

## **Response to Reviewer's comment 1 (RC1)**

We thank reviewer 1 for its positive feedbacks on the manuscript and very helpful comments and suggestions to improve it. We address below the comments with a point-to-point answer.

RC1: 'Comment on egusphere-2025-2863', Anonymous Referee #1, 02 Nov 2025

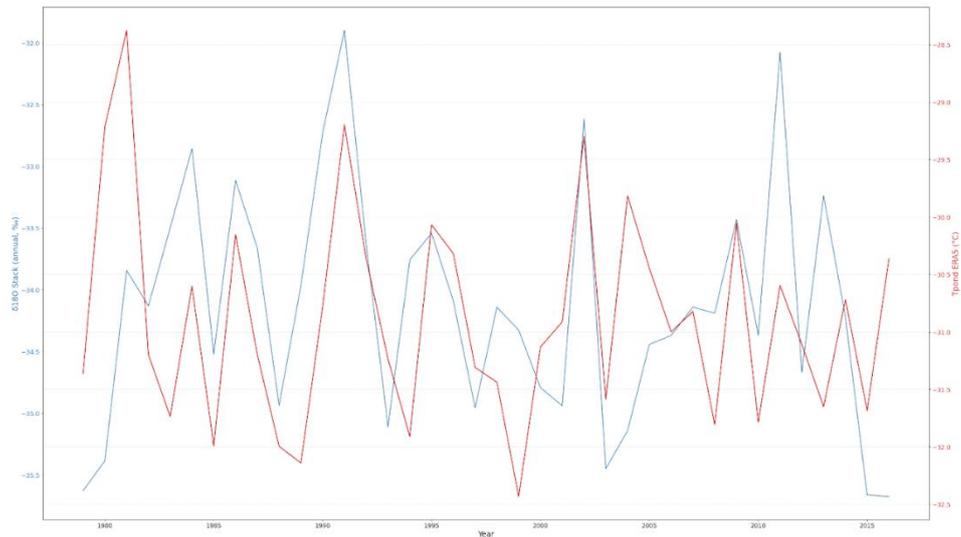
The paper by Tcheng et al. is reporting an in-depth analysis of the isotopic composition of individual and stacked records from firn cores (a total of 9 cores) collected at three sites in a coastal area of Adelie Land in Antarctica. These sites are characterized by a relatively high snow accumulation rate allowing, through a continuous flow analysis method, to obtain high resolution  $\delta^{18}\text{O}$  records along with  $\text{nssSO}_4$  profiles. These high-resolution records allow a very precise dating, at least for the two sites named Stop5 and Stop0. All the cores exhibit seasonal variations but at the D47 site the strong katabatic winds are strongly affecting the isotopic records. A stacking record, obtained for each of the three sites, permits to reduce the stratigraphic noise and is then compared to a Virtual Firn Core (VFC) calculated using two isotopic GCMs, ECHAM6 and LMDZ6, over a common period 1979-2016. The two less disturbed sites show consistent seasonal variability as the VFC records, allowing to observe common trends in  $\delta^{18}\text{O}$ . This study, through a very detailed analysis of the high-res records, demonstrates the difficulty in obtaining a clear link between temperature and  $\delta^{18}\text{O}$ , particularly in those sites that may be disturbed by redistribution processes by winds, as is the case for D47.

The paper is original and novel in the sense it uses a very detailed isotopic and chemical records for obtaining a climate reconstruction to be comparable to the climate data obtained from isotopic models. The data are well presented and discussed, and I have found the reading quite smooth although some parts could be reduced a little.

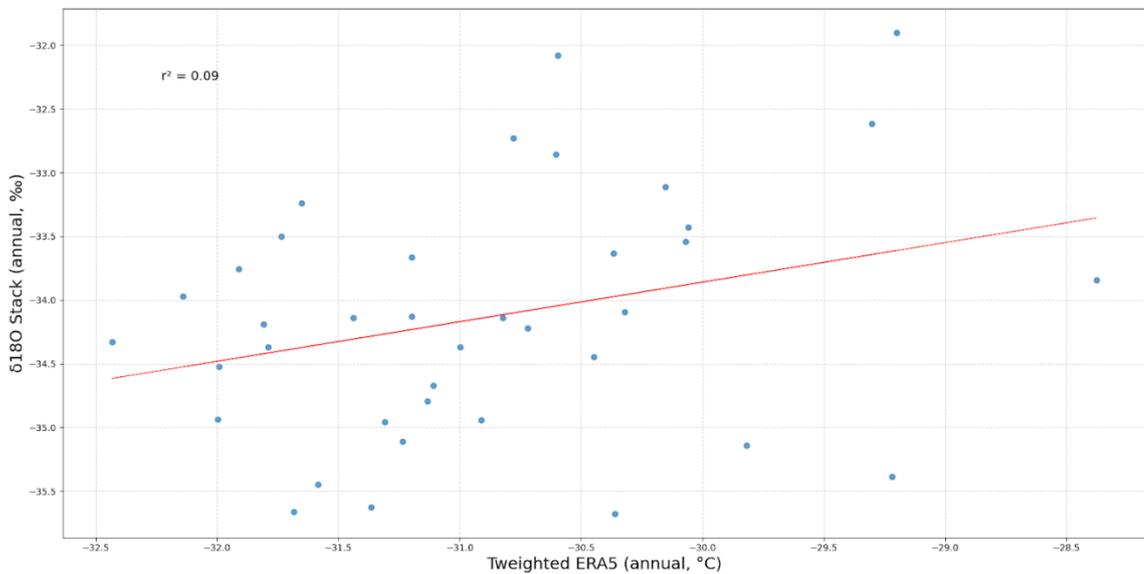
I recommend its publication after the authors have been considered to the following minor comments.

I have a general comment: why not attempting to calculate mean annual  $\delta^{18}\text{O}$  values to be compared to VFC data and ERA5 precipitation weighted data? This could bring some information to eventual the  $\delta^{18}\text{O}/T$  sensitivity at interannual scale. Has this been done or at least checked?

>> Many thanks for this comment. Indeed, we checked how it compares and unfortunately, the comparison is not very convincing. When looking at the temporal evolution of mean annual stacked  $\delta^{18}\text{O}$  and weighted temperature (Figure 1), we do not observe a very strong correlation which is confirmed by the graph showing the evolution of stacked  $\delta^{18}\text{O}$  vs weighted temperature (Figure 2).



**Figure 1:** Temporal evolution of the mean annual weighted temperature (red) and stacked  $\delta^{18}O$  (blue) at stop0. Averages are calculated from January to December (tests were also performed when calculating the average from July to June leading to the same results and conclusions).



**Figure 2:** Relationship between mean annual stacked  $\delta^{18}O$  and weighted temperature over the period 1979 - 2016. Averages are calculated from January to December (tests were also performed when calculating the average from July to June leading to the same results and conclusions).

The title: I would suggest changing “covering the last 40 years” since it is misleading, they are not the last 40 years...

>> The title has been changed as “Multiproxy analyses of multiple shallow firn cores from coastal Adélie Land”

Page 2, line 1: “ ... closely linked to .... ice mass loss ...”

>> Corrected

Page 2, line29: may you specify “long-term” how many years it is?

>> We already specified the period in the submitted version: “(continuously since January 2019)”

Page 7, line 147: may you add which is the period covered?~1

>> Many thanks, we now precise “(~last 40 years)”. We can not be more precise since this sentence is written before building the chronology.

Page 10, line 232: all the nssSO<sub>4</sub> profiles feature common patterns. However, If I am looking at figure 2 the sulphate records at Stop5 are not so similar....

>> Our sentence was probably not very clear. We referred to the similarities between nssSO<sub>4</sub>, Na and Cl records on each core. The sentence has been rewritten as “For each core, the variability recorded in the nssSO<sub>4</sub> profiles (Figure 2 and S1) share strong similarities with those in Na<sup>+</sup> or Cl<sup>-</sup> profiles “

Page 10, lines 248-249: the Pinatubo signal: I agree with the authors that it is quite ambiguous, but if I am looking at the figure S5 the signal is well evident at Stop0 (stacked profile).

>> Many thanks for your comment. As mentioned, we can see a signal corresponding to this eruption (“A slight increase in the average value of nssSO<sub>4</sub> can still be observed between 10 and 12 m at Stop0 and Stop5 and between 7 to 12 m at D47 and may include the signature of the Pinatubo eruption “). Still, because it was quite ambiguous, it was not used for the chronology construction. It is however a good support for the dating that we have this small signal at the right age.

Page 13, Table 3 and also in the text to be specified if the data regarding the snow accumulation from stake is referring to a mean value obtained from a stake farm or is one single stake value and which density values have been used and to which year is referred.

>> At Stop0, 5 stakes are used and the accumulation is calculated over the period 5/12/2016 to 30/1/2020. We added the number of stakes used at Stop0 in the caption and in the table. The density used at stop0 is the one measured on the firm core.

At D47, there are 2 stakes (as written already in the first manuscript) and the value is given as an average of annual accumulation estimated each year over the period 2004 - 2020. The density is measured every year through a 250 cm profile.

All missing details are now given in the revised manuscript in the Table 3 and caption.

Page 18, figure 5: If I am looking at the records from Stop5 I found very different trends and patterns between staked records and VFC ones, around 1990 +/-3 years. May you comment on this? Perhaps are you referring at this point at page 19 lines 392-393? Add in the text.

>> We propose to add the following sentence “As an example on Stop5, we see that while the stacked data displays a clear maximum around year 1989, the modeled signal is much more muted. “

Page 20, lines 411-413: may you explain better for the reader how the local roughness is calculated. There is an explanation in the table caption, but I would suggest you move it to the main text.

>> Done. We added the following sentence “the surface roughness is documented by calculating the root mean square of surface deviation on a circle of 300 m around the drilling as obtained from the Reference Elevation Model of Antarctica (Howat et al., 2019). “

Page 20, lines 427-429: see my previous comment above (figure5).

>> We have removed the definition from the caption.

Please, change in all the figures (text and supplementary) the X axis title and labels from mm to m.

>> Corrected

Please check all the delta symbols in the text.

>> Indeed, some “delta” were not well formatted. This has been addressed when sending the final manuscript through a thorough final reading.

Please check all the table format.

>> Corrected

In the Supplementary: the caption of figure S5 is referring to which core site? Stop0?

>>It was Stop5. We now specify the site in the caption

Is the code for calculating the VFC records free available?

>> The code can be found here: <https://gitlab.in2p3.fr/ips/lsce/glaccios/glacio-psm>  
It is now mentioned in the revised version of the manuscript.

## Response to Reviewer's comment 2 (RC2)

RC2: 'Comment on egusphere-2025-2863', Ruth Mottram, 01 Dec 2025

REVIEW: Tcheng et al. Multiproxy analyses of multiple firncores from coastal Adélie Land covering the last 40 years

Note: This review was written by a research group following a thorough discussion of the paper within the group. This was an interesting paper that we have enjoyed reading and discussing and we find it a potentially valuable contribution to the community of Antarctic climate researchers. It is well written and presented and includes some valuable datasets.

>> Many thanks for this positive opinion.

The paper uses the stacking method of firn cores as a way to quantify and reduce uncertainties in firn core isotopic signal, called stratigraphic noise. For several isotopes, at three sites, with three cores at each site, they compare the individual isotopic signals to the stacked one, and to virtual firn cores compiled from isotope-enabled climate models. The authors show that there is greater resemblance between the stacked signal and the virtual firn core signals for two out of three sites. The individual records are quite similar for the first two sites, but present slight variations in depth and amplitude. The site where stacking is inefficient is known for high winds, meaning significant ablation and heterogeneous accumulation (surface roughness, recorded wind speed, modelled wind speed), causing significant differences in signal over very short distances, and generally making this site inappropriate to retrieve any climatic information. It is observed that stacking does erase extremes, including the warmest months. In general, stacking appears to be a reasonable method to increase the reliability in isotopic signals from firn cores.

We identify a few major issues in the paper and a few minor ones.

2 Major issues

2.1 1. Structure and justification of the paper

It is not totally clear at the start what the major scientific objective is and how it fits into previous research. It would be helpful to introduce the scientific ideas that provoke this study, the introduction section reads a little disjointed. The objectives do become clearer upon reading the discussion however, in particular section 4.2. We therefore suggest adding a little more detail to the abstract and the introduction around the overall aims, in particular the potential to identify climate/seasonal/weather signals from the firn cores.

We assume that another objective is to use these kind of datasets to evaluate general purpose GCMs as well as isotope enabled ones, and indeed this is a valuable dataset for the purpose, but it is not clear upon first reading of the paper and some discussion of this would also be useful.

>> We agree with these two comments. To better convey the flow of ideas, we rewrote a bit the second part of the abstract as:

“At two sites, we find a good agreement when comparing the water isotopic profiles recovered from the stacked records to those obtained as modeling output from virtual firn cores calculated using the two isotope-enabled atmospheric general circulation models, ECHAM6-wiso and LMDZ6iso over the period 1979-2016 which support the good performances of the two models for the Adélie Land region. At the very windy site of D47, building a coherent signal from the 3 individual cores is not possible because the isotopic and impurities signals are much more affected by stratigraphic noise. This study

confirms that, even if the benefit of stacking is limited at very windy sites, combining several cores is of primary importance to faithfully reconstruct water isotope variability at one site. We also show that the stacked record permits to identify some strong climate signals recorded in the water isotope profiles.”

In the introduction, we were already mentioning the goal as “(2) assessing how much climatic information can be retrieved from water isotopic records in these firn cores.”. We now also modified the end of the introduction with this sentence “We then compare the stacked  $\delta^{18}\text{O}$  records at each site with virtual firn cores (VFCs) derived from the isotope-enabled AGCMs ECHAM6-wiso and LMDZ6iso showing how our approach can be used for the evaluation of these models. In section 4, we investigate the possibility to retrieve climatic information from our stacked cores and especially evaluate if the strongest summers can be recorded in the stacked isotopic records. “

In section 4.1, we added a sentence to insist on the good agreement between model and data hence validating the use of the isotope enabled AGCM on this region “At Stop5 and especially at Stop0, there is a strong resemblance between the stack and the VFC records even at the seasonal scale. This good agreement is a strong validation for the performance of the ECHAM6-wiso and LMDZiso on this region”.

Finally, we added the following sentences in the conclusion and perspectives to strengthen the importance of the use and validation of models in this study:

“Moreover, the good agreement between model outputs and stacked data support the good performances of the two water isotope models used here for the Adélie Land region.”

“Finally, a perspective is to perform modeling experiments to investigate how climate variability is imprinted in the VFC when changes in moisture origin and intermittency of the precipitation affect the water isotopic profiles. “

### 2.2.2. Objectives

There are two listed objectives on lines 41-44. The first one focuses on improved isotopic record with respect to stratigraphic noise, the second is to expand that firn core knowledge to the recent climate. It would be clearer if you mention that these research questions can be answered by specifying stacking, but also they should be tied back to the wider points you make at the start about Antarctica’s role in the global climate system. The general importance of good isotopic records is clear from the intro, and stratigraphic noise is raised as a limiting issue (lines 39-40) but it feels a little disjointed.

>> We tried to take this point into account to relate the questions to the stacking and put the objectives in the context of Antarctic climate with the modified sentences:

“In this study, we propose an integrate approach to produce dated stacks of shallow firn cores in Adélie Land in order to (1) reconstruct the isotopic composition of precipitation from firn cores in coastal Adélie land with a reduced impact of stratigraphic noise and (2) assess how much climatic information we can retrieve from water isotopic records in these firn cores hence improving the knowledge of climatic variability in Antarctica. “

### 2.3.3. Stacking

In this paper, you focus on stacking and comparison to the virtual firn cores. It would be nice to have a bit more background on the previous uses of firn core stacking, how it was developed and how obvious it is of a method (a very brief literature review found no recent papers), and it would be very beneficial to make it a key part of your research question. The definition of stacking is however a bit unclear, is it the application of the Palaeochrono software, which is described quite well? Or is it more complex than that? Reducing this ambiguity would be helpful for readers like ourselves who are knowledgeable on Antarctic climate, but not necessarily the specific isotopic methods applied in this

paper. Similarly, I found the results rather interesting, but difficult to place in context. You find that stacking does improve things for 2/3 sites, is that consistent with other studies in the literature?

>> Indeed, stacking is not much done yet for shallow firn cores and the approach that we propose here with PaleoChrono has not been applied before. This is first because PaleoChrono is mostly dedicated to chronology construction on long timescales and second because there are not so many sites where several neighbouring firn cores have been drilled and analyzed. Still, in Antarctica the stacking approach for local and regional series reconstructions has been suggested by Münch and Laepple (2018). We could also cite the studies of Münch et al. (2016) and Hirsch et al. (2023) which focus on compiling shallow firn records but without proposing a stacking as done in this study (these studies consider only the variability on a depth scale and there is no conversion to any common timescale). The stacking is however often done for long timescales, either sediment cores (Lisiecki and Raymo, 2005) or ice cores (Parrenin et al., 2012). For reconstruction at intermediate timescales, a study was however recently performed in Greenland (Hörhold et al., 2023) with stacking of multiple cores for a reconstruction over the last 1000 years using individual dating by annual layer countings on water isotopes and alignment between cores through volcanic eruptions.

We find that stacking is useful for 2/3 sites but actually, given the strong wind and surface roughness at D47, it was quite hopeless to do something here. It should be noted that D47 is a very specific site in Antarctica so that it is actually quite encouraging that on the two other sites the stacking is really beneficial.

To help making your research question more relevant, it should be clear in the introduction how stacking was used before, and where stratigraphic noise was a limitation. Lines 454-463 clearly identify stratigraphic noise as a limiting issue, that one-core can be completely un-representative, and that stacking solves those issues. This would make a great ending to your introduction, leading directly into your research question.

>> We followed this excellent suggestion and have rewritten the end of the introduction as follows: “This variability leads to the so-called stratigraphic noise, which complicates the retrieval of climate signal from water isotopes (Fisher et al., 1985), especially at the seasonal scale (Münch et al., 2016; Hirsch et al., 2023). Core stacking is a way to separate the local (including stratigraphic signal) from the more regional signal. It has largely been applied for global climate reconstructions using deep sediment cores (Lisiecki and Raymo, 2005) or ice cores (Parrenin et al., 2012; Landais et al., 2015; Buizert et al., 2018; Hörhold et al., 2023) but less often for shallow firn cores from the same site, even if this idea has been developed in Münch and Laepple (2018). In this study, we propose an integrated approach to produce dated stacks of shallow firn cores in Adélie Land in order to (1) reconstruct the isotopic composition of precipitation from firn cores in coastal Adélie land with a reduced impact of stratigraphic noise and (2) assess how much climatic information we can retrieve from water isotopic records in these firn cores hence improving the knowledge of climatic variability in Antarctica.”

In addition, as mentioned in the previous paragraph, wind is a known issue (you cite Dallmayr et al. 2025b, line 414 on the effect of wind-induced high surface roughness). Even if this is not a completely novel idea, a very valuable finding would be whether stacking can improve the records in those locations.

>> This is again a good point. We have precised our conclusion in l. 414 as “We thus conclude that D47 is not a good site to provide annually resolved climate and/or atmospheric water cycle reconstructions from water isotopic records in firn or ice core using only a stacking of 3 neighboring cores. “. We also added some precisions and perspectives in the conclusion as follows: “A comparison of these isotopic records with VFC isotopic curves produced from outputs of the two AGCMs ECHAM6-wiso and LMDZ6iso shows that stacking 3 firn cores does not enable the recovery of a climatic signal at the very windy place of D47.”

and

“A stacking of a higher numbers of cores could also permit to reconstruct climatic variability at windy sites affected by high surface roughness such as D47.”

On a side note, we wonder if sharp changes in isotopic signal at the windy location could be used to reconstruct severe wind erosion events?

>> This is an interesting point. We have indeed a few of these sharp changes in the isotopic record which are most probably linked to wind events. However it's not easy to further explore this idea with the data at hand because of the strong uncertainty in our dating for this site, hindering comparison with daily or even monthly wind gusts data.

#### 2.4.4. Models

It is not clear how you compare the 2 isotope enabled GCMs with the in situ data. They have two very different resolutions, with LMDZ-iso in particular being run rather coarsely. How did you sample or downscale this to the resolution of the in situ data? Does this have any impact on the results you find?

>> There was no downscaling of the model outputs. This is the reason why there are clear differences in the altitude when comparing data and model output on one grid. This is particularly true at D47 which is on the slope, hence with large altitudinal variations with distance to the sea. This is not as strong at Stop0 and Stop5. As discussed in section 4.1, this has a clear impact on the comparison of the mean annual temperature and hence  $\delta^{18}\text{O}$ . Still, when generating the VFC, we have tested several outputs of the models using the 8 neighbouring grid cells and not only the grid cell corresponding to the location of the site of interest. For the three sites, D47, stop0 and stop5, we found rather similar VFC profiles when comparing the outputs obtained with the different grid cells.

#### 2.5 Extreme month ranking

The use of the top 12 warmest summers for each proxy is quite an arbitrary metric, quantitative alternatives exist. In Table 6, you compare the different proxies and look for agreement in predicting the top 12 warmest summers. This is a very arbitrary number, and a relatively arbitrary method too, where the 13th position is infinitely less important than the 12th, although there is no significant difference in those two rankings, but the difference between 1st and 12th is non-existent. The general intention of that table somewhat works, there is some agreement between different proxies in finding warmer summers. But we would encourage you to replace it or complement it with a more quantitative method, for example the Spearman's rho, looking for resemblance in the ranking between the proxies.

>> Many thanks for this advice. It is indeed a suggestion that we envisaged when writing the manuscript but unfortunately, it is not so obvious to find a similar ranking between the different series as shown in Table 1 below. As a result, the Spearman correlation is providing very low values below 20%. This is probably because the differences in the summer peak values are relatively small.

**Table 1:** Ranked summer periods (12 most intense) from highest to lowest peak values for virtual firn core outputs, stacked  $\delta^{18}\text{O}$  profile, precipitation weighted temperature and non-weighted temperature.

| Year_VFC  | Year_stack | Year_weighted_temp | Year_temp |
|-----------|------------|--------------------|-----------|
| 2013-2014 | 2013-2014  | 2013-2014          | 1983-1984 |
| 1986-1987 | 2011-2012  | 1983-1984          | 2005-2006 |
| 2001-2002 | 2001-2002  | 2005-2006          | 1982-1983 |
| 2011-2012 | 1985-1986  | 2011-2012          | 2009-2010 |
| 1985-1986 | 2005-2006  | 2001-2002          | 1979-1980 |
| 2014-2015 | 1984-1985  | 1989-1990          | 2013-2014 |
| 2002-2003 | 1986-1987  | 2004-2005          | 2002-2003 |
| 1984-1985 | 2009-2010  | 1982-1983          | 2001-2002 |
| 1996-1997 | 2006-2007  | 1993-1994          | 1986-1987 |
| 2006-2007 | 2010-2011  | 1985-1986          | 1984-1985 |
| 1983-1984 | 1979-1980  | 1984-1985          | 1991-1992 |
| 2009-2010 | 1995-1996  | 2010-2011          | 2000-2001 |

Still, we completed our study with an analysis of the correlation of the summer peak values for virtual firn core (VFC) outputs, stacked  $\delta^{18}\text{O}$ , weighted temperature and temperature. The results show a clear and significant correlation between stacked  $\delta^{18}\text{O}$  and the three other series with a better correlation with the VFC outputs hence confirming the results discussed in the first version of the manuscript. We added this analysis in the revised version and included a new table (referred in the new text as Table 6) as:

“Another way to compare the stacked  $\delta^{18}\text{O}$  record to the different series (VFC  $\delta^{18}\text{O}$ , precipitation weighted temperature, temperature) over the summer peaks is to look at the correlation between the amplitudes of the summer peaks between the different series. The correlation is the highest ( $r=0.66$ ) when comparing the stacked  $\delta^{18}\text{O}$  record and the VFC  $\delta^{18}\text{O}$  and the lowest ( $r=0.49$ ) when comparing the stacked  $\delta^{18}\text{O}$  record and the temperature variations (Table 6). We again conclude that using the model derived VFC  $\delta^{18}\text{O}$  series improves the agreement with the stacked  $\delta^{18}\text{O}$  which is expected since it considers both the intermittency of the precipitation and the effects of source origin and air transportation on the  $\delta^{18}\text{O}$  record. Still, a correlation of  $r=0.66$  shows that part of the  $\delta^{18}\text{O}$  variations are not captured by the VFC calculations.

**Table 6: Correlation and p-value when comparing the values over summer peaks (1979-2016) between the stacked  $\delta^{18}\text{O}$  record and respectively the VFC outputs, the precipitation weighted temperature and the temperature.”**

|   | stack vs VFC | stack vs weighted T°C | stack vs T°C |
|---|--------------|-----------------------|--------------|
| r | 0.66         | 0.51                  | 0.49         |
| p | 0.0004       | 0.004                 | 0.005        |

### 3 Minor issues

Lines 214-215: Is the Figure the focus of the sentence? Or is it the science that it carries? In other words, should it be "X because of Y and Z (Figure A)" or "Figure A shows that X,Y,Z". We encourage you to make the science the focus of the sentence and not the figure.

>> corrected: "For each given site and as expected, the average  $\delta^{18}\text{O}$  and  $\delta\text{D}$  values are similar for the 3 cores (Figures 2 and S1, Table 2)"

Figure 3: to a non-expert eye, there is very little resemblance between the  $\delta^{18}\text{O}$  and nssSO<sub>4</sub> curves for different sites. The tie-points are used on Figure 2 between the repeated cores at the same site, and we understand that using such tie-points would have to be done with careful consideration for cores from different sites, but it would significantly help reading Figure 3.

>> Indeed, we were probably unclear: the nssSO<sub>4</sub> and  $\delta^{18}\text{O}$  cycles are different mostly because the  $\delta^{18}\text{O}$  cycles are smoothed by diffusion of water vapor which does not influence the nssSO<sub>4</sub> signal. What is similar between the  $\delta^{18}\text{O}$  and nssSO<sub>4</sub> is the depth of each maximum which confirms that the cycles observed both in nssSO<sub>4</sub> and  $\delta^{18}\text{O}$  can be attributed to seasonal variability. We have modified the sentence in the text to better explain this: "and despite  $\delta^{18}\text{O}$  profile being smoothed by diffusion, both water isotopes and nssSO<sub>4</sub> show concomitant maxima "

The significant increase in  $\delta^{18}\text{O}$  for D47 at 7400mm is not matched at Stop5 for example, is the Stop5 maximum offset left or right?

>> In fact, we consider each site separately in this study, so we have not tried to match features from D47 isotopic record to ones from the Stop5 record.

Lines 400-401: you write that surface smoothing increases at windy sites, but your windy site has the least near-surface isotopic signal smoothing. You explain in the following sentence (line 403) that the multiple effects listed before compete, but it may be beneficial to be extra clear that the wind effects especially do not apply here.

>> You are fully right. Wind effects were actually invoked for the snow redistribution but it was not clear in the sentence which has been rewritten as "Finally, the upper part of the firn is also very sensitive to wind pumping, snow mixing and redistribution; snow redistribution being expected to be particularly strong at windy sites"

Lines 475-476: it is very common that ensemble means completely hides extremes. You may be able to connect your finding to other examples of extremes disappearing in simple ensemble means.

>> We agree that it is common sense to lose the signature of short extreme events when averaging. It is probably so obvious that a search in the literature did not lead to numerous studies on this topic. Still, we found a few references now included in the introduction which may be useful such as those linked to stacking of an array of Greenland intermediate cores (Hörhold et al., 2023) with possible limitations of the stacking when identifying the extreme events from this global reconstruction (Gagliardi et al., 2025). However, in l. 475-476 we actually address the problems of intermittency of the precipitation and origin of air masses which are responsible for  $\delta^{18}\text{O}$  not being a perfect proxy for temperature. This is the reason why we wrote: “This is not unexpected since, as mentioned above,  $\delta^{18}\text{O}$  is not a direct indicator of temperature and cannot directly be used to identify maximum or minimum temperature levels; it is also influenced by the seasonality of the precipitation, the precipitation intermittency, the climate at the evaporative source and the trajectory of the vapor precipitated.”

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