

**RC1**

Major comments:

1. This study selected 13 cases from 22 previous snow pits and noted that the remaining pits showed no common feature in the upper 15 cm. Although this exclusion rate may appear statistically reasonable, please clarify whether this filtering method could bias the retained samples toward being convergent, thereby artificially reducing the uncertainty dating of the P0.

Once a snowpits is selected based on the recognizable features in the upper part, the rest of the snowpit is then aligned with the reference down to the bottom of the trench. This alignment in the deeper section is totally free. Here there may be sections without tie-points due to lack of recognizable feature, and the age is then linearly interpolated between tie points. The fact that the resulting age scales for the lower parts of the reference profile are convergent with the reference age scale constructed using only upper tie points is what we consider as a validation of the age model. This we think is not influenced by the selection criteria for the upper section of the snowpit. While we could enlarge this test by considering alternative time scales of all snowpits including those without an upper tie-point match, we might end up artificially increasing the spread in age models, as we are dependent of this upper tie point associated with the sampling dates. This is why we have limited ourselves to obvious peak matching where the snowpit age is the most certain.

2. This study treat the date of sampling as the absolute tie points. However, if the sampled surfaces experienced wind erosion or snow redistribution, the dating could be biased. Please clarify how ensure that the sampling time indeed corresponds to the age of the stratigraphic top at each pit, thereby demonstrating the reliability of these dating anchors.

To ensure that selected snowpits used for dating show no erosion in their upper part, one would need to compare replicate snowpits on a given date to find which one has fresher snow in the upper section, for example using SSA measurements like we did to construct a reference profile representative of latest snowfalls. Unfortunately, the snowpits we use are not replicated and don't have such physical properties data. The deposition date for the upper samples therefore comes with a moderate time uncertainty which we estimated to be less than a year: we observe only 20% of yearly accumulation hiatus according to previous studies on surface erosion at Dome C (Picard 2019), and 10% on average according to our study. Instead of implementing this as an uncertainty on the age, we focused on assigning an error bar on the reference depth of the top tie-point assignment based on the accuracy of the peak matching, which takes into account the possible lack of snow in the upper section of the snowpit (inaccurate peak matching can be due to a small amount of erosion). This 5-10 cm error bar translates to a +/- 1-1.5 year uncertainty on the tie-point assignment (Figure 5), which would contain the smaller uncertainty on the age of the upper snow (very likely less than 1 year). To summarize, we consider that our error bar accounts for both uncertainty in the absolute age of the upper snow and peak matching on the reference profile. We added the following sentences are rearranged the paragraph starting line 156 in section 2.3 to clarify this point;

To construct the age model of the reference profile, we chose to only keep the top of each snow pit as an absolute tie point, as it is the date known with the highest confidence. ~~The~~

alignment was realized for the 13 profiles, giving 13 dating points on the reference profile. However only six dating points were kept for the age model due to several snow pits being dug within the same month, resulting in duplicate tie points (Table \ref{table:snow\_pits}). Despite this accuracy in sampling date, we take into account the fact that the top of each snowpit might have been subject to erosion, as up to 20% of profiles might have snow missing for a given year (Genthon et al. 2016, Picard et al., 2019). We do this as we deal with peak matching uncertainty: error bars were assigned to the tie points depth on the reference profile. The error ranges between 5 and 10 \unit{cm} depending on the quality of the match in sulfate signals. Small errors (5 \unit{cm}) are assigned to the tie points just before a large and recognizable feature such as the broad sulfate peak in the 90-120 \unit{cm} depth (December 2005), but conservative errors (10 \unit{cm}) are assigned to tie points in a series of similar peaks, or in a section with little overlap, such as at depth 140-150 \unit{cm}, corresponding to December 2000. This later translates to an uncertainty on the age with contains the uncertainty on the age of snow in the upper section of the snowpit (very likely less than a year). The alignment was realized for the 13 profiles, giving 13 dating points on the reference profile. However only six dating points were kept for the age model due to several snow pits being dug within the same month, resulting in duplicate tie points (Table \ref{table:snow\_pits}). For a comprehensive use of all the data, we ensured that the error bars associated with the dating capture the variability observed across these duplicates, which represents the fact that snow accumulation can be uneven in the upper sections of profiles of a given year.

3. The manuscript suggests that meter-scale surface topographic features can persist for multiple years. Mott et al. (2018, <https://doi.org/10.3389/feart.2018.00197>) indicates that wind-driven snow transport is generally expected to infill and smooth surface depressions. Could the authors provide a more in-depth explanation of why such micro-topography can be maintained over long timescales under the specific environment conditions at Dome C?

Our analysis is not contradictory with classical results in snow cover dynamics; we do observe a very slight tendency for depression to accumulate more snow (significant positive slope of 15%/yr at the 2 meter scale, Figure 8). The difference however with the cases in the review of Mott et al. is that they describe mountainous environment where snow accumulation rates are much higher (up to several meters per year) than that of Dome C (8 cm /year). At Dome C, this is of the same order as the micro-topographic features that are 10-30 cm high. The mean wind of 4m/s and frequent wind gusts above 10m/s (Genthon 2021) can cause the light snow to be removed and will limit the tendency for snow to accumulate in depressions.

We acknowledge that our conclusions might seem contradictory to the review in Mott et al., but with the elements above it is clear that these dynamics can be very different in a mountainous setting or in one such as Dome C. We added the following sentence at the end of section 3.5 (line 327) to reflect on these open questions:

"The low determination coefficient also shows that there is no consistent relationship between topography and accumulation. This supports the hypothesis of a strong multi-year persistence of the meter-scale topography. While these findings might seem contradictory with general observations in snow accumulation dynamics, where wind-driven snow transport is generally expected to smooth surface topography (Mott et al. 2013), one must recall the unique setting of Dome C, where the very low accumulation rate (8 cm / yr) is of the same order as the micro-topographic features 10 - 20 cm high. This coupled with frequent wind speeds above 10m/s (Genthon 2021) can cause the light snow to be removed and will limit the tendency for snow to accumulate in depressions.

Minor comments:

L93: delete “does”

This has been fixed.

L125: “envenly” should be “evenly”

This has been fixed.

L242: “Figure 4a” should be “Figure 4c”

This has been fixed.

L328: Could you please explain how 8% was calculated?

The 8 % /yr was a typo (confusion with the determination coefficient of 0.08). The slope value is 15%/year, as indicated in Figure 8 and in the rest of the paragraph. This has now been fixed to 15 % / yr in the text.

While re-reading this paragraph, we thought we should also better introduce the units of the slope value, which might add confusion as to how it was calculated.

The slope of the linear regression of accumulation rate against  $\Delta z$  in units of (cm / yr) / cm is expressed as a fraction in % / yr expressing how much of the topography is being smoothed per year on average.

We have now added a sentence to introduce the change of units to the reader;

While the highest accumulation amounts ( $> 10$  cm / year) are more often associated to holes as expected, the bulk of the distribution is very spread out. **We perform a linear regression of snow accumulation against  $\Delta z$  and express the slope in filling fraction of the holes ( (cm / yr) / cm = % / year).** This gives a positive slope of 8 % / yr, and a determination coefficient of  $r^2 = 0.08$  (p-value  $< 0.05$ ).

Figure 5 label: Why was it written as 12-2018 instead of 01-2019?

This was a typo in the table and has now been fixed.