The authors would like to sincerely thank Reviewer #1 for their insightful comments and their time for reviewing this manuscript. We have addressed their comments below and believe their feedback has strengthened the science of this manuscript.

Reviewer 1

In their manuscript Hudak et al. present CFA analysis and Ar40 dates of two Allen Hills cores alongside a description of the CFA setup at OSU. The CFA data is than used to investigate differences in variance of the Allen Hills data in comparison to existing EDC data. The application of continuous flow analysis to the discontinuous AL ice is certainly a challenge in terms of data analysis opening interesting avenues of investigation. Overall, the manuscript is well written and argued and fits the scope of Climate of the Past.

I do, however, have a request and some suggestions that should be taken into consideration prior to publication:

All the data analysis that the authors present is based on re-sampled versions of the existing EDC data. Because most of the analysis is dependent on the choices made during the resampling these choices need to be very well justified and compared to the ALHIC data, i.e. its age density. This justification and its implications for the analysis is probably best placed in the EDC data section or in an extra section that describes the statistical analysis. I understand that the age density of the ALHIC data might be ill defined but as so much of the discussion hinges on the differences in the data distributions to the EDC data it is well spent time/space to argue this carefully to take the reader along.

We agree that EDC sampling is an important consideration. Section 2.4 (EDC datasets) has been modified to add this section:

The purpose of the EDC comparison is to assess whether discontinuous blue ice at the Allan Hills reliably preserves environmental signals and glacial-interglacial variability. ALHIC 2201 and 2302 do not have a robust age scale due to discontinuous stratigraphy in conjunction with limited discrete ^{40}Ar dates. Therefore, the EDC comparative analyses are conducted with all EDC data resampled on a 1 m binned depth scale, rather than on an age scale. Comparison between these two records on a depth scale allows for interpretation of the distribution of environmental variability in a more meaningful way. The depth binning of the EDC record limits the influence of sampling resolution biases as a result of uneven depth sampling in the EDC datasets. The continuous 800 kyr EDC record is an appropriate benchmark for the ALHIC CFA records in that most of the estimated ages fall within this age range (see Section 3.1) and the distributional comparison of the environmental variables is independent of the continuity of the record. Due to regional differences between Victoria Land and the East Antarctic Plateau, a mean offset is applied to the Allan Hills water isotope and dust values for better comparison to EDC.

The authors put a lot of focus on purely descriptive statistics (mean, variances, simple correlations). The quantitative analysis is hidden in section 3.3.2 and mostly on the appendix. This is unfortunate as I think the analysis of the variance reduction and the representation of the climate states is clever

and provides a lot of substance to the observations. For my taste, most of the descriptive statistics tables/figures could be moved to the appendix instead of the variance reduction analysis.

We thank the reviewer for highlighting the value of the quantitative variance reduction and accumulation scaling analysis in Section 3.3.2 and Appendix B. We agree that this analysis offers important context to the observed patterns as seen in the descriptive analyses. We would like to retain the current format of the manuscript, with the main focus being on the descriptive analyses and maintaining the discussion in Section 3.3.2 and leaving most of the methods in Appendix B. We believe that these descriptive analyses of the distributions and variances are a fundamental observation needed for understanding the Allan Hills archive. The aim of the additional quantitative analysis is to offer an exploration into understanding potential drivers of the distributional patterns observed in the archives. Because this is an exploratory analysis and not a fundamental character of the archive itself, we would like to maintain the structure of the manuscript to allow these analyses to be the focus of the main conclusion of the manuscript.

To reduce the number of descriptive analyses we will move Figure 4, the correlation figure, to Appendix A, as it does not add a substantial contribution to the manuscript.

Specific Remarks

Section 2.2.2: The information about the precision of the water isotope and CH4 data would be a valuable addition to this section to gauge the differences between the discrete and the continuous data.

Thank you for noting precision of the measurements was missing from this section. The precision of the CH4 measurements at Oregon State University are 2.7 ppb (Epifanio et al., 2020, *Climate of the Past*) and for the University of Washington water isotope discrete measurements are 0.19 ‰ for δ^2H and 0.02 ‰ for $\delta^{18}O$ (Schauer et al., 2016, *Rapid Communications in Mass Spectrometry*).

In re-evaluating the CFA and discrete measurement offsets, we found that the numbers reported need to be updated. The mean and median offset is -6.55 and 2.31 ppb, respectively, with a standard deviation of 57.67 ppb. These large offsets, as stated in the text, are likely due to the depth registration issues with ALHIC 2201 due to ice fracturing in transit before processing in conjunction with stratigraphic offsets across the slab of the ice core due to apparent dipping layers. Intrusion of laboratory air is always a concern with CFA analysis, however the good agreement in the mean would suggest this effect is minimal. In-situ methane production is typically only seen in near-basal samples, and expected to be minimal at the depth ranges investigated here. As seen in Figure 3 and 5, many of the discrete methane measurements appear to match the CFA data quite well.

L192ff: I think all is fine with the solubility correction, but with the large error on the average offset it is not possible to make any quantitative statements. What is also missing is the magnitude of the solubility correction that was applied. If that is on the same order or smaller than the uncertainty of the offset than it is even less advisable to make any quantitative statements. I suggest rewriting this paragraph.

Thank you for pointing out the solubility correction magnitude was missing for this section. We will add in the following statement:

“For the ALHIC 2201 core, the solubility correction was 12.0 ± 1.6 % across eight campaign days between March 4-15th, 2024. For the ALHIC 2302 core, it was 5.8 ± 0.6 % across six campaign days between March 24 and April 15th, 2025.”

Because these corrections are small in comparison to the large error between the discrete and CFA CH4 data, we will revise the last sentence of the paragraph regarding “validating the solubility measurement” to:

“The consistency of the solubility correction across campaign days indicates that there was no systematic drift in solubility over the course of each campaign.”

Section 2.2.3+2.2.4: The information about how the delay times for each analyser were determined (and how large/variable they are) seems to be missing. What is also missing is how the missing data at sample breaks was handled. Depending on the measurement resolution a significant amount of data will represent a mixture of non-continuous ice. In the application to the discontinuous ice here, that might be okay, but a few sentences should be added for explanation.

Thank you for noting the missing information regarding the delay time method and missing data at sample breaks. As noted by the reviewer, we do not account for any mixing at the sample breaks because of the discontinuous ice (i.e., we are not missing any expected ‘time’ with missing depths). Any missing depths from sample processing are accounted for in the depth allocation to the data and are shown in the figures with gaps in the data.

In Section 2.2.4 the following will be added for clarity regarding ice breaks:
“Due to the discontinuous stratigraphy of the ice and cm-scale resolution of the analytes, we do not account for any mixing that occurs at ice core breaks.”

Information regarding the determination of the delay time will be added to Section 2.2.3 as follows:

“The time required for meltwater or air to travel from the melthead to each respective analyzer was measured at the beginning of each. For dust, travel time was measured from the time a stick of Milli-Q ice first contacted the melthead to the time the meltwater passed through the Abakus. For water isotopes, travel time was measured from the time a stick of Milli-Q ice first contacted the melthead to the point at which the water isotope signal had asymptotically approached its new steady-state value on the analyser. This travel time also includes the memory effect of the analyzer. Because Milli-Q ice contains no air and test glacier ice (e.g., Taylor Glacier ice) does not provide a clear contrasting methane signal, the methane travel time was determined in two parts. First, the travel time from the melthead to the debubbler was measured by timing the first appearance of bubbles from polar ice samples in the debubbler after melting Milli-Q ice. Second, using the standard air loop, the travel time from the debubbler to the methane analyzer was measured based on the time required for the signal to reach an asymptote. These two times were then added together to determine the total methane travel time.”

L274: The final sentence here is probably better placed as the second sentence of the paragraph.

We appreciate this suggestion and incorporated this fix as it provides a more logical flow to the paragraph.

Section 2.4: This section is missing the age-scale information for the EDC data. Later AICC is mentioned, but I think it would be better to state that here. What is also missing here is how the original EDC datasets where (sub/re)-sampled for the statistical analysis. It is important to be very explicit with this because especially the variance of the data and its range is very much dependent on the choices made here. The choices made here also need to be justified carefully considering the age density of the ALHIC data presented. Ideally the age densities should match as much as possible.

In this section, we will add the following for clarity:
"For the variance and accumulation scaling analysis described in Section 3.4 and Appendix B, the AICC2023 ice age was used."

To clarify subsampling, the following will be added to the end of Section 2.4:
"The correlation analysis in Appendix A for Figure A4 (Note: the current A4 will be moved to the main text) and Table A1 used 1000 randomly selected observations from the EDC 1 m-binned datasets for better comparison of correlation and histograms between the ALHIC and EDC datasets. For each correlation, 95% confidence intervals were estimated using 1,000 bootstrap iterations. All other analyses were not subsampled."

L295: should be "are due to depths not analysed"
Thank you for noticing this, we will update the sentence.

L321ff: I think the comparison Figure A4 should find a place in the main part of the manuscript. Especially if sections are discussed in detail here, as in the final sentence.

Thank you for this suggestion. We will move this to the main text as it provides context for the complications that arise in the dating and provides evidence of ages being a result of stratigraphic mixing.

L389: Should probably be "The average ALHIC water isotope values…"

Thank you for this suggestion as it is more correct and adds clarity; we will make this change.

Section 3: From my point of view there is an over emphasis, both in text and figure on the simple descriptive statistics of the data. I feel that it does not bring a lot to the arguments the authors are trying to make and could mostly be moved to the appendix, to shorten the main part of the manuscript.

This is addressed above regarding the discussion around the quantitative analysis in Appendix B and the focus on descriptive analytics.

Section 3.3.2: I think the approach used here is quite clever and deserves to not be hidden in the supplement: This is the data analysis that goes beyond simple descriptive statistics and adds quite a

bit of value to the paper, from my point of view. I would suggest presenting it a more prominently and extend the section.

We thank the reviewer for highlighting the value of the quantitative variance reduction and accumulation scaling analysis in Section 3.3.2 and Appendix B. We agree that this analysis offers important context to the observed patterns as seen in the descriptive analyses. We would like to retain the current format of the manuscript, with the main focus being on the descriptive analyses and maintaining the discussion in Section 3.3.2 and leaving most of the methods in Appendix B. We believe that these descriptive analyses of the distributions and variances are a fundamental observation needed for understanding the Allan Hills archive. The aim of the additional quantitative analysis is to offer an exploration into understanding potential drivers of the distributional patterns observed in the archives. Because this is an exploratory analysis and not a fundamental character of the archive itself, we would like to maintain the structure of the manuscript to allow these analyses to be the focus of the main conclusion of the manuscript.

To reduce the number of descriptive analyses we will move Figure 4, the correlation figure, to Appendix A, as it does not add a substantial contribution to the manuscript.

Section 3.3.1: I think the decile plots are probably best presented with the deciles as the x-axis labels not the data ranges. That would make the plots cleaner and would also make it easier to look at a specific decile without having to count the labels. Alternatively, the deciles could be added as an additional axis.

Thank you for this suggestion. Following this suggestion, we added the decile numbering below the labels, but we kept the ranges of values as we believe they provide context for understanding which climate states are over- or under-represented.

Appendix A: Both figures A1 and A2 include linearly interpolated values over data gaps, these should be removed.

Thank you for noticing this. We agree these gaps should be removed and will be updated in the next iteration of the manuscript.

Fig A2: It is quite surprising to see so large depth offsets (>10cm) between the replicate measurements. I suggest the authors add some detail on the precision of the depth assignment to the discussion.

As noted throughout the manuscript, there are dipping layers across the ice core and therefore the adjacent replicate core will not be identical to the other at the same depth (Kirkpatrick et al., 2025). Additionally, as noted in Section 2.2.3, ALHIC was heavily fractured in transit to OSU, making the depth assignment, especially between replicates, very challenging. Between the dipping layers, fractured ice complicating the depths assignments, and the assumption of constant melt rates for depth registration, we believe these analyses are robust. To aid the discussion of the analytical procedures, we added the following to Section 2.2.3 at the end of the first paragraph:

"Offsets between replicates (Fig. A2) arise due to a combination of cm-scale uncertainty in depth registration due to ALHIC 2201 ice fracturing in transit, dipping layers across the ice core slab (see

Section 3.2), and differences in melt rates between replicates (as depth registration to the data assumes a constant melt rate between timed markers every 10 cm).”

Additionally, we will add clarity to line 239: “…assuming a constant travel time that was pre-determined and a constant melt rate. The travel time is expected to stay fairly constant as this is dependent mainly on tubing diameter and length and peristaltic pump speeds that were all held constant.”