The basal ice layers of the Antarctic ice sheet are often affected by bed topography, basal materials, ice flow, and ice deformation, potentially resulting in folding and disturbed stratigraphy. These processes make it challenging to establish reliable chronologies and accurately interpret climate and environmental records preserved in basal ice, particularly in blue-ice areas where the stratigraphic structure can be highly complex. In this study, the authors use 3D ECM measurements to identify planar and steeply dipping layers and establish a dip-adjusted depth scale to account for variations in layer inclination. They further investigate the alignment of multi-proxy records using this adjusted depth scale and propose a cutting method following layer dip to obtain samples corresponding to the same stratigraphic horizon. This approach has the potential to improve stratigraphic interpretation and sample collection in ice cores from blue-ice areas.

The study addresses an important methodological challenge in Antarctic ice-core research and presents a potentially useful approach. However, several aspects require further clarification and justification, particularly the implementation of the 3D ECM method and the interpretation of comparisons between the basal ice samples and other ice-core records. The consistency and interpretation of some proxy measurements also warrant further discussion. Addressing these issues would improve the methodological transparency and strengthen the scientific conclusions. I consider the manuscript suitable for publication in The Cryosphere after major revisions addressing the comments and questions below.

**Major comments:**

1. The results presented in this paper are primarily based on 3D ECM conductivity measurements; however, the uncertainty associated with the determination of layer dip and the subsequent depth correction is not sufficiently discussed. How do uncertainties in the measured dip angles propagate into the corrected depth scale? The authors should quantify or discuss the associated uncertainties and assess their potential impact on the alignment of the multi-proxy records.

The description of the 3D ECM method is not sufficiently clear to readers. For

example, for the 30 cm ice sections 228_4 and 230_4, with ECM tracks spaced at intervals of 5 or 10 mm, it is unclear why 9 tracks were measured on the half-width faces and 20 tracks on the full-width face of section 228_4, whereas no tracks were measured on the half-width face of section 230_4. Furthermore, Figure 1b shows data from the half-width face of section 230_4, but no data from its full-width face. The calculation of the dip angle is also insufficiently explained, and Equation (1) is difficult to follow. A schematic figure illustrating the calculation procedure for the dip angle would greatly improve the clarity of the method.

2. Line 336-338: The authors report good agreement between replicate ion measurements. However, no figures are provided to demonstrate the consistency of Na, $NO_3^-$, Mg, and $Ca^{2+}$ concentrations relative to $Ca_{TE}$, or of $SO_4^{2-}$ concentrations relative to $S_{TE}$. The corresponding plots should be included in the Supplementary Information to substantiate this statement.

3. As shown in Figure 6, the concentrations of $Cl^-$ and Ca differ considerably among replicate samples, with substantially greater variability than that observed in the water isotope measurements. The authors should discuss the possible reasons for these discrepancies.

4. Line 414-415: The aeolian dust variability across glacial-interglacial cycles reported for the ALHIC1903 core is limited to MIS 6-5e, whereas the ice samples from sections 228_4 and 230_4 are older than 1.5 Ma, for which no continuous ice core record is available. Given this substantial difference in age and the lack of a continuous reference record, is a direct comparison of particle size and concentration between the two ice cores scientifically justified? The same concern applies to the comparison of the chemical concentrations. Furthermore, could an individual dust or air mass transport event have caused transient changes in ion concentrations that happened to be captured in sections 228_4 and 230_4? If so, how can the authors distinguish such episodic signals from longer-term climatic or environmental variations? The authors should

discuss these limitations and provide a stronger justification for the comparisons.

5. Line 454: The acid-leachable $Mg_{TE}$ concentrations are substantially higher than the ionic $Mg^{2+}$ concentrations. The authors attribute this difference to the presence of particulate, largely insoluble Mg bearing silicates that do not readily dissolve in meltwater and are therefore not detected by IC measurements. However, if this interpretation is correct, why is such a discrepancy observed only for Mg? Particles should also contain other elements, such as Ca, which could similarly contribute to the acid-leachable concentrations. The authors should clarify this discrepancy and explain why a comparable difference is not observed for other elements.

**Other comments:**

1. The two parts of the Introduction are closely interconnected in terms of content and logical flow. However, the first part is substantially longer than the second. To improve the coherence and overall structure of the Introduction, it is recommended that the two parts be merged into a single section. In addition, redundant statements should be removed, such as the first sentence of Line 39.

2. The descriptions of the analytical techniques used for the multi-proxy measurements are overly detailed, given that these methods are commonly used in ice core studies. It is recommended that these descriptions be streamlined to improve the overall structure of the manuscript and enhance its conciseness. Some parts can be put into the Supplementary Information.

3. Question marks appear at several locations in the manuscript (e.g., Lines 190, 268, and 468). These instances should be checked and corrected.

4. Line 199: What is the rationale for rinsing all samples with methanol after scraping off the outer layers? The purpose of this step should be clarified.

5. Line 216: The time periods represented by MIS 6 and MIS 5e should be specified when these terms are first introduced in the manuscript.

6. Equation 2: Why is ammonium ($NH_4^+$) not included in the equation?

7. Line 292: The rate of increase in atmospheric $^{40}Ar_{atm}$ should be 0.0066 ± 0.007‰.

8. Line 317: The age difference between 1.65 Ma and 1.58 Ma is 0.07 Ma, which slightly exceeds the relative dating uncertainty of 0.066 Ma.

9. Figure 4: The colors of the boxes in this figure are consistent with those used in Figure 3. However, the caption states that the water isotope measurements are represented by blue, green, and yellow boxes, which do not match the colors shown in the figure. Please revise the caption accordingly.

10. Line 333-335: This statement is redundant, as the same information has already been provided in the Methods section, and should be removed.

11. Figure 5d: The sample IDs of the six $^{40}Ar_{atm}$ measurements should be provided in the Method section.

12. Line 349: The figure showing the measured ionic balance should also be included in the Supplementary Information.

13. Figure 7: The figure captions contain several errors, particularly inconsistencies between the colors described in the captions and those used for the lines in the figures. These inconsistencies make the figures confusing, which in turn makes Section 3.3 difficult to follow. The authors should carefully check and correct all figure captions to ensure that the color descriptions accurately match the figures.

14. Line 368-369: Why do the particle concentrations measured using the Coulter Counter not show clear alignment on dip-adjusted depth? The authors should clarify the possible reasons for this discrepancy. Furthermore, it is not appropriate to state that the dip-adjusted depth scale was applied to all the measurements listed in the manuscript, including particle size and concentration.

15. Line 393: The slope in Figure B1 is 0.257, whereas the text reports it as approximately 0.27. The authors should ensure that the value is reported consistently throughout the manuscript.

16. Line 487-490: What is the scientific significance of the relationship between $CO_2$ and $\delta^{18}O_{ice}$? The observed inverse relationship may simply reflect random variability in the data rather than a meaningful relationship. The authors should clarify whether this correlation is statistically significant and provide a physical explanation for the observed pattern.

17. Line 574: The relevant figures should be explicitly cited in the text when discussing their corresponding results.

18. Line 600: This information has already been presented in Line 480 and does not need to be repeated.