

Key:

Community & referee comment

Author response

The paper by Choi et al. presents particulate mineral dust analyses (mass concentration, mass size distribution and some mineralogical investigations) on a new shallow ice core from the Allan Hills Blue ice area. The authors recognize that the dust record is influenced by

- temporal changes in the atmospheric composition, concentration and size distribution of mineral dust
- a potential interglacial bias of the record
- postdepositional alterations by chemical in situ weathering and neoformation of minerals in the ice
- entrainment of subglacial material into the ice

They discuss the data with respect to these issues and in how far such dust analyses could be used to separate atmospheric changes from post-depositional changes occurring in this extremely thinned and folded glaciological regime.

Overall, the results are sound, but I struggled with reading the paper, as the authors did not yet attempt a strict separation of samples affected by englacial entrainment and those sample where this can be excluded. Because of this, the discussion remains still on a descriptive level. Such a conservative triage of samples affected by only one of these factors could have made the structure of the paper more stringent and helped to separate the discussion of the issues above. At the moment, the reader has to think about the potential effects of all processes at the same time (see my more detailed comments below), which makes it difficult to distill the main take home messages.

Thanks for this constructive comment. To address it, we plan to change the formatting of the manuscript to begin with a detailed description of a multi-proxy screening process by which samples will be distinguished into the following categories: 'glacial', 'interglacial', 'unresolved', or 'flagged', with the last category being atypical for post-MPT glacial/interglacial variability. We will then restructure the discussion into sections that allow us to interpret the dust record in samples by category and provide several interpretations regarding the driving mechanism behind the flagged category including basal processes and local aeolian input (and related paleoclimate implications).

The authors also add another section on the association of dust concentrations and elevated CO₂ levels in the ice as well as the potential of carbonate minerals in contributing to elevated CO₂ levels in Allan Hills Blue ice. This could be an important topic, but the short discussion on these issue does not add quantitative insight into this process yet that goes beyond what has

been described in the work of Marks-Peterson. I leave it to the editor and authors to decide, whether this section should remain part of this paper or warrants its own, more quantitative paper.

Thank you for this comment. We have carefully considered what the aim and scope of this work will be and based on this comment (and others), we have chosen to diminish the discussion of the observed relationship between dust and CO₂, and instead have decided to frame this work as developing methods for screening blue ice as reliable paleoclimate records using the framework described in our revision, which will utilize a multi-proxy screening process to categorize samples based on their physical dust characteristics.

Similarly, the introduction could benefit from a more stringent, linear structure to set the scene for the paper. At the moment one has the feeling that the introduction starts several times with a different aspect in the focus to motivate the paper.

Thank you for this useful comment. In response to this and to others brought up by other referees, we have taken steps to streamline the introduction to frame this work as more of a method to screen samples from blue ice areas for glacial/interglacial aeolian dust delivery, potential input from basal contamination, and the implications for the preservation of reliable paleoclimate records. This includes reducing focus on additional motivations not directly related to central scientific questions such as in lines 54 - 69 discussing iron fertilization and Southern Ocean which will be made more concise/restructured as a part of this reframing as well.

In summary, I regard this paper as a relevant scientific contribution to the field, but it needs some major revisions to get the messages across more clearly.

Thank you for your constructive feedback, which has greatly improved the manuscript quality. Specific replies to comments are below.

Specific comments:

abstract and throughout the text: The authors recognize (as previous papers about the Allan Hills blue ice cores) that the record appears to be biased towards interglacials. They make low glacial accumulation rates (hence low glacial layer thicknesses) responsible for this. However, there is also glaciological evidence from deep ice cores that glacial (dust-rich) ice thins faster than clean interglacial ice (Parrenin et al., CP 2007), especially in deep, highly thinned strata. This explains at least in part the underrepresentation of glacial strata or in extreme cases could even explain a complete loss of glacial layers. Together with the formation of z-folds and finally a complete mixing of layers, this can lead to a homogeneous ice composition as found in the bottom 5 m of the presented data that is biased towards interglacials. Above 155 m still hard cuts in the dust concentration and size distribution exist, which show that there is still layering in the ice, however, it is not clearly discussed which of the samples are affected and whether these layers are mainly intrusions of deeper ice with englacial entrainment of particles (I think they are).

Thanks for this comment. We have added a detailed description of a multi-proxy screening process by which samples will be distinguished into the following categories: ‘glacial’, ‘interglacial’, ‘unresolved’, or ‘flagged’, with the last category being atypical for post-MPT glacial/interglacial variability. We also plan to add more text leaning on published literature for addressing why the ALHIC1901 record is biased towards interglacials specifically referencing evidence from other deep ice cores such as Dome C and TALDICE as suggested in the referee comment. We will then restructure the discussion into sections that allow us to interpret the dust record in samples by category and provide several interpretations regarding the driving mechanism behind the flagged category including basal processes and local aeolian input (and related paleoclimate implications).

The revised figure showing the relationship between dust mass concentration and particle diameter is shown below. Above 150 m, there are only 3 isolated samples that have been flagged, and from 155-150 m there are 4 samples that have flagged. These flagged samples and the possible mechanisms behind these trends including basal entrainment and local aeolian delivery will be discussed in the revised manuscript.

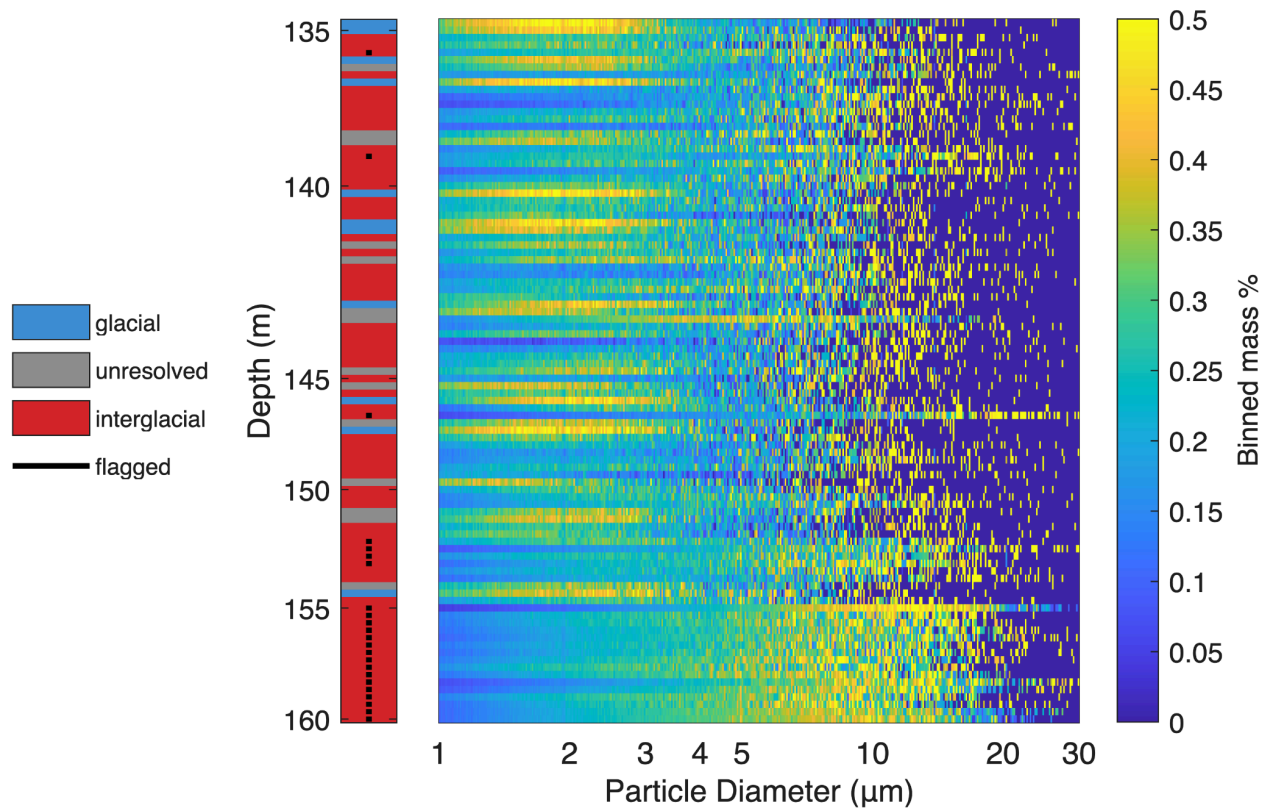


Figure 4. Particle size distribution variability with depth with screening class alongside. Each row represents a discrete subsample of ice from ALHIC1901 which has been measured for mass concentration and size distribution. The particle mass for each diameter range is represented by

a pixel, which has been normalized by bin interval (natural log of the bin width) and the cumulative mass (1–30 μm diameter) for each respective sample. The pixels are colored according to the color bar and sum to 100% when integrated across a sample row. Note the depth axis is not to scale and is more representative of the sampling frequency. Screening classes for each sample are shown for glacial, unresolved, and interglacial with black ticks marking flagged samples.

abstract line 32-33: Here it is stated that the dust concentrations may be smoothed by thinning. This needs some more explanation. As the particulate dust is not subject to diffusion, a more homogenous concentration record is likely only a result of the limited resolution of the data not being able to resolve the increased thinning. Note, however, that the crystal size, which is influenced by the presence of mineral dust particles sets a natural scale of variability in the impurity records.

Thanks for this comment. We have edited the abstract text to be more specific: "The dust concentrations may also be further smoothed due to loss of sample resolution due to ice thinning."

abstract line 34: write "in situ mineral weathering in the ice matrix"

We have removed discussions of mineral weathering in the paper and from the abstract.

Introduction: The first paragraph motivates the paper by helping to understand the MPT, although the paper doesn't really discuss the MPT, the second paragraph motivates ice core dust studies because of iron fertilization, the third paragraph starts again by listing what else we can learn from ice cores, the fourth paragraph is somewhat redundant again. This needs some streamlining in my opinion.

Thanks for this constructive comment, which we have found very helpful in reframing the introduction to instead focus on the development of a framework and methodology for screening ice and dust records developed from blue ice areas for potential input from basal contamination and implications for the preservation of reliable paleoclimate records. Any discussion of the MPT is focused on comparing expected patterns of dust delivery for post- and pre-MPT ice.

line 62-63: In the sentence before, the case is made that the dust entering the ocean from above undergoes chemical alteration in the water column. This chemical alteration is crucial for its biological availability, so the pristine ice core record per se will not help to understand how bioavailable the dust really is. I would delete this sentence.

Much of the iron that enters the ocean undergoes processing that could also make it less biologically available. For example, rapid oxidation of Fe(II) to Fe(III) can make the Fe less accessible. However, organic complexation can keep Fe in its bioavailable state (Gledhill and Buck, 2018). The changes to the chemical composition and speciation of Fe preserved in the

marine sediment record therefore may not provide a straightforward record of bioavailable Fe supplied to the ocean, as this Fe may have undergone chemical changes following deposition in the surface ocean. Regardless, we have chosen to remove this from the introduction as we are reframing the introduction to focus more on the development of a framework and methodology for screening ice and dust records from blue ice areas for input from basal contamination.

Source:

Gledhill, M., & Buck, K. N. (2012). The organic complexation of iron in the marine environment: a review. *Frontiers in microbiology*, 3, 69. <https://doi.org/10.3389/fmicb.2012.00069>

line 89: "strengthened transport of local Antarctic material and/or diminished..."

Thanks for this edit, changed.

Line 95: cite also Wolff et al., CP 2022 for the use of dust as synchronization tool

Reference is now included.

Line 103: "However, with increasing age..."

Thanks for this edit, changed.

Line 119-120: see my comment above on differential thinning of glacial and interglacial ice. Here also high ablation rates are mentioned. This needs some more explanation. The ice found in the Allan Hills ice core comes somewhere from upstream where there was a positive net accumulation (otherwise no signal). I can't follow the ablation argument here. Maybe, it is just a wording issue and you mean the effect on net mass balance between annual accumulation and annual ablation at the site of deposition?

Net accumulation rates at the site of deposition may not have been consistent throughout time resulting in the lack of signal preservation during presumably drier glacial periods. An additional line clarifying this will be added to this summary of the processes impacting climate signal preservation in this ice core, and we have removed mention of ablation.

line 133: It would be helpful for the outsider if it were mentioned where (roughly) the snow was initially deposited relative to the current drill site, what the accumulation and ablation conditions are at that site of deposition and how the ice flowed into the current ice core location.

While the exact deposition site is undetermined, the local ice divide is estimated to be approximately 15 km upstream (Kerhl et al., 2018. Accumulation rate variability is not available at the divide.

line 168: "...and mass size distribution..." it is important to be exact here

Particle size bins are specified further in line 175.

line 190: here it says, you used the "midpoint of the bins". What do you mean exactly by midpoint? As these bins are equidistantly defined on a logarithmic scale, it should be the geometric mean of the bin (not the arithmetic midpoint).

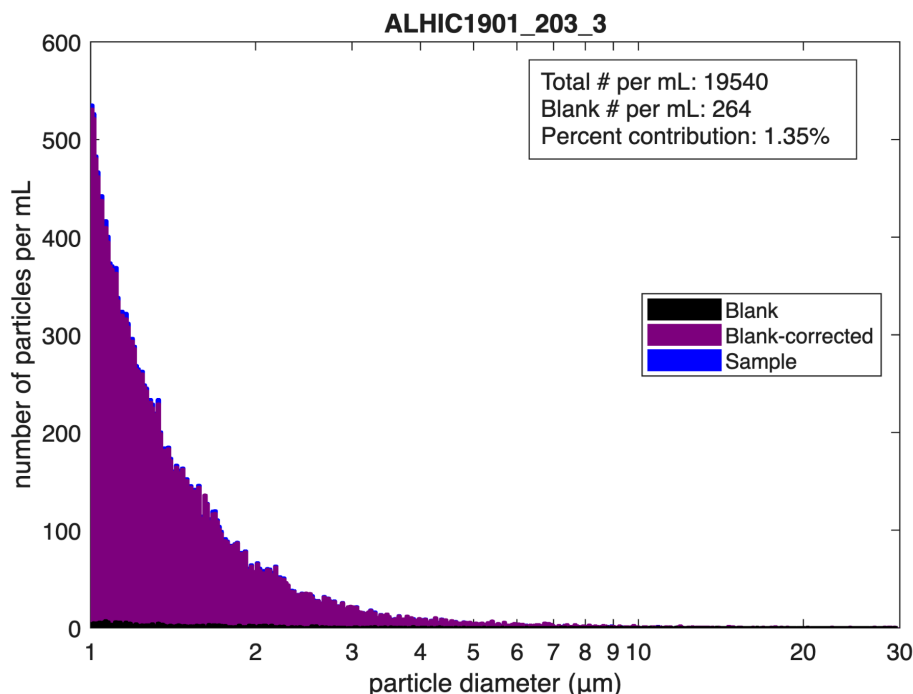
Although bins themselves from 1 to 30 μm are defined on a logarithmic scale (i.e. the range of each bin increases with larger particle size) the midpoint of each bin used for particle mass calculations is the arithmetic midpoint.

line 205: "...artificially lower measured dust concentrations and a shift of the measured size distribution to smaller particles."

We will append the additional qualifier to this sentence.

line 214-217: It says that the background count for each bin (blank) was subtracted and that this correction was less than 2% of the total particle count. I assume that this blank count is different for the different bins. Please provide the number of the maximum and minimum contribution to the individual bin counts and the bin mass (a few blank counts can completely screw up the mass in the large particle bins) and tell the reader, which particles are more affected by the blank (small one or big ones).

Below is an example of particle count distribution for a given representative sample and included is the same for the blank and the sample after blank corrected. The blank contribution is negligible.



SEM was only performed for a total of 4 individual samples. These samples were selected to include a 'pristine' sample above 155 m and several randomly selected samples from regions where the particle mass concentration was elevated and regions where normal mass concentrations typical of aeolian transport were observed. This has been clarified in the methods section.

line 230: Please mention the EDS spot size here (typically 1-2 microns), as this limits which particles can be measured using EDS.

The analyses were measured using an accelerating voltage of 15 kV. The exact spot size is dependent on the density of the material and the accelerating voltage, rather than being a preset quantity. We can estimate this following Goldstein et al. (2018, Eq. 4.12). At 15 Kv, this gives 2.2-2.5 micron for the silicate and carbonate phases analyzed ($\rho = 2.6\text{-}2.8 \text{ g cm}^{-3}$) and 1.1 micron for the denser Fe-Ti oxides. Please see example equation below:

According to Goldstein et al. (2018, Eq. 4.12):

$$R = 27.6 (A / Z^{0.89} \rho) [E_0^{1.67} - E_c^{1.67}]$$

where

- R is the X-ray production range (nm)
- A is the mean atomic mass (g/mol)
- Z is the mean atomic number
- ρ is the density (g cm^{-3})
- E_0 is the incident beam energy (15 kV)
- E_c is the critical excitation energy of the measured line (keV)

For example (calcite, Ca K α),

CaCO₃: $\rho = 2.71 \text{ g cm}^{-3}$; mass weighted $A = 25.16$, $Z = 12.57$

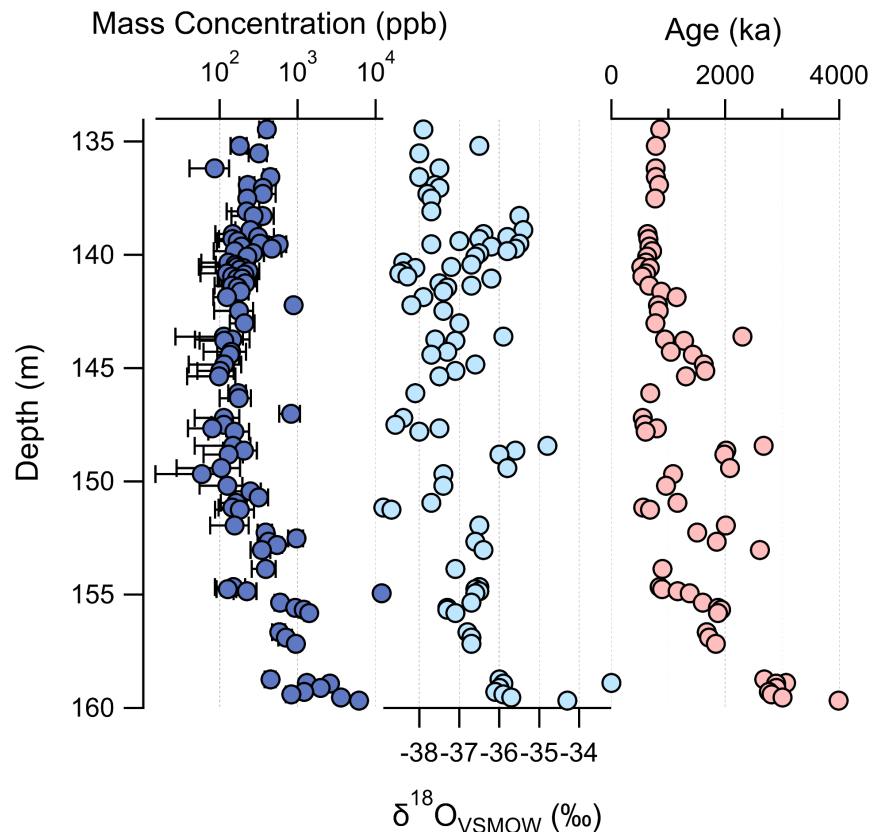
$E_0 = 15 \text{ kV}$; E_c (Ca K edge) = 4.038 kV

$R = 2.2 \text{ }\mu\text{m}$

Goldstein, J. & Newbury, Dale & Michael, Joseph & Ritchie, Nicholas & Scott, John & Joy, David. (2018). Scanning Electron Microscopy and X-Ray Microanalysis. 10.1007/978-1-4939-6676-9.

Figure 3: It would be helpful to have the d18O record of the ice overlaid on this figure. It is hard to distill information from the scatter plot S1 in the supplement.

We have revised Figure 3 to now include panels for $\delta^{18}\text{O}$ and $^{40}\text{Ar}_{\text{age}}$.



line 255: here you say 30 micron, but before you wrote the aperture was 50 micron? I also would write: "... given the aperture limit of 30 microm, but given their large size an aeolian origin of such particles can be safely ruled out (see also below)."

The aperture tube we used has an opening diameter of 50 micron, but can only tolerate a maximum diameter during measurement that is 60% of its diameter (i.e., 30 microns is the upper limit we can measure).

To clarify this, we will add the following text into "Section 2.2 Coulter Counter analysis":

"The range of particle diameters that can be measured is fixed between 2-60% of aperture tube size. For a 50 µm aperture tube, particle diameters can therefore be measured as small as 1 micron and as large as 30 µm."

line 281: "produced by aeolian processes"

We will revise this clause.

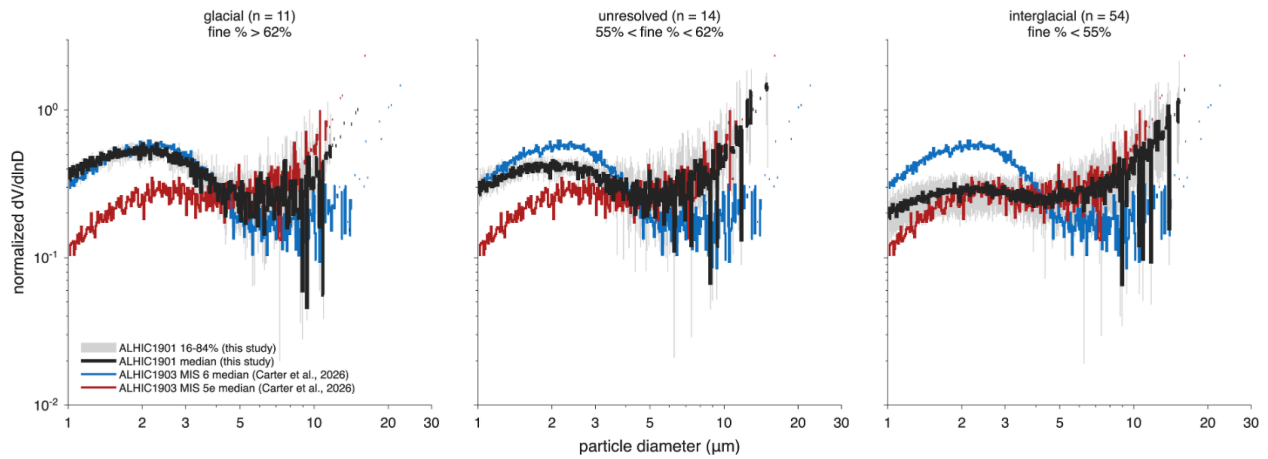
libe292-294: I do not understand/support the sentence: "In isolation...variability". This needs some rewording in my view.

To complement the colormap figure (Fig. 4), we will include a benchmark comparison of normalized bin % (either as an additional panel or supplementary figure) with the stratigraphically ordered ALHIC1903 ice core dust record, which contains glacial and interglacial ice spanning MIS 6 to 5e. We will then modify the text to say the following:

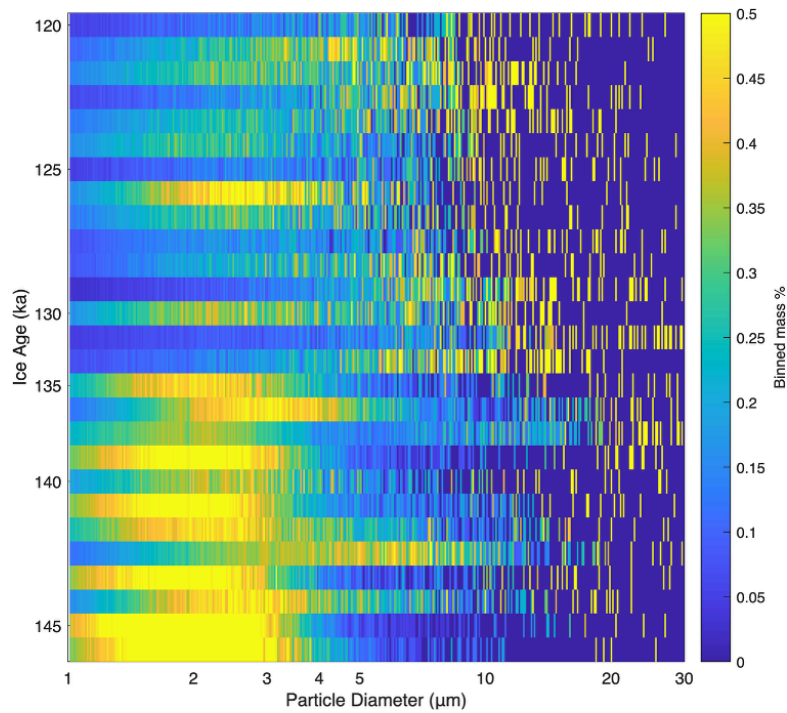
“These patterns are similar to what would be expected for glacial and interglacial variability when compared to the ALHIC1903 record.”

Figure 4: This is a central Figure! First of all, it would be great for the reader to see some representative examples of the measured (unnormalized) size distributions for individual samples (not just the aggregated information in this normalized colormap). Moreover, even above 155 m one can clearly see individual samples where the typical aeolian mode around 2 microns is completely missing (suppressed by the normalization procedure). These samples are clearly not of aeolian origin (neither glacial nor interglacial, even in the Talos Dome samples that show local larger dust, one can clearly see this mode). I would recommend a stringent screening algorithm (as you suggest in your conclusions) to sort out those samples that are likely of aeolian origin, and then investigate whether their fine particle fraction changes between glacial and interglacial and, whether the mode of the small particles increases with depth as expected from in situ aggregation. The latter effect was for example seen in the bottom-most parts of the EPICA ice core by Lambert et al., 2008, 2012.

Thanks for your comment. For coastal Antarctic ice cores, the modal distribution is often larger than 2 microns in diameter (see Aarons et al., 2017; Carter et al., 2026). Additionally, at Talos Dome, the lognormal distribution at 2 μm is completely absent in Holocene ice (see figure 2 in Albani et al., 2012). Regardless, we have included a new main figure that includes the full size distribution of particles binned by category (i.e., glacial, unresolved, and interglacial). The size distributions are normalized by total mass concentration so that distribution shapes are directly comparable with each other. We have included expected shapes for glacial and interglacial variability as reported in Carter et al., 2026. Notably, the interglacial dust size distribution typically lacks consistent modes at 2 μm within the Allan Hills. This is not a suppression feature, rather an expected pattern for interglacial dust deposition at this site. We have applied the same normalization procedure to ice from ALHIC1903 spanning MIS 6 to 5e transition (see figure below and figure in revised manuscript) that highlights the preservation of the 2 micron diameter modal distribution in glacial MIS 6 samples.



Normalized particle size distributions within ALHIC1901, categorized by the screening algorithm by glacial, unresolved, and interglacial. The median expected particle size distributions for glacial MIS 6 and interglacial MIS 5e are given in blue and red, respectively. Here, particle size distributions must be normalized by total particle size distribution in order to directly compare distribution shapes.



Normalized bin % of particle size distributions from ALHIC1903, covering MIS 6 to 5e (Carter et al., 2026).

line 335-337. You performed SEM/EDS. Can you use that information to help screening the data for folds?

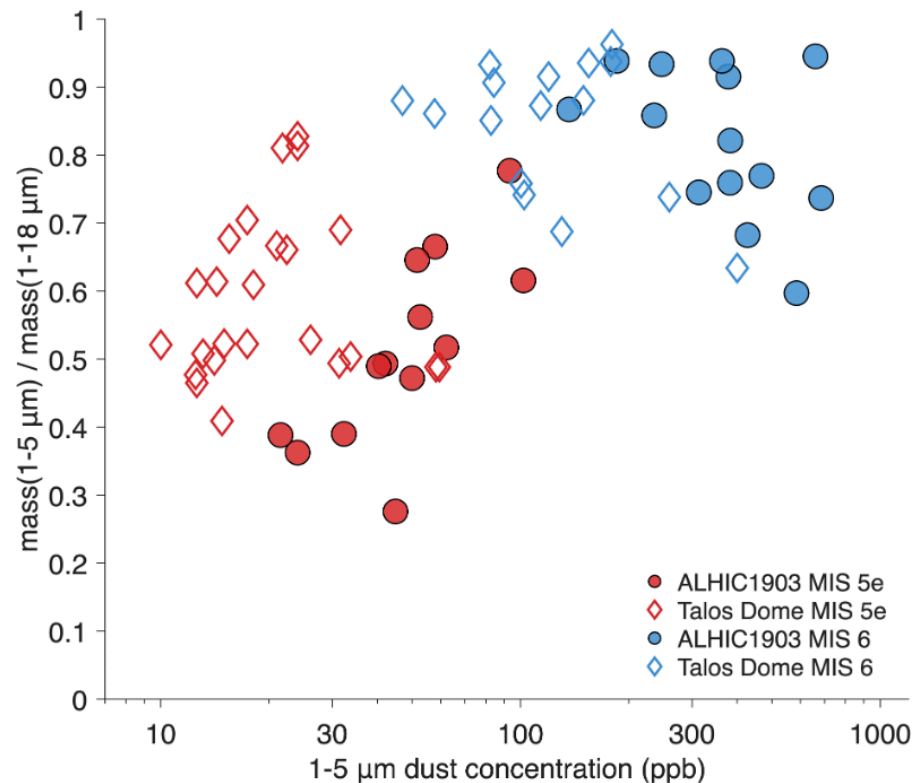
Thanks for this comment. Due to limited SEM/EDS sampling and sample availability, we are not able to use current available data to help screen if particulate matter present at depths of 142.23 m and 147.02 m is of aeolian or basal origin. SEM/EDS provides semi-quantitative analysis. Instead, future work could pursue quantitative approaches such as XRD for further mineralogical analysis and cryoEBSD for identification of shear zones and other stratigraphic disturbances. Further geochemical screening of these depths using Sr-Nd isotope composition analysis will also provide insight into the source and origins of elevated particulate mass concentrations at shallower depths.

line 348-352: Here the Carter data are mentioned (which I couldn't access yet) but they are only shown indirectly in Figure 17. It would be helpful for the reader, if they were somehow included in the other figures 2 and 3 directly as they represent a benchmark. As that paper seems to be accepted it should be possible to include the data. I also recognized that the size distributions for ice not affected by englacial entrainment of material appear to be very similar to Talos Dome (Albani et al., CP 2012), which could be another benchmark to compare with. This is also a relatively coastal core with local and remote dust deposition but no glacial entrainment of material.

We assume you are referring to figure 5 (not figure 17). We have already included the location of the ALHIC1903 ice core on the map shown in Fig. 2. The data are also included directly on Fig. 5 and 6b. We also apply the same dust concentration and size distribution normalization procedure to ALHIC1903 ice from Carter et al., 2026 and this will be included as an additional panel in either the main text or the supplement. All data produced by Carter et al., 2026 is hosted in the U.S. Antarctic Program Data Center and has been publicly available at the following links:

Strontium and neodymium isotope compositions of ice core dust from ALHIC1903 drilled at the Allan Hills Blue Ice Area.
Rare earth elemental concentrations of leached and fully digested ice core dust from ALHIC1903 drilled at the Allan Hills Blue Ice Area.
Oxygen and hydrogen isotope compositions and associated d-excess of ice from ALHIC1903 drilled at the Allan Hills Blue Ice Area.
Concentration and flux of ice core dust from ALHIC1903 drilled at the Allan Hills Blue Ice Area
Grain size distributions of ice core dust from ALHIC1903 drilled at the Allan Hills Blue Ice Area

Additionally, we will include a new supplementary figure which compares the MIS 6 and MIS 5e ALHIC1903 data with the Talos Dome data. The goal here is to confirm that the screening algorithm we apply to ALHIC1901, which relies on ALHIC1903, is also applicable to the Talos Dome data. While concentration can be offset due to dust loading, the general thresholds for fine % as a way to discern climate periods is applicable and consistent across the sites.



line 356: " "there is also an array"

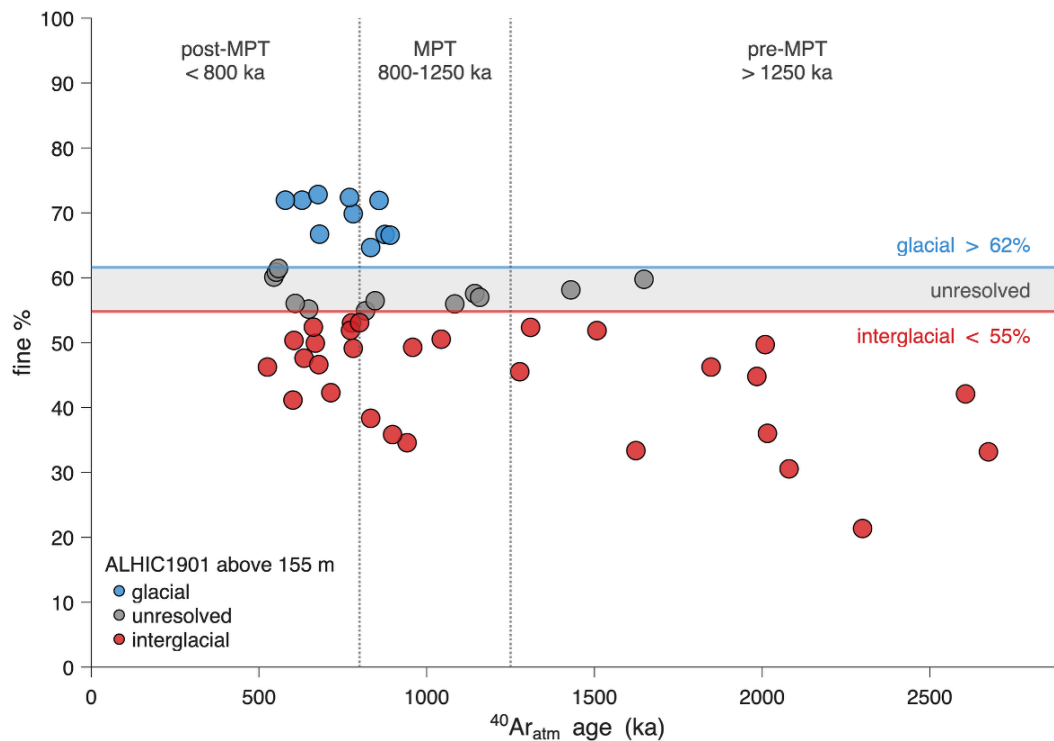
We have changed "are" to the singular "is." Thanks for pointing this out.

Figure 5: Is the trend of the glacial samples (blue dots) for high mass concentrations to increasingly reduced fine particle fraction a sign of aggregation (should increase with age)?

The glacial samples (blue dots) are from ALHIC1903, from a previously published study (Carter et al., 2026). There is no evidence of particle aggregation from that ice core. All new data are the colored squares with respect to age.

Figure 6 and text related to it: This is an example where the analysis and discussion would benefit from conservatively screening the samples for any samples affected by folds and englacial entrainment of material.

Thanks for this comment. We have taken steps to clearly define how we distinguish between the samples that are affected or unaffected by basal entrainment or processes through the addition of a new methods section. We will restructure the discussion to clearly separate between samples that are classified as typical glacial, interglacial, unresolved, and flagged. The below figure is one such result which shows the new ALHIC1901 data with respect to age and these categories. We will edit Figure 6 that shows this same sorting of samples with respect to dust mass concentration.



line 403-404 and line 412-414: see my comment above, a precise wording would likely improve clarity. As mentioned above the ice must have been deposited in a net accumulation area upstream. So the ablation at the Allan Hills drill site that leads to the formation of blue ice is not exactly relevant.

Thanks for this comment, we've removed reference to ablation being a reason for interglacial biasing.

line 417-420: Also here a more precise wording would help. We still have a net accumulation, otherwise we would not have a signal, but I of course agree that the net accumulation is likely to have been much smaller in the glacials than in the interglacials at the deposition site.

Thanks for this comment. We agree and have revised the wording to reflect variability in accumulation rate, rather than ablation.

line 423-424: I don't understand the point of the small "subsections", please elaborate.

One of the challenges of interpreting blue ice from the Allan Hills is its high age density per sample (1,000 to 100,000 yr/cm; Shackleton et al., 2025). The calculated age density varies by depth and is sample specific. In Shackleton et al. (2025) age density is calculated based on the change of age over the change of depth, where age is determined by applying the $^{40}\text{Ar}_{\text{atm}}$ geochronometer on a ~600 g sample of ice. Here, we reference the mass of each Coulter Counter sample (50-150 g) to make the case that temporal averaging in these small subsections is likely to have a less than minor impact on the measured signal. That is to say, the effects of thinning are not expected to substantially impact our measurements. However, we note that the sample size for CO₂ measurements are similar to those used for Coulter Counter analysis. In Marks-Peterson et al., 2026, CO₂ measurements were categorized into short vs. long snapshots related to age density of the ice. Therefore, it's possible the thinning mechanism could still play a factor in the dust analyses as well. For this reason, we remain more agnostic to whether thinning plays a dominant role or not and revise this section accordingly.

line 448: "expected to dissolve by cloud reworking during the long-range..."

Thank you for your comment. We plan to remove discussion of the presence of carbonate minerals in this manuscript (to be covered in a subsequent study).

Figure 7: add scale to each subpanel

A scale bar has been added to each subpanel.

line 475: what is meant here by "kinetic evaporation"?

Thank you for your comment. We plan to remove discussion of the presence of carbonate minerals in this manuscript (to be covered in a subsequent study).

line 509-522: The pH values without proper titration or correction for the dissolution of CO₂ from the lab air are not sufficient. In fact, the pH of 5.6 is what we expect from lab air exposure. I would delete this paragraph.

Thank you for this comment and the fair points provided. Accordingly, we will remove this paragraph.

line 527-528: "due to particle migration..."

This section will be revised.

Section 3.6: While this is an interesting topic, the discussion remains circumstantial. Importantly, the discussion in the first paragraph suggests that carbonates may be responsible for in situ CO₂, however, the data by Marks-Peterson (paragraph 2) point to carbon that is isotopically lighter than the atmospheric value (respired organic carbon). If this section remains in the paper, this needs to be worded more clearly.

While we argue that the connections between dust and CO₂ are important to our understanding of non-atmospheric CO₂ in the ice core record, we also agree that our discussion of it and the mechanisms associated are inherently limited due to the scope of this study. Your points regarding carbonate vs. organic carbon are also important to consider. Given the $\delta^{13}\text{C}$ of CO₂ reported in Marks-Peterson et al., 2026, the most likely explanation of non-atmospheric CO₂ is from respired carbon, although some mixture with carbonate is still plausible. This would change the discussion as argued in our manuscript currently. Additional analysis would be ideal, but would be outside the scope of the manuscript. For these reasons, we have opted to remove the CO₂ discussion in this manuscript with the goal of it being built upon in a subsequent manuscript.

line 592-595: here the authors suggest a multiproxy filtering approach to distinguish samples affected by the various processes. The paper would gain of significance, if exactly this would be attempted in this paper.

Thanks for this constructive comment. To address it, we plan to change the formatting of the manuscript to begin with a detailed description of a multi-proxy screening process by which samples will be distinguished into the following categories: 'glacial', 'interglacial', 'unresolved', or 'flagged', with the last category being atypical for post-MPT glacial/interglacial variability. We will then restructure the discussion into sections that allow us to interpret the dust record in samples by category and provide several interpretations regarding the driving mechanism behind the flagged category including basal processes and local aeolian input (and related paleoclimate implications).