, in Elsa Gautier. Empreinte isotopique et histoire du volcanisme stratosphérique des 2600 dernières années enregistrées à Dôme C, Antarctique. Sciences de la Terre. Université Grenoble Alpes, 2015. Français.)

5. *The study by Ollivier et al. is often cited, especially in the discussion. Yet, since this paper is not published, it is in parts difficult to follow the line of argument. The authors should briefly summarise the key findings of Ollivier et al. where relevant. Additionally, it remains unclear whether the results presented here (and those of Ollivier et al.) are specific to the Dome C region or whether they can be generalised to the broader East Antarctic Plateau. A discussion on the question of spatial applicability would strengthen the manuscript.*

Many thanks for this comment. Indeed, we should better explain the main finding of the Ollivier et al. (in revision). The authors used a model for post deposition processes at the surface of the firn on the Dome C site taking into account the sublimation effect. This model was already developed for Greenland (Dietrich et al., 2023) and applied by Ollivier et al. (in revision) for the first time to the surface of Dome C. Ollivier et al. found that to get the best agreement between model outputs and isotopic measurements of the surface (0-3 cm) and subsurface snow (3-6 cm), a mixing length of 4 to 8 cm should be introduced in the model. Because this paper is still in revision, we will better describe the model and the results in the new version of the manuscript.

Reviewer 1 also pointed out the possible differences between DC and LDC and associated limitations when using results from Dome C to LDC. However, 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 rates 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.

We agree that we have to be more specific in our conclusions. We propose to add the following comment in the revised version:

"The firn cores from LDC are characteristic of a site with very low accumulation, typical of the East Antarctic Plateau. Among the sites investigated on the East Antarctic Plateau, Dome C has a particularly low accumulation rate, and Little Dome C is estimated to have an accumulation rate that is even slightly lower. The raw result of our study in terms of combination of level of noise and surface mixing scale applies first to Little Dome C on which we have firn isotopic profiles, but the model with the mixing addition can be used for other Antarctic sites, to identifying the best range of noise level and mixing scale for different sites."

6. *I am missing a data availability statement, links to a data repository to access the data and a link to the code of the model. I would only accept this study for publication if these links were included.*

Your request is perfectly justified. Data are ready to be published to Pangaea, at the same time as the study publication, under the following references that will be added in a "Data availability" section in the final version of the manuscript:

Samin, Emma; Landais, Amaëlle; Fourré, Elise; Combacal, Thomas; Minster, Bénédicte; Gautier, Elsa; Jossoud, Olivier; Jacob, Roxanne; Orsi, Anaïs; Possenti, Philippe; Alemany, Olivier (2026): Field density profile of ICORDA-LDC firn core from Little Dome C, Antarctica [dataset]. PANGAEA, https://doi.org/10.1594/PANGAEA.993899

Samin, Emma; Landais, Amaëlle; Fourré, Elise; Combacal, Thomas; Minster, Bénédicte; Gautier, Elsa; Jossoud, Olivier; Jacob, Roxanne; Orsi, Anaïs; Possenti, Philippe; Alemany, Olivier (2026): Sulphates record from ICORDA-LDC firn core from Little Dome C, Antarctica [dataset]. PANGAEA, https://doi.org/10.1594/PANGAEA.993900

Samin, Emma; Landais, Amaëlle; Fourré, Elise; Combacal, Thomas; Minster, Bénédicte; Gautier, Elsa; Jossoud, Olivier; Jacob, Roxanne; Orsi, Anaïs; Possenti, Philippe; Alemany, Olivier (2026): Water isotopes record from ICORDA-LDC firn core from Little Dome C, Antarctica [dataset]. PANGAEA, https://doi.org/10.1594/PANGAEA.993901

Samin, Emma; Landais, Amaëlle; Fourré, Elise; Combacal, Thomas; Minster, Bénédicte; Gautier, Elsa; Jossoud, Olivier; Jacob, Roxanne; Orsi, Anaïs; Possenti, Philippe; Alemany, Olivier (2026): Water isotopes record from Subglacior-LDC firn core from Little Dome C, Antarctica [dataset]. PANGAEA, https://doi.org/10.1594/PANGAEA.993902

The code of the model is still under development to implement new surface processes, such as sublimation, and will be available in its final version in the coming months. We would prefer to keep only a single version of the model online, but we propose making our version available on request, by adding this note to the revised version.

Minor comments:

- *L. 26: It is mentioned that the deposited snow preserves an imprint of local and global climatic conditions. I am a bit puzzled with this statement and am also missing the respective references to both, the local and the global signal.*

With this sentence, we actually had in mind the fact that water isotopic composition archives a climatic signal which can be representative of a large spatial scale (when it comes to large changes such as deglaciations) but it also affected by local variations in deposition rate for example. Still, we agree that the sentence is not so easy to understand with much context and we propose to replace it by:

"The successive layers of snow accumulated in polar regions preserves an information on past climatic conditions. As an example, the isotopic composition of the snow is largely used as a proxy for temperature at the time of precipitation (Jouzel et al., 2013).

• *L. 169: Using 1 cm, are you just following Laepple et al. (2018) or did you also perform a sensitivity test to figure out what the best noise scale is (for your specific data)?*

We carried out tests at the beginning of our study modifying the noise scale (between 1 and 3 cm), but none of these runs improved the agreement between VFC outputs and observations. The model - observations mismatch could only be improved through implementation of the mixing parameters. Moreover, the 1 cm noise scale proposed by Laepple et al. (2018) seems well justified and we thus kept this value for the manuscript. We will however make clear in the new manuscript that we ran tests with different noise scale without being able to improve the data - model comparison.

• *L. 186: Are you applying the MTM method in depth or time dimension?*

We apply it in depth. We will include this clarification in the revised version.

• *L. 228: The unit "m" following "0.48 ‰2" is unclear.*

The unit of the PSD is the square of the unit of the variable (‰ in this case), divided by the frequency, thus $‰^2/m^{-1}$, or $‰^2m$.

• *Figure 5: what are the cycles than are mentioned here? I am missing the description and usage of them in the text.*

Thank you for your comment, we will extend the description of these cycles in the revised version.

- *L. 309: remove**s***

Thank you for pointing that out. It will be corrected in the revised version of the manuscript.

- *L. 342: Please clarify what physical process or quantity is represented by the surface mixing term.*

Thank you for your comment. We are aware that our study is not a process study and we cannot point to a specific mechanism for explaining the mixing in the top layer. The surface wind may be a key driver for the mixing process but to parameterize this 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 the simplest way to include surface mixing, 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. We will better explain this point it in the revised version.

- *L. 421: "visual comparison" – can you try to quantify it or at least show the comparison (not only in the appendix)?*

Following a major comment of Reviewer 1, we decided to revised our computation method for 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. The integration of the score along a wider range of the spectrum allows a better picture of the model performance.

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.

Both improvements improved the robustness of the comparison between the model and the observations spectra. The new figures and results will be discussed in Section 4.2 of the revised version.