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Final response, December 18th 2025

Final author comments

Response to RC1

Ooms et al. reconstructed the snow accumulation time series at Dome C over the last 20 years, using chemical composition and physical properties from 35 vertical profiles in a 50 m long snow trench. This is a rare, high-resolution multi-profile trench dataset from the Antarctic Plateau, providing a reference for subsequent SMB research. Their results showed that the annual SMB broadly agrees with stake farm and ERA5, exhibits meter-scale spatial variability, and reflects persistent meter-scale topographic features over multiyear timescales. I believe this work will interest researchers studying surface mass balance (SMB), stratigraphic processes, and ice core interpretation on the Antarctic Plateau. However, the manuscript needs more confirmation and clarification before acceptance.

Major comments:

1. The key result of this study is the establishment of a high-resolution dating method for snow pits. The dating is achieved by aligning the target profile with the sulfate records of old snow pits. The process of manually selecting tie points for alignment is subjective. The article does not quantify the agreement between different operators.

We fully recognize the existence of operator dependent variability in the construction of the trench age model. The detailed alignment protocol (Section 2.2 line 120) is meant to guide the operator as much as possible and to reduce subjective choices.

We had performed replicability tests with other users which have been included in the revised version of the manuscript, in section A4, figure A4b. The results are very similar to sensitivity tests with the same operator using a different reference profile (P48) as a target for the alignment. The resulting accumulation estimate falls within the 20% uncertainty envelope of the global dating uncertainty.

In the main text, we have added the following to Section 4.2, line 352-372:

> "First, the reference profile can itself be subject to hiatus. In section 3.4, we found that annual accumulation hiatus occurs in 5% of the profiles on average, and 6 monthly hiatus about 40% of the time. Comparing the sulfate records in five 100 m long ice cores, Gautier et al. (2016) found a higher probability of 30% of missing one single volcanic event in a single core, which is consistent considering the peak deposition takes place over less than a year (Cole-Dai et al. 1999). Accumulation hiatus on the reference profile lead to missing minima or maxima in its sulfate record. Such a missing feature would be present in 95% of the trench profiles and inaccurately aligned on the reference. However, with our approach, these effects are automatically corrected for on average. For instance, if year 2015 was missing in the reference, but years 2014 and 2016 were clearly identified, the accumulation for 2014-2015 and 2015-2016 may be inaccurate, but the accumulation of the two years period 2014 2016 would still be correct. Sensitivity tests on the choice of reference profile, using P48 instead of P0 as an alignment target, indeed show such variations, changing the intensity or phasing of inter-annual accumulation, with a standard deviation of 22% (Sect. A4). Similarly, user induced variability when performing the alignment with the same reference P0, which will inevitably impact assignment of ambiguous features, is contained within the global uncertainties of the accumulation reconstruction, with a 20% standard

deviation on inter-annual accumulation values. We have considered the alternative of using a stacked profile as the reference to get a more representative average sulfate profile, but precisely because of stratigraphic noise, this produces a very smooth profile (Appendix, Figure A3.a) on which it becomes impossible to recognize any feature at the inter-annual scale."

2. I believe that all readers will want to know why this study uses a single profile P0 as the reference to align all other sections in the entire 50-meter trench.

(1) If P0 itself is abnormal, then this anomaly may be passed on to all other profiles, causing a deviation in the dating of the entire [trench]. Münch et al. (2016, https://doi.org/10.5194/cp-12-1565-2016) pointed out it is necessary to average multiple ice cores or snow pits to extract climate signals and filter out stratigraphic noise.

The choice of using a raw, non-stacked reference profile is indeed a very complicated topic, and is a deliberate choice given the constraints of our alignment and dating method. Figure A3.a in the appendix illustrates the fact that averaging out profiles without alignment produces a very smooth profile. It becomes difficult to compare this stack with the individual profiles of the trench. Our goal here was to trace isochrones in the trench by exceptional markers that leave non uniform signature in the different profiles, hence the deliberate choice to work with a raw profile as a reference, which will contain some, but not all, of these signatures.

The second step of dating the reference profile is exactly meant to reduce the bias caused by anomalies of the reference profile, since periods with more or less accumulation, and accumulation hiatus, are squeeze and stretched accordingly. The method is limited by the little amount of historical snow pits available, and comes with large uncertainties, but it is our best attempt to reduce the impact of using a raw imperfect reference profile.

These questions are already addressed in the discussion section of the manuscript (Section 4.2). We added a reference to this section when introducing the reference profile in the method to guide the reader (line 124):

> "We chose to use the single raw profile P0 as the reference profile because of its highest resolution (1.5 cm instead of 3 cm) and the presence of several sulfate features that are suitable as an alignment target. Despite these advantages, we observed that P0 features a lower SSA in its top snow than other profiles, and that on some profiles up to 3 cm of upper snow could not be matched to this reference. We interpret these facts as a missing snow layer at the top of P0. To solve this issue, we completed the 3 cm upper part of P0 with the mean surface
> snow properties in the 3 cm upper part of all profiles over the trench. This permits us to align all profiles with respect to the reference up to the surface (Supplements, Sect. S2.1). Similarly to this missing snow layer at the surface, in any single raw profile, there will be missing layers that can hinder alignment with other profiles. Yet, we chose to stick with aligning against a single reference profile because a pre-averaged profile is too smooth to obtain confidence in identifying successive peaks. We realized sensitivity tests about the impact of aligning on a single reference profile, as described in the Discussion section 4.2."

In the discussion section, we included the following additions (line 353):

> [last sentence in dark red in the text addition mentioned in point 1.]

(2) When P0 is compared with snow pits that may be hundreds of meters or even farther away, the basic assumption is that the sulfate signal is stable over a considerable spatial scale. However, an important finding of this study is that there is a significant meter-scale spatial variability in snow accumulation. Gautier et al. (2016, https://doi.org/10.5194/cp-12-103-2016) indicates that even ice cores just a few meters apart may have missing records of volcanic events.

We decided to compare raw profiles, keeping in mind that feature identification will fail for a certain fraction of snow pits due to the anomalies in the reference. This actually lead to only 13 snow pit profiles being used in the end because we could not identify the peaks corresponding to the upper 15 cm of the snow pit profile with the reference profile. Here, an important point comes from the fact that we do not need to include all the events, but enough to find common feature to add tie points to our reference profile.

However the point was not made explicitly that this 41% (9/22) discarding rate can be attributed to hiatus, inline with the observations of Gautier et al. They mention that, for a selection of two cores, there is a 60% chance of observing a volcanic peak in the two cores. Consequently, if we transpose our method, i.e. using one core to date a reference core, this attribution would fail for 40% of peaks. The numbers are not directly comparable, as Gautier uses volcanic peaks above +2 sigma, while our features are in the +1-2 sigma range, but the probabilities are of the same order.

Consequently, we have added the explicit reference to the Gautier study on line 140:

> "From the set of 22 snow pits dug at Dome C in the last 20 years (Gautier et al., 2016; Traversi et al., 2009; Caiazzo et al., 2021) we selected 13 that could be aligned with the reference profile of the trench (the others showed no remarkable feature in common with the reference in their upper 15 cm section). This 41% discarding ratio is comparable to the observations of Gautier et al. 2016, where they showed that a volcanic peak only has a 60% chance of being present in any two given cores."

Additionally, just like in the case of trench inter-profile matching, a stacked reference profile would be difficult with the historical snow pits, whether or not they are replicated themselves. For instance, the VolSol cores from the study of Gautier 2016 are only replicated beyond 5 m depth, so that only one profile can be used for our applications. Since our goal here was to identify with the highest level of certainty how to align our trench with historical profiles, it is expected that there will be hiatuses both in our reference profile and in the other profile we're using for dating.
We acknowledge that this point was not made clearly and added the following paragraph in section 4.2 to clarify our approach:

> "We have chosen to work with a reference profile as a target for the alignment of all the profiles in the trench, instead of pair by pair alignment. Beyond the obvious gain in time in the alignment (n vs $n(n-1)/2$ pairs to align, where $n = 35$ is the number of profiles) and dating (only one profile to date), we also gain in coherence as all features recognizable on the reference were matched precisely.
> For the same reason, we chose to work with a single raw reference profile instead of a trench stack. Indeed, the older snow pits themselves are mostly single, non-replicated profiles. Stacking the trench unaligned profile produces a smooth profile (Appendix, Fig A3.a) which becomes unpractical for peak matching with older snow pits. By using a single raw profile, we were unable to match 9 out of the 22 snow pits used for dating (due to the hiatus or gaps in the reference and in the snow pits themselves), but we keep a high confidence for successful identification of the remaining 13 snow pits. This 60% ratio is consistent with the two ice-core volcanic peak identification rate of Gautier et al 2016. Reference profile

alignment however comes with some caveats, and the dating of the reference remains the main source of uncertainty."

(3) The manuscript indicates in the method section that due to too little snow was available to measure chemical elements of the P0 sample, it was mixed with a high-resolution sample from another profile. The authors did not explain the specific method of mixing, and this operation may artificially change the chemical signal of the reference profile.

We acknowledge that the method of mixing was not clearly described in the method. The fact that it changes little to the composition of the sulfate profile is supported by the decorrelation length of 1 m. We have added the following sentence in the relevant paragraph:

> At the localization of the first profile (P0), we actually realized four profiles (P-0.3, 30 cm before the reference profile, P0.1, 10 cm after the reference profile, and P0.3, 30 cm after the reference profile. Because the decorrelation length is roughly 1m (Appendix, Fig. A6), it is possible to mix snow profiles 10cm and 30cm away, respectively. Here, some of the snow samples of P0 did not have sufficient amount of snow to realize the entire span of chemical measurements (anions and cations), so in this case, we used extra snow from P-0.3 to complement. "

So I am skeptical about using a single and mixed profile P0 as the reference for the age alignment of the 50 m trench.

Let us summarize our answer to the three points above: We are aware that a single reference profile is subject to stratigraphic noise. Our point was not to have a perfect reference profile, but something easy to work with and that proves to work very well both for inter-profile alignment (1) and for older snow pit alignment (2) when common features are recognized in matching profiles (~60% of the time). Furthermore, we know by the observed spatial decorrelation length of sulfate that the fact that P0 is a mix of two profile has little impact on the signal.

The uncertainties associated to using a raw reference profile as a target for the alignment are already presented in the manuscript (Section 4.2, line 360). But we had omitted to state clearly in the main text that this uncertainty by itself contains the variations seen in sensitivity tests, when using the profile P48 or when alignment is performed by another user (Section A4, figure A3b).

Therefore we have added the following to Section 4.2, line 352-372:

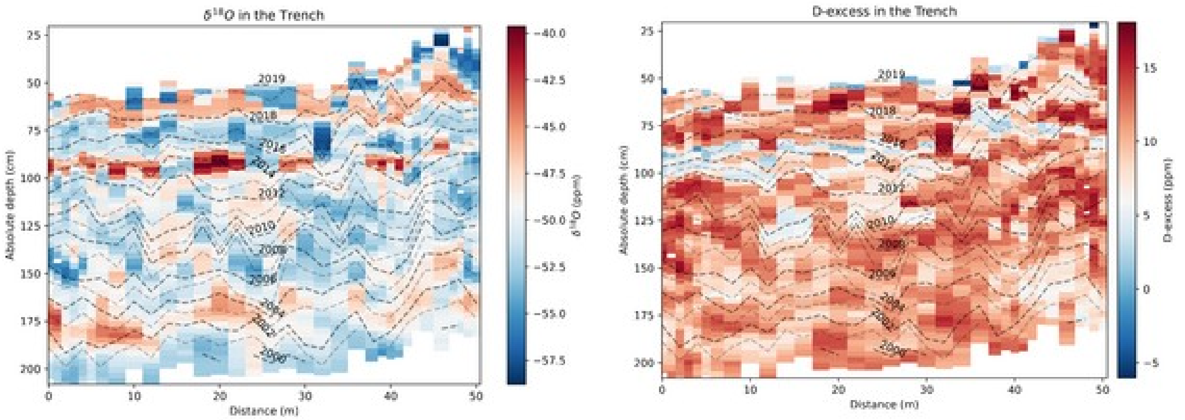
[Same section as the updated section mentioned in the first point, concerning user dependent variability].

3. The authors take the mid-2015 isochrone as an example to validate past topography. Using imperfect individual cases to prove isochrony over a twenty-year period is too weak. I suggest adding more isochrone for verification rather than relying on a single “perfect case”. For example, Sinnl et al. (2022, https://doi.org/10.5194/cp-18-1125-2022) emphasize the use of multiple time periods and various indicators to validate age model.

We agree that it would be better to have more isochrones to compare the trench alignment with. Unfortunately, we did not identify any other physical isochrones that could have been linked with a time marker in the rest of the trench.

Another indication that gives us confidence in the age model comes from water isotopes analysis, which are part of a manuscript being written, in which it is possible to identify two layers in 2014 and 2015 which can be recognized with the surface snow isotopic composition in d18O and d-excess. We see a strongly enriched d18O layer which our age model attributes to 2014, and a depleted d-excess layer attributed to 2015. Comparable modification in the isotopic composition of surface snow for these periods have been measured and explained by the influence of sublimation flux and in firn vapor exchange for these two exceptional years (Casado et al. 2021). Unfortunately, this manuscript hasn't been submitted yet, so it is difficult to include this in the present study. More importantly, because these two other dates are so close to the one we were able to identify in Mid-2015, it is not adding any constrain on the dating uncertainty.

In addition, an enriched d18O layer in 2002-2003 provides another potential stratigraphic marker, which is not covered by surface snow observations, but which we are in the process of modeling with isotope enabled GCM models. The isotope stratigraphy is outside of the scope of the present study, but it will come as an independent validation of the chemical and physical stratigraphy.



(a) (b)

Additional Figure 1: stable water isotopic composition of the trench, with d18O (left) and d-excess (right)

Instead, the validation of the age model at +/- 1 year is based on the little divergence of snow pit age models (obtained by linear interpolation to the Pinatubo horizon, transferred to the reference profile) as shown in figure 5. All models converge toward early 1999 for 150 cm depth. We recognize that this was not stressed enough in the manuscript and added the following sentence for emphasis on line 216:

> The next six dating points were obtained by alignment of the reference profile to snow pits dug in the Dome C area over the past 20 years. The depth uncertainty of dating points (horizontal error bars in Fig. 5) leads to an uncertainty in the snow age at a given reference depth (vertical error bars) ranging from ± 0.5 years for the January 2019 tie point to ± 1.5 years for the January 2012 tie point. The average limited spread of the individual age models (doted colored lines in Fig. 5) has a mean standard deviation of 0.7 years between models and gives confidence in the accuracy of the trench age model at ± 1 years.
>
> The last tie point at the bottom of the reference profile is obtained from the mean over the six linear age models transferred from the aligned snow pits reaching the Pinatubo horizon (Table 1). The standard deviation among the age models gives an error estimate. They indicate that the snow layer buried at 151.5 cm depth was deposited in early 1999 ± 1 years. From the surface snow deposited in June 2019, the trench thus archives about 21 years of snow accumulation, with a mean elevation increase of 7.3 cm yr^{-1} .

We also want to emphasize that, while validating the stratigraphy at the inter-annual scale is important, it is something which will be out of reach for many ice-core applications where no previous ice-core are available. The more important point we want to make is that a linear age model (gray line in Figure 5) gives annual accumulation time series (Figure A3(b) dashed black) that sits within the dating uncertainty (Figure A3.b red envelope). In other words, we argue that linear age model spanning a few decades (such as between two volcanic horizon) coupled with the alignment of replicate cores will give a good estimate of inter-annual accumulation.

To summarize, while we are not able to provide more isochrone for verification, the validation given by the 2015 isochrone was just given as a striking example showing that linear interpolation of the age model on a 5 year period can fall very close to a given time marker.

Minor comments:

1. The horizontal decorrelation length of the sulfate signal is 1.26 m in the aligned trench in L204. However, in Figure A6 of the appendix, the legend shows the aligned decorrelation length as "1.40 m".

Thank you for pointing this out. This has been fixed to "1.40m" in the text, as in the figure.

2. Only 6 dating points were kept for the age model in L144, but table 1 lists seven different records of snow pits/ice cores.

The table lists explicitly the two snow pits for 2017 because they both have a linear age model based on volcanic horizon, which are useful to validate the age model. Naming them makes Figure 5 more readable. The caption of the table was updated as follow to remove the ambiguity:

> "Summary information for the snow pits and ice cores used in the dating. Last column indicates whether the Pinatubo volcanic horizon of 1992 can be identified in the sulfate signal, allowing us to compute a linear age model for that snow pit. Only the snow pits whose upper tie-points were used for dating are shown (non duplicates, 6 out of 13 snow pits), except for Traversi2017, which is redundant, but whose linear volcanic age model is shown alongside other snow pits in Fig. 5 for validation."

L24: "offsetted" should be "offset"

This has been fixed.

L38/57: unify spelling of "snow pit / snow pit".

All instances have been spelled as "snow pit"

L73: Could you explain the direction of the dominant wind?
The paragraph has been updated to:

> "The trench was perpendicular to the main orientation of the wind and the sampling was carried out on the wall face sheltered from the wind, which is southerly on average at Dome C (Genthon et al., 2021)"

L78: remove height .
This has been fixed.

L81: "was" should be "were".
This has been fixed.

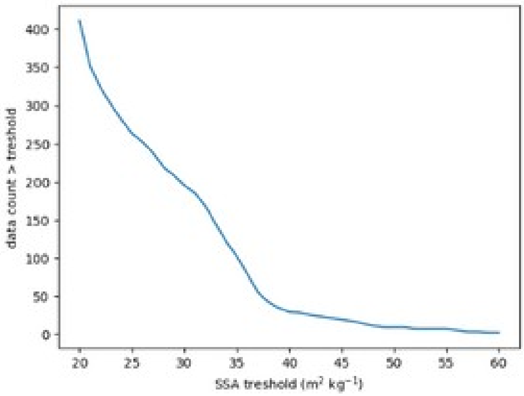
Figure 1 caption it is more appropriate to change the "+" to "follow by".
This has been fixed.

Figure 1 caption: “1.5cm” should be “1.5 cm”.
This has been fixed.

L116: Could you tell me why you chose SSA > 40 as the threshold?

This value is based on observations of Libois et al. 2015 at Dome C over the course of two summer seasons (2012-2013,2013-2014) where they show that precipitation events always have a signature in increase of surface snow SSA to values higher than 40 m^2/kg, and that when there is no precipitation, surface snow SSA will decrease to values under 40 m^2/kg over the course of only several days. Therefore we use this threshold to identify snow necessarily linked to recent precipitation events.

To further strengthen this choice of threshold value, we have now added a sensitivity test to the manuscript (Supplement S1). We reproduce the figure here for convenience:



The following sentence was added to the text:

> As the trench was sampled at the beginning of summer, before significant metamorphism had occurred (Picard et al., 2012), we used the high SSA values (> 40 m2 kg−1) of recently deposited winter snow (Libois et al., 2015) to match the topmost portions of the profiles and to detect hiatus in snow deposition. We have performed a sensitivity test to ensure that this was also an appropriate threshold for the trench dataset. We counted all SSA values in the trench dataset (26 evenly distributed profiles) above a certain threshold, for threshold values ranging from 20 to 60. We see a clear transition around 38-42 m2 kg−1, where the data point count increases sharply under 38 m2 kg−1, indicating a longer persistence of such SSA values during grain coarsening. This confirms that 40 m2 kg−1 is a sweet spot to identify fresher snow. The details and corresponding figures are provided in supplement S1. The rest of the profile is aligned by matching sulfate peaks, first with the largest peaks and then refined with sub-features.

L175: Does this indicate that the previous use of 320 as the density value of Dome C is too high?
This is indeed what our accumulation reconstruction seems to indicate. The density analysis carried out on the trench data (Figure A2) shows the same bias: a linear interpolation on 1.5m of snow at 3 cm resolution, as was done by Genthon et al. 2016 on 1m of snow with 10 cm resolution, gives a

surface density of 320 kg/m2. The observed density however in the upper section of the trench is closer to 300 kg/m2. This could be explained by grain coarsening due to snow metamorphism during the first year of residency. Consequently, fresh snow readings on the snow stakes should be converted to mass using a lower density.

L487: “alignment” should be “alignment”
This has been fixed.