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.

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 the 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.

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.

3. Separate Results and Discussion more clearly

I have been confused by the logical flow between result presentation and discussion. I strongly recommend to have dedicated Results ad 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.

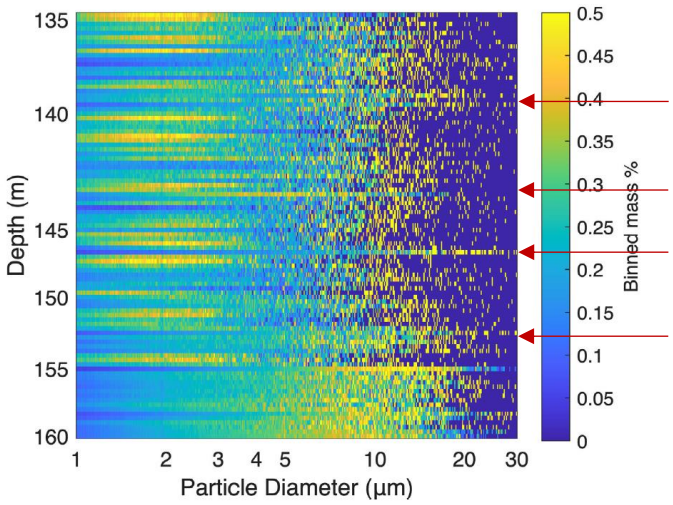
The Discussion should then evaluate the possible mechanisms responsible for those observations. A clearer separation would also support the conclusion: several different mechanisms may produce similar observations, and these alternatives need to be evaluated explicitly rather than introduced successively throughout the text.

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

Much of the interpretation remains qualitative and I recommend to increase 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.

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 introducion. Because of this important reorganization that I believe is required and the possibly additional analyses, I prefer to reject the paper not of non-interest 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.

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.

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.

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

L134: yr^{-1} should be a^{-1}

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

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 ‘anormal final particle count number’?

L196: the uncertainty of total particle mass should be provided

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

L194: The Coulter Counter analysis is restricted to particles <30 µm (and several comparisons use <20 µm), 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.

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

L248-251: Please provide quantitative ranges for the reference glacial and interglacial populations (e.g. median and interquartile range, or mean ± 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.

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

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.

L260: Delmonte 2004 a or b?

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?

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).

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*.

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.

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

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.

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

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

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.

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?

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.

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

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”

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 Δ17O profile from Banerjee 2026?

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

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

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?

Additionnal 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.