

Key:

Community & referee comment

Author response

The manuscript presents new results from dust concentration, size distribution, and composition from the deepest section of the ALHIC1901 ice core from Alan hills blue ice area. The discussion focuses primarily on the preservation of an atmospheric dust signal, the possible contribution of basal or subglacial material, and post-depositional processes affecting the oldest ice. These observations are potentially valuable and contribute to the growing literature on the interpretation and fidelity of blue ice archives. In its present form, I find it difficult to identify the central scientific contribution of the manuscript. The manuscript considers many plausible processes and I acknowledge the meticulous discussion, but the distinction between observation, diagnosis, inferred mechanism, and climatic implication is often unclear. As a result, potentially interesting observations are not yet developed into a sufficiently well-defined scientific argument. I do not think that the manuscript is yet sufficiently mature for a conventional major revision. The extent of restructuring required goes beyond what I would normally consider appropriate for a major revision. At present, the organization makes it difficult to evaluate the strength and novelty of the scientific conclusions independently from the presentation. I believe that the dataset may ultimately support a useful contribution, but the manuscript would benefit from substantial reconception before resubmission.

Thank you for this constructive feedback. We have taken this comment and others raised by the other referee to carefully reconsider what the aim and scope of this work is. Based on this comment (and others), we have chosen 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. We have also chosen to diminish the discussion of the observed relationship between dust and CO2.

1. Define the central scientific question and reorganize the manuscript around it

The most important issue is that the central scientific question is not sufficiently clear from the introduction and only emerges relatively late in the manuscript. The sentence “A central question arising from the insoluble particle record of ALHIC1901 is whether a primary climate signal is preserved within the particle mass concentration record” could, in my view, provide the core question of the entire manuscript. The introduction should therefore be substantially shortened and reorganized around: (1) the scientific context; (2) the specific problem associated with interpreting insoluble particles in old blue ice; (3) why existing observations are insufficient to resolve this problem; and (4) how the present measurements address it. The current introduction contains useful background information, but it reads more as a broad introduction to ice-core paleoclimate, with considerable emphasis on CO₂, than as a focused motivation for the analyses presented here. I understand why the authors framed the manuscript around the interpretation of anomalous CO₂ in blue ice, particularly in light of recent work such as Marks-Peterson et al. (2026) and Banerjee et al. (2026). However, I think the present dataset is

more naturally suited to a question focused on the fidelity of the particulate record itself: to what extent does the deepest ALHIC1901 ice preserve an atmospheric dust signal, and where do non-atmospheric or post-depositional processes become important? Following such a framing, the manuscript could be organized more explicitly as observation => diagnostic/comparison with existing literature => possible mechanism => climate implications. This distinction is currently blurred in several places. In particular, mechanisms such as basal entrainment, in situ mineral formation, or preferential preservation of interglacial ice are sometimes presented more strongly than the available observations appear to warrant. I think that a particularly useful outcome would be to define criteria, where possible, for distinguishing regimes dominated by (1) preservation of an atmospheric dust signal; (2) substantial post-depositional / in situ modification; and (3) basal contribution. These need not necessarily be sharp depth thresholds, but future studies would benefit from general observations that enable to identify dominating regime. The CO₂ relationship could then either be used as one component of this diagnostic framework or discussed later as an implication of the particulate observations.

This comment is well received and has aided substantially in our reframing of the manuscript. We will take the following steps to define the central scientific question and improve the flow of the manuscript:

Reframing the content and scope of the introduction to focus on how we can screen for anomalous dust characteristics in ice core records.

- 1. Reduce focus and detail on certain motivations, particularly climatic interpretations, in the introduction that are not expanded upon within the discussion and results of this study*
- 2. Define a multi-proxy screening process at the beginning of the manuscript by which samples will be distinguished into the following categories: 'glacial', 'interglacial', or 'unknown', with the last category being likely driven by basal processes. This will be accompanied by an additional figure contextualizing this screening process with an Allan Hills ice core that captures a glacial-interglacial transition continuously (Carter et al., 2026)*
- 3. Discussions on the mechanisms behind excess CO₂ production have been removed. We agree that this part of the study requires further analyses and may be better placed in a separate CO₂ focused paper.*

2. Basal origin is plausible, but not independently demonstrated

One of the major interpretations of the manuscript is that the deepest particle-rich ice reflects entrainment from the bed/subglacial environment. This interpretation is plausible and is supported by several observations, including elevated particle concentrations, coarser grain sizes, visible debris, and unusual mineralogical features. However, the present measurements do not independently determine the provenance of this material. Several origins may remain compatible with the observations, including local aeolian material, folded debris-rich ice, or old-ice postdeposition processes. Sr–Nd isotope compositions, ideally compared with local source materials, would provide such a test and the authors mention such analyses would be

done in the future. If these measurements are available, I strongly encourage the authors to consider including them in the present study, as they would substantially strengthen one of the central conclusions. I am concerned that separating the current observations from a future provenance study may weaken the scientific contribution of both papers if they ultimately address closely related aspects of the same question. If provenance measurements cannot be included (or the authors prefer not to), I recommend moderating the corresponding language throughout the manuscript. The present observations appear consistent with basal or subglacial input, but they do not demonstrate a bedrock or subglacial origin. As such it is not possible to deconvolute the post-depositional alteration of the atmospheric signal from non-atmospheric, basal or subglacial particle origin.

Thank you for your comment and feedback.

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).

Current available particulate matter data suggests a bedrock/subglacial origin but does not independently confirm this. The discussion section will be edited to reflect the possibility that ice flow may have brought the ice at its current location near exposed local material in the past which would also be consistent with our observations. It is possible that the particle-rich ice is sedimentary material sourced from tens of kilometers away from the Allan Hills location, and if so comparing the deep ice particulate Sr-Nd compositions to the Allan Hills surrounding bedrock would not provide a clear answer as to where this material originated from. However, we do now include a hand sample description of an englacial rock fragment from the bottom 5 m of the ALHIC1901 ice core (see image below). This has been physically characterized and attributed to be from the Jurassic Lashly Formation or the Late Permian Feather Conglomerate, which these units are primarily exposed in the northern Allan Hills.



3. Separate Results and Discussion more clearly

I have been confused by the logical flow between result presentation and discussion. I strongly recommend having dedicated Results and Discussion sections. The result section would include the concentration and size distribution profiles, as well as SEM observations comparison with water isotopes and chronology and present overall variability and uncertainties. The Discussion should then evaluate the possible mechanisms responsible for those observations. A clearer separation would also support the conclusion: several different mechanisms may produce similar observations, and these alternatives need to be evaluated explicitly rather than introduced successively throughout the text.

Thank you for your feedback. We agree that the takeaways of this study will be better communicated with separate results and discussion sections. The discussion will be formatted to interpret the data relative to the screening process we described above.

4. More quantitative support is required for the proposed regimes and mechanisms

Much of the interpretation remains qualitative and I recommend increasing the quantitative treatment of the data. For example, the manuscript links elevated particle concentrations with an increased contribution of coarse particles. Is this relationship systematic across the record (i.e. even above 155m)? Are all high concentration samples associated with elevated coarse-particle concentrations, or is it only a pattern for the deepest section? This distinction is particularly important because some coarse-particle layers occur above the bottommost section of the core (Figure 4 below with red arrows added). These layers may provide useful information for testing whether the mechanism proposed for the deepest ice is unique to the basal interval or occurs elsewhere in the record. Similarly, statements that samples are predominantly “glacial-like” or “interglacial-like” should be supported quantitatively.

Thank you for raising these important points. We have edited the manuscript to increase the quantitative treatment of the data and communicate that more clearly to the reader. This includes establishing specific parameters for samples to be defined as glacial, interglacial, unresolved or flagged. The screening algorithm incorporates quantitative approaches such as the 95th and 5th percentile of fine % from known glacial and interglacial samples as reported in Carter et al., 2026 which will be outlined in several additional figures.

5. Overall recommendation

There are potentially valuable observations in this study, and I believe that a substantially reorganized manuscript could make this manuscript an important paper for deep- and blue-ice dust records. However, I do not think that the present version yet resolves a sufficiently well-defined scientific question as highlighted by the introduction. Because of this important reorganization that I believe is required and the possibly additional analyses, I prefer to reject the paper not of noninterest but to provide more time to the authors. This recommendation is not based on a lack of interest in the dataset or the conclusion, but would enable the authors to make this paper a milestone of dust interpretation in deep and blue ice. Still, I have listed below some points (either major or minor) which could help write a stronger argument. The text would benefit from removing vague qualifiers ('often', 'generally', 'critically'...) when they do not add specific meaning.

Thank you for your constructive comments on this manuscript. It has been very helpful in reframing the study and our specific responses to your detailed comments are below.

L1: title. I find the title overstated. "Toward extending the archive to 4000 ka" implies that an interpretable dust archive is extended from the EDC record. However, whether the measured particles preserve an atmospheric dust signal is precisely one of the central questions of the manuscript. I suggest a title focused more directly on the problem addressed, for example the preservation or deconvolution of atmospheric and non-atmospheric particle signals in old blue ice.

We propose a new title that more accurately describes the takeaways of this study: "Dust record from 4 Ma Allan Hills Blue Ice: Distinguishing Climate Signals and Basal Contamination in Deformed Blue Ice"

L53: I am not convinced that the discussion of marine dust records is necessary here. As currently written, it gives the impression that marine records are intrinsically problematic, whereas the limitation is more specifically related to comparing different archive types and depositional environments. I suggest removing this discussion unless it is directly needed to motivate the present study.

We have taken steps to streamline the introduction including 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.

Fig 1: I would add the depth interval considered in this study (and maybe that from the Carter et al 2026 paper).

ALHIC1903 was not drilled to bedrock and is on a different flow regime compared to ALHIC1901, so it may be misleading to include both depth intervals on this schematic. We have included the depth interval from the Dome C ice core as well as the depth interval from ALHIC1901.

Either here or in Fig. 2, a depth overview highlighting the bottommost 5 m, available age horizons, major stratigraphic features/anomalies, and relevant previously studied intervals would greatly help the reader.

Thank you for this suggestion. We are keeping this schematic on a conceptual scale and we lack age horizons because the chronology has been established using $^{40}\text{Ar}_{\text{atm}}$ geochronometer measurements, and contains discontinuities and/or reversals. The relationship between age and depth will be added as an additional panel to figure 3.

The conceptual Figure 1 should also include the post-depositional processes such as in situ formation (Baccolo et al., 2021a;b; de Angelis et al., 2013.)

The conceptual figure 1 is in the process of being updated to more accurately reflect the scope and focus of this manuscript.

L134: yr-1 should be a-1

We use 'a' to mark a position in time (e.g., ka and Ma) and we use 'yr' to mark elapsed time (e.g., yr, kyr, Myr). Because accumulation rate represents a duration, we opt to keep yr-1 to be consistent with this convention. In a recent publication in The Cryosphere by Morgan et al., 2022, accumulation rates were reported as per 'yr' so for consistency we opt to report it the same way.

<https://doi.org/10.5194/tc-16-2947-2022>

L144 "50-150 g" what is the sampling resolution in depth and time?

The Coulter Counter sampling resolution in depth ranged from every 3 cm to 1.6 m with a median sampling interval of 20 cm. However, the sampling resolution with respect to time is difficult to constrain because Allan Hills ice has high age density per sample (1,000 to 100,000 yr/cm; Shackleton et al., 2025). Specifically, the calculated age density varies by depth and is sample specific, and without accumulation rate history, it's difficult to confidently determine time resolution. This will be updated in the revised manuscript.

L178: “a greater number of times to ensure accuracy of data.” How many? Please provide the standard deviation of the measurement. Besides, please define what criterion was used to identify an ‘abnormal final particle count number’?

We clarify that we mean to say when referring to abnormal final particle count number:

“For a given sample, analytical runs that contained total particle counts greater than 30% relative to the previous run indicated instrumental blockage, and the aperture was subsequently cleaned. Additional runs were performed until a total of five were obtained for each sample.”

L196: the uncertainty of total particle mass should be provided

We compute and report uncertainty of total particle mass for each sample and add the following text around L216:

“Uncertainties on mass concentration were estimated from the variability between replicate analyses for each sample. For each size bin, the standard deviation of the particle count across replicate runs was taken from the instrument output, and the same was determined for the procedural blank. Sample and blank uncertainties for each bin were combined as the square root of the sum of their squares and converted to mass following Equations 1-4. The uncertainty for each reported size range (e.g., 1-5 μm , 1-30 μm) was then calculated as the square root of the sum of the squared bin uncertainties across the size range and is reported as two standard deviations.”

L220 section 2.3, specify which samples (which depth) were taken for SEM analyses. How did you select these depths?

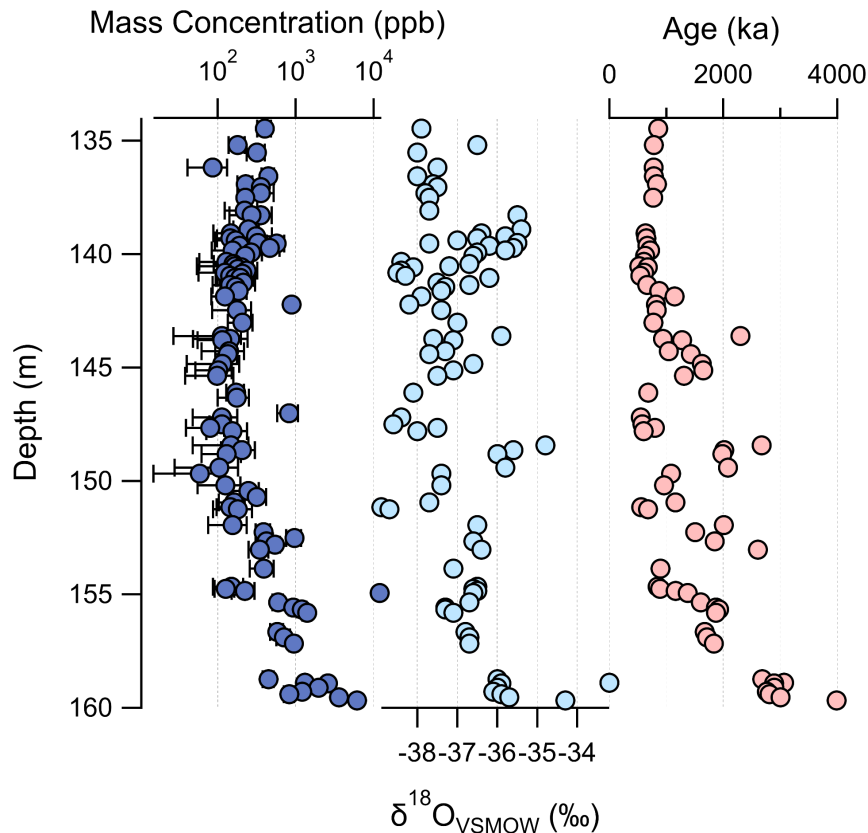
Depths were selected for samples which were large enough to contain residual sample volume after uptake for Coulter Counter analysis. Specifically, we also chose to include samples deeper than the 155m abnormality identified in figure 4 (228_3, 230_3B, and 233_2B) as well as a shallower depth (203_3_2).

L194: The Coulter Counter analysis is restricted to particles $<30 \mu\text{m}$ (and several comparisons use $<20 \mu\text{m}$), whereas visible particles are reported. Thus, the reported “total particle mass concentration” may underestimate the actual particulate mass in these coarse samples. Please avoid the term “total particle mass concentration” unless it explicitly refers to the total within the measured size range.

Thank you for bringing up this important point. We understand the confusion and distinction between the total particle mass concentration in a sample (exclusive of upper and lower grain size limits) with the total particle mass concentration measurable by the Coulter Counter (1 to 30 μm) in this study. References to the “total particle mass concentration” will be amended to say “total measured particle mass concentration” or “mass concentration”.

Fig 3: add water isotopes and highlight glacial and interglacial layers. A CPP profile would be welcome. Age horizons or argon profiles would also be a nice addition

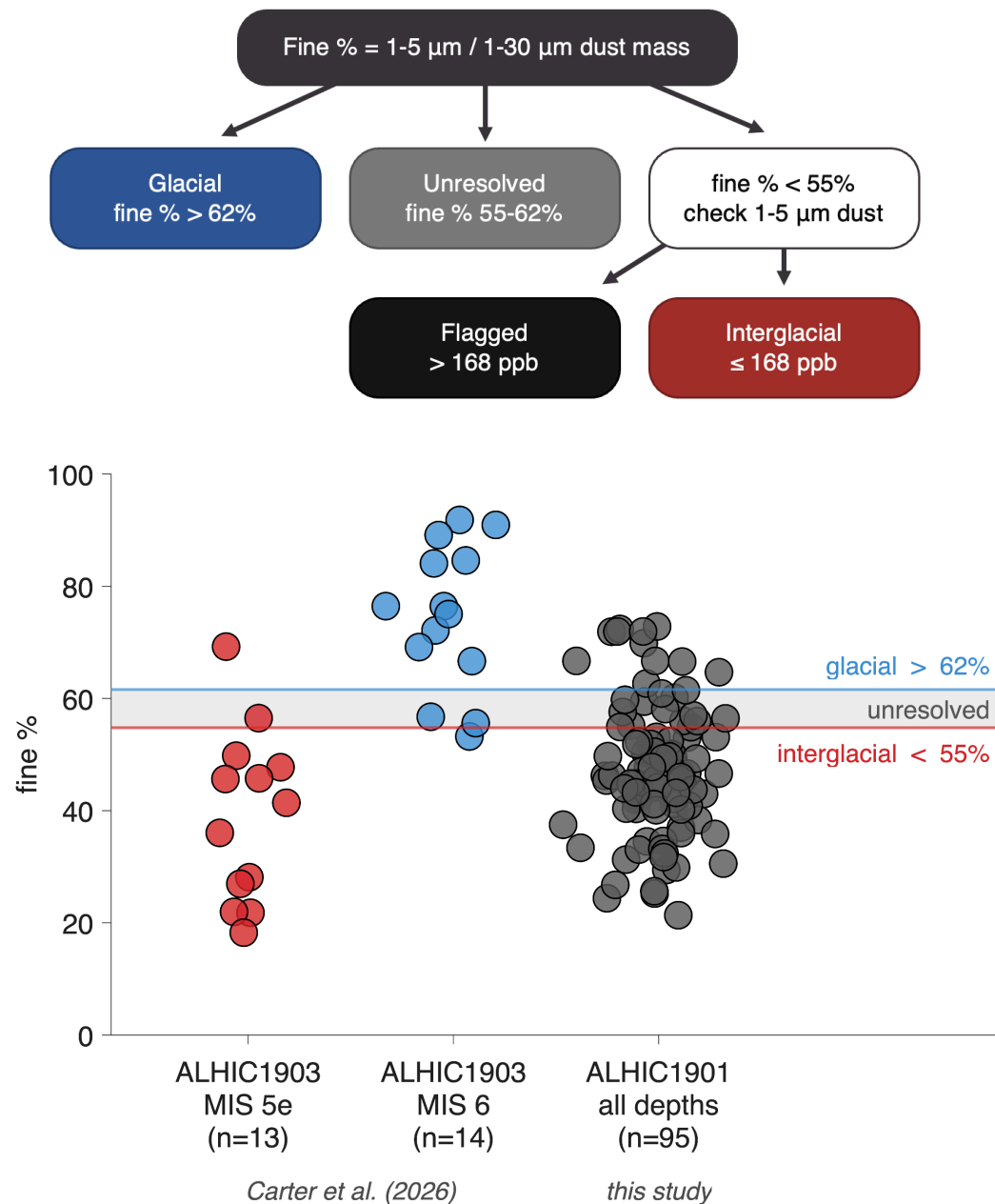
Water isotopes will be added to this figure, and samples will be colored based on the screening algorithm to indicate glacial or interglacial samples. The screening algorithm takes into account the fine particle percentage so we've chosen not to include FPP or CPP on this figure. We also plan to include the depth versus Ar age, see the figure draft below:



L248-251: Please provide quantitative ranges for the reference glacial and interglacial populations (e.g. median and interquartile range, or mean $\pm$ 1 SD if normally distributed). Should these ranges appear in Figure 3? Besides, a statistical test would be beneficial to demonstrate whether the concentrations in the deep 5 meters are significantly different from glacial periods or not.

We use the stratigraphically ordered ALHIC1903 as our benchmark for expected glacial/interglacial patterns at the Allan Hills (Carter et al., 2026). Glacial dust is fine and abundant, interglacial dust is coarser and sparse. A flagged sample has interglacial grain size with glacial-level dust. Specifically, the screening algorithm uses the fine % of the published ALHIC1903 dust record as a benchmark for characterizing ALHIC1901 dust by climate state. The glacial threshold ($>62\%$, 95th percentile of MIS 5e) and interglacial threshold ($<55\%$, 5th

percentile of MIS 6) were determined statistically. The flag then adds a fine concentration floor of 168 ppb, the 5th percentile of MIS 6. This screening algorithm is agnostic of depth and takes into account the physical characteristics of the dust particles to separate into glacial, interglacial, or flagged populations. This reframing will be reflected in the final manuscript as well. See flowchart below:



L251-255: the observation 'to the naked eye' is interesting, but is part of the size distribution section

Agreed. As we restructure the manuscript, the majority of the content in lines 251-255 may be better placed within a section focused on insights into sediment source from size distribution

data. We will emphasize the grain size limitations of particle mass concentration analyzed in this study despite observed grain sizes outside of the 1 to 30 μm range.

L259: is glaciofluvial processes really the only source of dust? I do not question the Southern America origin but more the unicity of the glaciofluvial origin. Citation of Sugden 2009; Kaiser & Lamy 2010 are also welcome.

Has been amended to include: “glaciofluvial processes (Sugden et al., 2009) and environmental changes such as sea level shifts (Kaiser and Lamy, 2010).”

L260: Delmonte 2004 a or b?

This should be Delmonte et al., 2004a. We will revise this accordingly.

L291: “samples at varying depths are a mix of either dominantly fine particle (1–5 μm) mass contributions or intermittently spaced coarse particle (>5 μm) mass contributions.” Can you add contribution details in % and absolute concentrations as well?

The fine % and absolute concentrations for each sample are displayed on Figure 5.

L292: “the patterns observed at these depths suggest the preservation of a climate signal (i.e., glacial and/or interglacial variability).” is difficult to see without water isotope profiles and statistical test to support this assertion. Please see also my previous comments regarding the link between high concentrations and large particle size even above 155 m (red arrows on Figure 4).

Water isotope data are now included in figure 3, we now include the screening algorithm which uses the fine % of the published ALHIC1903 dust record as a benchmark for characterizing ALHIC1901 dust by climate state. The glacial threshold (>62%, 95th percentile of MIS 5e) and interglacial threshold (<55%, 5th percentile of MIS 6) were determined statistically. The flag then adds a fine concentration floor of 168 ppb, the 5th percentile of MIS 6. This screening algorithm is agnostic of depth and takes into account the physical characteristics of the dust particles to separate into glacial, interglacial, or flagged populations. This reframing will be reflected in the final manuscript as well.

L321: any use of the word significantly needs to be associated with a statistical test. Either add this test (which I recommend) or replace the word by substantially.

All instances where “significantly” was used without an association with a statistical test has been replaced with the word “substantially.”

L322: the Fig S1 cannot allow to properly identify whether a shift is absent in the water isotope profile or not. Their addition to Figure 3 would help.

Water isotope data has been added to figure 3 (see above).

L328-335: I recommend adding some age horizons to support the argument of a jump in age scale.

An Ar age scale will be added to figure 3 as well (see above).

L340-342 'A central question ... impacted the archive'. At this point in the paper, the central question should not arrive at this point in the paper. By clearly stating it in the introduction, all sections would converge to answer it.

The manuscript will be revised to bring up this central question early on in the paper and we use this framework to restructure the paper and discussion sections.

L355: 'the majority of samples are consistent with interglacial' is typically an example where statistical tests are welcome

We will alter the language of this sentence to specify the number of samples associated with interglacial conditions out of the total number of samples.

L356 'an array of samples' this is not clear which sections do you precisely refer to.

These samples will be specified in terms of depth to clarify which sections they are in reference to to reduce confusion.

L360: "a mixture of ice from both climatic periods or an injection of basal material at greater depths, or a combination of both." I agree and this is one of the most important sentence of the paper. I also agree that it cannot be currently answered, yet the authors should add some perspective analyses that would enable to answer this.

Thanks for this comment. We plan to add more discussion on possible scenarios that could result in the dust concentration and size distribution that we measured in these deep ice samples, and we plan to expand upon this in the revision.

Fig 6: maybe the addition of water isotopes would help identify dominating processes. Typically is there more agreement for the 500-750 ka period than for the 750-900 ka period?

This is included now in figure 3 and previously in S1 there is no clear trend between dust mass concentration and water isotopes.

L403-433: the coherent absence of glacial layers among different proxies is striking. Low accumulation and preferential preservation are plausible explanations, but I encourage the authors to discuss whether processes known to affect very deep continuous ice-core records (thinning, folding, stratigraphic disturbance, physical mixing, and post-depositional modification)

could explain the observations here. Comparison with deep TALDICE and EDC would help distinguish processes specific to blue-ice settings from more general processes affecting old deep ice.

Thanks for this comment. We do mention that thinning and stratigraphic disturbance may affect this record already, but we plan to add more discussion on how folding and physical mixing may result in the post-depositional modifications described above.

L428-433: the interpretation is not clear to me. Can you clearly explain step by step the reasoning?

This interpretation is based on figure 5, which shows the use of fine particle percentage and particle mass concentration to determine if a particular sample is more consistent with that expected of interglacial or glacial conditions. These fine particle percentage and particle mass concentration parameters are defined from ALHIC1903 dust analysis where the MIS 6 to 5e deglacial transition is captured continuously (Carter et al., 2026). Additionally, figure 6 provides a different dimension of this interpretation, showing a comparison between the particle mass concentration of ALHIC1901 and EDC, a continuous deep ice core. Overall ALHIC1901 contained lower particle concentration and reduced variability suggestive of a preservation bias. Both these lines of evidence support the takeaway that when compared to other ice cores (both from the Allan Hills region and old continuous ice), ALHIC1901 lacks preservation of distinct glacial-interglacial cycles and in samples that preserve an atmospheric signal, these samples are biased towards containing dust more consistent with interglacial conditions. The lack of preservation of distinct glacial-interglacial cycles is also supported by the plotting the oxygen isotope composition of ice and dust mass concentrations. Glacial periods are associated with higher dust mass concentrations and depleted oxygen isotopes while interglacial periods are associated with lower dust mass concentrations and enriched oxygen isotopes so one would expect these co-measured proxies to be directly correlated. However, measured samples lack such correlation (Figure S1).

L440-444: “The mere presence of carbonate minerals in the lower portion of ALHIC1901 is puzzling, as Southern Hemisphere mid-latitude sourced dust reaching Antarctica has been noted to have virtually no carbonate minerals present (Delmonte et al., 2004).” As later acknowledged by the authors, the presence of carbonate minerals is not puzzling. Carbonates and secondary mineral formation have previously been observed in deep Antarctic ice (e.g. De Angelis et al., 2013; Tison et al., 2015; Baccolo et al., 2021 a, b). The discussion should therefore distinguish between carbonate inherited from the original dust source and carbonate formed or modified in situ. In the absence of provenance constraints, carbonate occurrence alone cannot discriminate between these processes. Consequently, remove the sentence “as these minerals are most likely to be dissolved during transit prior to deposition on the ice sheet surface”

Discussion of the carbonate minerals in ALHIC1901 will be removed.

L471: “formed carbonate” similar to EDC (Tison et al., 2015) Link CO₂-dust: Is the dust–CO₂ relationship equally strong throughout the investigated interval, or is it restricted to the deepest samples? Do you observe a link with $\Delta^{17}\text{O}$ profile from Banerjee 2026?

Discussion of CO₂ will be removed.

The particle mass concentration and CO₂ relationship shown in figure 8 is strongest in the bottom 10 meters with a single outlier at approximately 140 m depth. Additionally, most of the observations of elevated CO₂ concentrations from a non atmospheric source (anomalous $\delta^{13}\text{C}$ values less than -7.1‰) were prevalent in the deepest samples.

Because we are removing the discussion of CO₂ from this manuscript, the CO₂-dust relationships were not analyzed using reconstructed CO₂ values from Banerjee et al., 2026.

Fig 8: Why not tracing all samples and show that the correlation if only valid for the subset of nonatmospheric CO₂?

Discussion of CO₂ and dust relationships will be removed from this study. The CO₂ -dust relationship is not shown for all samples due to the large range of particle mass concentration measurements. Due to this wide range, earlier figures showing particle mass concentrations generally do so with a log scale axis which would make it more difficult to view the linear relationship between the subset of non-atmospheric CO₂ and particle mass concentrations.

Fig S5 add the depth colorbar to all sub-figures.

This figure will be removed alongside discussion of elevated CO₂.

Finally, I want to hear the authors opinion on the representativity of the site, following the Hudak et al. preprint. This could be mentioned in the discussion. The fact that you may have similar processes than in deep continuous ice (TALDICE and EDC) add element to this discussion. At the same time, the comparison of values between ALHIC1901 and ALHIC1903 needs to be more discussed. In the end, are the conclusions of this paper specific to ALHIC1901, and its specific flow regime, or can it be extrapolated to other sites?

Processes resulting in elevated particulate matter concentrations could be occurring at other marginal ice core sites as well as in deep continuous ice as you mention above. However, the screening process utilized, especially specific depth, particle mass concentration and FPP cutoffs may vary across sites and chronologies. Therefore the screening process outlined in this study establishes a procedure that could be adapted for other ice core dust studies but we stress that the results of this screening technique does not indicate conclusively that bedrock entrainment has occurred. This screening technique incorporates elements of dust behavior during Pleistocene glacial-interglacial cycles and the behavior of the dust cycle during Pliocene glacial-interglacial cycles may be fundamentally different. We will add text and discussion to the manuscript to emphasize this point. In addition, methodologies may vary from study to study.

For example, the Hudak et al. preprint measures particulate matter using a laser particle counter and reports these data as particles/mL whereas in our study we use a Coulter Counter to quantify dust mass concentration. This may pose a challenge for applying this screening technique at other sites in the future.

Additional references I recommend to include:

Angelis, M. de, J. L. Tison, M. C. Morel-Fourcade, et J. Susini. Micro-investigation of EPICA Dome C bottom ice: evidence of long term in situ processes involving acid–salt interactions, mineral dust, and organic matter. *Quaternary Science Reviews* 78 (2013): 248-65.
<https://doi.org/10.1016/j.quascirev.2013.08.012>.

Tison, J. L., M. de Angelis, G. Littot, et al. Retrieving the Paleoclimatic Signal from the Deeper Part of the EPICA Dome C Ice Core. *The Cryosphere* 9, 4 (2015): 1633-48.
<https://doi.org/10.5194/tc-9-1633-2015>.

Kaiser, J., & Lamy, F. (2010). Links between Patagonian Ice Sheet fluctuations and Antarctic dust variability during the last glacial period (MIS 4-2). *Quaternary Science Reviews*, 29(11-12), 1464-1471.

Banerjee, Asmita, Edward J. Brook, Christo Buizert, et al. Triple Isotopes of Oxygen as a Proxy for Carbon Dioxide in Altered Early Pleistocene Ice Core Samples. *Journal of Geophysical Research: Atmospheres* 131, 14 (2026): e2025JD045900.
<https://doi.org/10.1029/2025JD045900>.

Thank you for these recommendations, these studies will be incorporated into the revised manuscript if and where appropriate.