Author comments:

We thank the reviewer for taking the time review the manuscript and provide thoughtful and constructive feedback. Reviewer comments and suggestions have been addressed; please see the below replies in blue italicized text.

Reply to RC2 (Anonymous reviewer):

The Harlan et al. paper is the latest in a series of publications on the new MBS ice core, one of the rare high-resolution records of the last millennial. Harlan et al. present the CFA dataset, including meltwater electrolytic conductivity, sodium (Na+), ammonium (NH4+), hydrogen peroxide (H2O2), and insoluble microparticle measurements.

Despite some language inconsistencies, the paper is well written and easy to read. Figures have been tested for several types of colorblindness and seem adequate in terms of colors used. The quality of the figures is good and the coherent use of colors in the different figures is appreciable.

However, I have a few concerns, especially about the lack of discussion about errors and data quality assessment, as well as a proper description of the interest and novelty of such a long and highly resolved record. The different major and minor comments are discussed below.

> *We thank the reviewer for acknowledging the value of our work, and for providing comments which will improve the manuscript.*

Major comments:

The dataset presented here is unique, with fine resolution and spanning a millennium. However, I feel that a key point is missing, namely the emphasis on what could be done with these new records, as well as highlighting the importance of such longer time records. Readers and future users would need information on how valuable the dataset is and on its potential future use.

> *Thank you for highlighting the significance of the MBS CFA record. We have highlighted in the introduction (lines 31-34) some of the exciting work that has been published so far using the MBS records. We intentionally refrained from including further descriptive interpretation of the record, in regard to proxy interpretation and potential applications of the data for climate reconstruction, as the defined scope of the ESSD data descriptor manuscript type does not include interpretation or analysis of data presented. We do agree, however, that highlighting potential use cases would help to highlight the importance of this type of long, high-resolution climate record.*

This is particularly important as the actual minimum resolution of this dataset is considered to be 3 cm, the same resolution as the discrete (complementary) dataset published along the paper of Vance et al. (2024a). This also raises the question: what is the value, and the reliability, of a dataset with a resolution of 1 mm? A topic only partly addressed by the authors.

> *Thank you for raising these concerns. We agree that this could be further clarified in the text. We will add text to the data resolution section which addresses the following points:*
>
> - *While we present the 3 cm dataset as the "minimum resolution" dataset, our language was perhaps unintentionally misleading. While the effective smoothing of the peroxide dataset from the 2018 campaign results in an effective resolution of 3 cm, every other species has lower resolution, including at or below 1 cm in the case of the conductivity*

signal. Resolution of 1 cm is a substantial improvement over what could be reasonably achieved with discrete sampling of a core of this length.

- *In an effort to provide a concise and user-friendly dataset, we chose to provide all analytes on the same resolution (3 cm), rather than providing each analyte on the corresponding minimum depth resolution for that analyte (e.g. 1 cm for conductivity, 2.5 cm for NH4, 3 cm for H2O2), despite the fact that this underrepresents the resolution of the higher-resolution species like conductivity. Additionally, providing both a conservatively smoothed 3 cm dataset together with the 1 mm resolution dataset allows users to choose the best resolution for their particular uses.*

- *While the 1 mm resolution dataset is below the "actual" resolution of the dataset when accounting for data smoothing, this high-resolution dataset can be helpful in identifying extreme events in the dataset. Because the 3 cm averaged resolution dataset may smooth extreme events by reducing the amplitude and producing an artificially elevated baseline signal in the vicinity of extremes, it can be useful to provide the higher resolution dataset as a point of reference.*

On line 31, it is confusing to read a mention of ice cores in the plural when the paragraph begins with the presentation of the new Mount Brown South ice core in the singular (line 29). I think it would be interesting to first present the site and the multiple cores (currently section 1.2) and then introduce Continuous Flow Analysis (currently section 1.1). This would also provide a more logical sequence for the rest of the content. In addition, the sub-section presenting these different cores could be expanded, typically by adding information on the length and position of Alpha, Bravo and Charlie cores in relation to the main core. Consideration should also be given to adding a paragraph (or a dedicated sub-section) on what has already been done on these different cores, and possibly summarizing the main findings of Crockart et al. (2021), Jackson et al. (2023) and Vance et al. (2024a). This would also provide more clarity on the addition of this paper compared to the other publications, particularly compared to the Vance et al. paper (2024a) which already mentions the CFA data (even if it's not clear whether Vance et al. use it or not). Finally, section 4.2 on MBS chronology could be added to this part of the text instead of being presented in the data processing section (while it has not been processed in this paper).

Many good points are raised in this comment, which we will address individually point-by-point below:

- *It is confusing to read a mention of ice cores in the plural when the paragraph begins with the presentation of the new Mount Brown South ice core in the singular (line 29).*

  We agree that we worded this in our initial manuscript in a confusing way. We will clarify in the text when we are referring to Mount Brown South (the ice core site), the Mount Brown South ice core array (ice cores plural), and the Mount Brown South ice core (the MBS-Main core specifically).

- *It would be interesting to first present the site and the multiple cores (currently section 1.2) and then introduce Continuous Flow Analysis (currently section 1.1)... In addition, the sub-section presenting these different cores could be expanded, typically by adding information on the length and position of Alpha, Bravo and Charlie cores in relation to the main core.*

  We will revisit these sections and introduce the ice core site (currently section 1.2) before introducing continuous flow analysis. We will also briefly introduce the Alpha, Bravo, and

Charlie shallow cores, referring the reader to the MBS2023 chronology paper (Vance et al., 2024a) for further information, as we do not present any data from these cores in this manuscript.

- *Consideration should also be given to adding a paragraph (or a dedicated sub-section) on what has already been done on these different cores*

  In lines 31-34 of the introduction, we briefly introduce some of the other works based on the MBS ice cores. As this manuscript is a data descriptor, and discussion of interpretation of other studies performed on the MBS ice cores falls outside the scope of a description of the CFA impurity record. Unless indicated otherwise by the editorial team, we would prefer to refrain from further discussions of other studies in this work.

- *…particularly compared to the Vance et al. paper (2024a) which already mentions the CFA data (even if it's not clear whether Vance et al. use it or not).*

  The MBS2023 chronology presented by Vance et al. (2024a) was developed prior to and independently of the production of the CFA impurity record presented here, and relied on stable water isotopes and discrete ion chemistry (as stated in l.168-169). We will clarify in these lines that MBS2023 did not use the CFA impurity record, and that this dataset is analytically independent from the data presented in Vance et al. (2024a) and the discrete ion chemistry dataset (Vance et al., 2024b).

- *Finally, section 4.2 on MBS chronology could be added to this part of the text instead of being presented in the data processing section (while it has not been processed in this paper).*

  We included section 4.2 in its current place in the text as we believe that the chronology information is most relevant in regards to the usage of the dataset (as we provide a decadally averaged dataset using this chronology), however understand that this could be confusing as the chronology was not produced as part of this manuscript/dataset. We will move the description of the MBS2023 chronology into the introduction, to follow our introduction of the MBS ice core site (what is currently section 1.2).

Are there any traces of melt layers and wind crusts in the cores? Have their (potential) influences been considered in the various recordings? Even if it has been said that one of the criteria for selecting a site is a minimum of snowmelt in summer, it seems essential to document their occurrence and explain the potential influence of these melt layers and wind crusts on the recordings, especially for the 1 mm resolution dataset.

In the processing of the MBS ice core(s), we have not observed melt layers and in fact have not yet documented any melt layers across the full 295 meter Main core. Certainly, in the upper 20-25 meters of the ice core, where one would logically expect a potential increase in melt due to a warming climate, we have not recorded any observations of melt, even during summer periods. The annual mean temperature of the MBS site is -27.9 degrees C, and mean summer (DJF) air temperatures are -18.4 - cold enough to preclude melt (Vance et al., 2024a).

Nonetheless, we have observed numerous thin (~1mm thick) ice layers in the core which have been described as "bubble free layers." These features are described in Section 2.6 of Vance et al. (2024a) and are common in Antarctic ice cores (including Law Dome and WAIS among others), and while they have been poorly studied in the past, are an active area of research in the

MBS cores. These features have been investigated in the Law Dome ice core in the work of Zhang et al. (2023) and are distinct from melt features. These features are also distinct from "wind crusts," but rather appear to be associated with regional scale atmospheric circulation, such as mid-latitude blocking and meridional moisture transport. Work is ongoing to investigate the chemistry and isotope signatures of these bubble free layers, and their atmospheric drivers. For the purposes of this study, the sub-millimeter scale of these features would be overcome by the internal smoothing of the CFA system, so we do not discuss them in the scope of the CFA dataset.

My major concern is the absence of any discussion of the accuracy and precision of the measurements, which is an essential point in a data paper.

Sub-section 6.3 only presents the limit of detection (LOD), i.e. the smallest concentration that can be measured by the instrument, but not how precise and accurate these measurements are. The relative standard deviation (RSD) - the standard deviation of concentrations of the standards divided by their known levels - of the lowest level standard can be used as a measure of precision, as in Griemann et al. (2022). With regard to accuracy, intermediate standards could be considered as samples (i.e. not used for calibration) and used to define the accuracy of measurements by evaluating the difference between their known concentration and the concentration measured during the run.

We disagree that accuracy and precision are not discussed, perhaps not using this exact wording, but throughout the text (i.e. in sections 5 and 6) we carefully list uncertainties and errors on the data that influence the final accuracy and precision. For example, in Section 4.1 on the depth scale, we discuss the uncertainties on depth scale and provide numbers on these and in section 6 we specifically discuss the analytical precision and data quality. However, in addition to this, we will add the relative standard deviations of the standards used (following Griemann et al. (2022), as suggested) alongside the LOD of the analytes in Table 2, as well as text describing this in Section 6.2.

l.267: Have you compared the 3 cm resolution Na+ dataset with Vance et al.'s (2024a) discrete dataset? This could be used as (partial) data quality assessment.

We agree that a comparison to the published discrete dataset would provide an interesting interlaboratory comparison, however we point out that this might be misleading if presented purely as a data quality assessment tool. The discrete measurements were analyzed using ion chromatography, and there is a similar (if not higher) likelihood of e.g. laboratory contamination for these measurements as for the CFA measurements.

We ultimately chose not to present a comparison to the Vance et al. (2024a) discrete dataset, as the defined scope of data description manuscripts published in ESSD states that "any comparison to other methods is beyond the scope of regular [data descriptor] articles."

Similarly, no error is mentioned for the third dataset presenting a decadal scale record. A global error calculation - including measurement and dating errors - should be associated with the decadal scale record.

We are unsure what the reviewer means by a "global error calculation" here. However, the errors associated with the measurements are described in the text of the manuscript, which is linked by DOI to the dataset as available from the Australian Antarctic Data Centre. Therefore, users of the decadal scale data set will have easy and open access to the descriptions in this text when accessing and using the dataset. Additionally, further discussion of layer counting error (as *well as a comparison to the WAIS and Law Dome ice cores) is thoroughly detailed in the manuscript accompanying the MBS2023 chronology (Vance et al., 2024a), which is referenced in the manuscript text.*

Section 4.4 on signal delay time is not detailed enough. Are the numbers and errors the result of statistical analysis? How many estimates have led to these values? How regularly were the bulk and individual delay times measured? And, in practice, how is the "total" delay time applied to the different measurements?

We thank the reviewer for raising the matter of delay time estimation. We will include in the text more detail on the calculation of the bulk delay times based on the individual sample runs, and how many/often standard runs were used to make these calculations.

I don't see how section 6.3 (l.256-261) can be used to verify data quality and depth scale accuracy. Firstly, it should be specified what data are involved here (conductivity) and the comparison between the conductivity measured by CFA and the discrete measurements of nssSO4 by Vance et al (2024a) should be clearly mentioned. Secondly, approximating 90 cm as equivalent to 3 years is not correct: it is mentioned in the text that accumulation is highly variable on an interannual scale. Moreover, even if the accumulation were more or less constant, we assume that the authors are using a sliding average of 90 cm on the raw data (observed depth), but density changes with depth, so 90 cm at the surface does not represent the same number of years as at a depth of 200 m when the snow has been compacted into ice. Thirdly, I disagree with the statement "we are able to identify many of the volcanoes reported in Vance et al. (2024a)". Looking at Table A2 in the Appendix, only 16 of the 32 volcanoes identified in Vance et al. (2024a) are also present in the conductivity record, i.e. 50%. This deserves an attempt at an explanation (for example, it is possible that the relative proximity of the coast induces a greater presence of impurities resulting in a higher background noise in the conductivity measurements).

Based on these comments, we understand that this section of the text requires some clarifications. We do state in the text of section 6.3 that we are using the CFA conductivity measurements (presented in this manuscript), and comparing to volcanoes identified in Vance et al. (2024a), wherein the methods of volcanic identification are described. We will respond to the individual comments about the content of this section point by point below:

- I don't see how section 6.3 (l.256-261) can be used to verify data quality and depth scale accuracy.

  Our intention in this section is to demonstrate the ability of the CFA record described here to correspond with signals identified in the discrete dataset. We consider that a matching eruption signal to within one year depth uncertainty is a basic validation of the general accuracy of the depth scale used to prepare this dataset. We will rephrase the opening of section 6.3 to better describe our intentions with this matching exercise.

- *Firstly, it should be specified what data are involved here (conductivity) and the comparison between the conductivity measured by CFA and the discrete measurements of nssSO4 by Vance et al (2024a) should be clearly mentioned.*

  It is stated in line 256 that we use conductivity peaks above 3 σ in this matching exercise, compared to the data provided in Vance et al. (2024a). We will add text to this section to indicate that the Vance et al., (2024a) dataset uses the non-sea salt sulfate signal to identify volcanic eruption signals.

- *Secondly, approximating 90 cm as equivalent to 3 years is not correct: it is mentioned in the text that accumulation is highly variable on an interannual scale. Moreover, even if the accumulation were more or less constant, we assume that the authors are using a sliding average of 90 cm on the raw data (observed depth), but density changes with depth, so 90 cm at the surface does not represent the same number of years as at a depth of 200 m when the snow has been compacted into ice.*

  We understand that 90 cm does not correspond to exactly 3 years throughout the full core, however, as we are comparing volcanic signal depths identified in the CFA conductivity to the depths of corresponding volcanic signals identified in Vance et al. (2024a), we chose to use a sliding window of fixed depth. We state in the section that this is an "approximation" of 3 years, as this is true as an average approximation across the core. As we have clearly described the accumulation variability characteristic of the site, we consider that describing this as an "approximation" is not inaccurate. We will update the text to clarify that this is indeed only an approximation and does not represent exactly three years throughout the length of the record, due to accumulation variability and density changes downcore.

- *Thirdly, I disagree with the statement "we are able to identify many of the volcanoes reported in Vance et al. (2024a)". Looking at Table A2 in the Appendix, only 16 of the 32 volcanoes identified in Vance et al. (2024a) are also present in the conductivity record, i.e. 50%.*

  We will rephrase the text in line 257-258 to the following: "Using this method, we are able to identify 16 of the 32 volcanic events reported in Vance et al. (2024a) to within one year age-at-depth uncertainty."

- *This deserves an attempt at an explanation (for example, it is possible that the relative proximity of the coast induces a greater presence of impurities resulting in a higher background noise in the conductivity measurements).*

  The reviewer is correct here, in that as we are comparing bulk conductivity in the CFA record with the discrete non-sea salt sulfate record, there will be inherent differences in the two datasets. However, we have intentionally avoided substantial interpretation of the record here, as ESSD specifies that significant data interpretation is beyond the scope of a data descriptor manuscript. However we will add the following statement to the text:

  "We consider the ability of the CFA conductivity signal to identify half of the volcanic events found in the non-sea salt sulfate signal by Vance et al. (2024a) across the MBS record to be an external validation of the CFA conductivity record described here. We attribute existence of peaks that are not identified both datasets to the different datasets used (comparing conductivity to non-sea salt sulfate), as it is likely that some peaks in the conductivity signal are related to sources of other soluble ions, in addition to volcanic sulfate."

Minor comments:

Some inconsistencies in the text:

Two periods of CFA measurements are mentioned in the text but using different terms (CFA sampling/measurement/melting campaign) (l.54, l.218-219, l.238). The same wording should be used to facilitate understanding.

We will update the text to ensure consistent wording throughout.

The depths given for dry-drilled and wet-drilled sections are not the same throughout the text: for example, in l.95-96, dry-drilled and wet-drilled sections are respectively at ~4-94 m and ~95-295 m vs. 5-95 m and 96-295 m in l.218-219.

We will update the text to ensure that the relevant depth ranges are described accurately and consistently throughout the manuscript.

There is a problem with the table numbering. The first Table mentioned in the text is actually Table 2 (line 211). The numbering of the figures and the order in which they are presented should be changed in the text accordingly.

We thank the reviewer for pointing out this error in table numbering. We ensure that the tables are presented in the correct order in the revised manuscript.

l.2: In the abstract, the authors mention a mean annual accumulation estimated at 20-30 cm ice equivalent. However, 20 cm is never mentioned in the main text. The abstract should only contain information that is present in the main text.

We will update the abstract to reflect only the information in the manuscript.

Figure 1 (near l.61): some values of lat-lon would be welcome.

We will update Figure 1 to include lat/lon values.

Legend of Figure 2 (near l.80): "Shaded areas indicate cuts made with a bandsaw." Do you mean dotted lines?

We thank the reviewer for pointing this out. There is an error in the figure caption for Figure 2. It should state "dotted lines indicate cuts made with a bandsaw. Shaded area (hatched line) indicates surfaces planed for intermediate layer core scanning." We will update the caption to correct this.

l.90: A few more details on the scraping of horizontal ends could already be given here: at the very least, mention how much thickness is removed and insist that this is taken into account in the depth logs, especially as one of the datasets is published at millimeter scale.

We will add text at line 91 stating the following: "1 - 2 mm of ice was removed in this cleaning process. All sample pieces were measured after cleaning, and any ice removed in cleaning is accounted for in the depth record."

l.95: The authors should mention why the dry-drilled section only begins at a depth of 4 meters and how the chronology of these first 4 meters is carried out.

As is common with intermediate/deep ice cores, the drilling took place from a trench dug into the firn at the ice core site. This accounts for the 4 meters missing from the top of the Main core. These 4 m were matched with the overlapping shallow cores drilled nearby during the same season. This is described in the manuscript by Vance et al. (2024a), and as we do not present the *data from the shallow cores or the chronology, we refer readers to Vance et al. (2024a) for this information.*

l.114: It would be interesting for the readers to mention which type of analysis will be performed in the future.

At time of submission, it is not known exactly which analyses will be performed in the future. As such, we consider this to be beyond the scope of this data descriptor manuscript.

l.137: What are the resolution and accuracy of this cable-driven rotary encoder (in comparison to the previous system)?

The Waycon SX-80 has a sensitivity of $\gamma = 25$ counts mm^{-1}, and linearity of +/- 0.15%. We will add text that describes the specifications of the instrument at line 137-138.

l.145: The last sentence of the paragraph raises a question: why would we need more details about the gas extraction system if it was not used for data collection? The answer is only 3 pages later. I suggest adding a few words alluding to the effect of gas extraction on the CFA data presented here.

Thank you for the suggestion. For brevity and to avoid unnecessary repetition, we will amend the last sentence of the paragraph to state "Further detail on the gas extraction system and its impact on measurement quality follows in section 6.1 below."

l.159: What is the impact of inaccuracies of one or two millimeters on the 1 mm dataset? This should be estimated and mentioned in the text.

The impact of inaccuracies on the scale of 1 - 2 mm are described in section 4.1 describing the depth scale. Such inaccuracies would be well below the annual resolution of the record and are unlikely to be of climatic interest to users of this dataset.

l.165: It is mentioned that the procedure for correcting differences in measurements of the same stick is presented in Vance et al. (2024a). However, Vance et al. (2024a) state that "The scaling and shift factors will be described in detail as the CFA trace chemistry and water isotope datasets are developed and published". So it seems that information is missing here.

We apologise for this omission. This was a two-step process: first, the depth scale for the entire Main core needed to be developed taking into account small differences in field to lab core lengths, and then also small differences in the IC (discrete chemistry) stick lengths. This is described in detail in Vance et al. (2024a). However, as the reviewer states, the shift and scaling factors (and CFA datasets) were still under development at the time of publication of Vance et al. (2024a). These discrepancies were accounted for in the assignment of the top depths for each meter-long core section. Further description of the depth scale procedures can be found in Gkinis et al. (2024). We will update the text to more clearly describe the depth adjustment process.

l.184: For readers unfamiliar with CFA measurements, it might be interesting to explain the principle of calibration with standards run at the beginning and end of each measurement. Is it a single calibration with some standards run at the beginning and others at the end? Or two distinct calibrations to account for e.g. measurement drifts?

The standards run procedure is relatively standardized across most CFA systems. In line 188, we refer to Kaufmann et al. (2008), wherein there is a very clear and concise description of the *standard calibration procedures, which we use also here. As this is a data descriptor manuscript, and not a methods paper, we will update the text to more specifically refer the reader to the work of Kaufmann et al. (2008).*

l.189: Shouldn't the MATLAB script mentioned be made available in a code availability section, as required by the ESSD guidelines?

The MATLAB scripts used here only perform simple calculations in an automated manner. All computations performed in the MATLAB scripts used in data processing are described in the text of the manuscript, and as such, we do not consider the scripts themselves to provide significant additional information to the reader. Additionally, the majority of similar ESSD manuscripts (e.g. Erhardt et al. (2023)) do not include scripts used for simple data processing/calibrations.

l.205-206: Attention should be paid to the significant figures used here (either 79 ± 4 seconds or 79.x ± 3.6 seconds).

We thank the reviewer for pointing this out. We will update the text to ensure consistency of significant digits throughout.

l.254: Is Figure 4 needed? It is not really discussed in the text and is quite redundant with Table 2.

We find Figure 4 to be a helpful means of visualizing the range of variability of the dataset, however we will consider moving it to the supplementary materials if the manuscript length is a concern.

l.279: When mentioning "baseline values", do you mean "true" observed baseline or the limit of detection? It seems more accurate to define the LOD as a threshold.

The baseline values referred to here are the measured MilliQ (Merck RiOs™16 MilliQ) levels. We will update the text in line 279 to clarify this.

Figure 5:

1) The last data point of each complete series reaches 0. If this is an artifact of the plotting (which it probably is), the last point of each series should be removed.

The reviewer is correct that this is a plotting artifact. We will correct this in the revised figure.

2) It looks like there are several points reaching 0 in the H_2O_2, Na^+ and NH_4^+ series. If this is the case, these points are probably below the LOD/baseline (especially for H_2O_2 and Na^+) and should be removed according to text line 279. It may be interesting to try to plot the LOD for these series (even though they are low for H_2O_2 and very low for NH_4^+).

As described in the reply to the comment on Line 279, MilliQ the baseline value used was the threshold for data removal (which will be clarified in the revised text). We will add the LOD to the plot in Figure 5 to provide a visual representation of the information provided in Table 2.

l.286: Is there a reference to confirm that the layer thinning is negligible at this ice core site? This is surprising, given that layer thinning already has a significant effect at 80 m depth in records such as that from Philippe et al. (2016). I also recommend adding the cause of this "layer thinning" (due to strain rates) to the text, to leave no doubt for the reader.

Layer thinning is expected to be small at this site, with a bedrock depth of approximately 2000 metres, as discussed in Vance et al., 2016. However, the reviewer is correct that there will be some layer thinning due to strain across the core, and we will note this in the text. Layer thinning does not affect the use or interpretation of this dataset (which is already on an established chronology), but is of course taken into account when developing accumulation records (regardless of how small layer thinning is expected to be).

Technical corrections

Pay attention to sentence syntax (same word used several times in the same sentence):

l.2: the second word 'accumulation' can be deleted.
l.184: twice the word 'run' in the same sentence.
l.223-224: the first part of the second sentence (this gives a smoothing effect) repeats the first sentence (the dynamics lead to smoothing).
l.226: 'calculated' used twice in the same sentence.
l.227: similar as previous comment with 'using' and 'use'.
l.272: similar as previous comment with 'applying' and 'applies'.

We thank the reviewer for pointing these out. We will revise the text to avoid unnecessary repetition.

Some inconsistencies in the writing:

l.99: lowercase after the ':' (as in l.7 for example).
There should always be a space between the number and the units (see ESSD guidelines): l.123, l.163, legend of Table 1, l.227, legends of Figures 4 and 5 (1 μm)
(e.g. l.152) The word 'meltrate' is sometimes written 'melt-rate'.
l.178-179: standardize the expression "a # step calibration" or "a # step calibrations".
l.218-219: interval values (in this case, depth values given in brackets) must either be joined to the dash or separated by a space, but must be consistent throughout the text (including in Table 1 or in other intervals like in l.95-96, l.139-140, legend of Table 1, l.227, …).
l.225: CFA system (with a lower-case s, for consistency).

We thank the reviewer for identifying these typographical errors and inconsistencies. We will update each instance to ensure the text is correct and consistent with ESSD guidelines.

Some bibliography citations need to be revised:

General question: which order do you use to cite multiple references in text (for example, in l.112-113)?
l.100: should be "(Bigler et al., 2011)".
l.122: should be "in Dallmayr et al. (2016)."

We will cross check the references to ensure that they are presented correctly and consistently throughout the text.

Purely technical corrections:

l.18: delete the comma before (Legrand and Mayewski, 1997).
We will correct this.

l.122: a point is missing after the citation.

We will correct this.

l.148: "The position derived *from* melt-rate data".

This is correct as written, as we refer here to the melt-rate data which was derived from the position of the core at each time step (we will revise this as "position-derived melt-rate" for clarity).

l.199: "(15 seconds for both *CFA setup* systems)".

We will revise the text to read "(15 seconds for both the 2018 and 2019 CFA setups)"

l.219: meltrate (instead of metlrate).

We will correct this.

l.220: the second parenthesis) is missing at the end of the sentence.

We will correct this.

Table 1: min-1 should be in superscript.

We will correct this.

l.229: min-1 should be in superscript.

We will correct this.

l.254: a point is missing at the end of the sentence.

We will correct this.

Legends Figures 4 and 5: perhaps the expression should be "particles > 1 µm per ml".

We will update the figures to use this phrasing.

l.272: I suggest adding a 'by' before 'applying'.

We will correct this.

Table A2: a hyphen is missing at 213.58 m.

We will correct this.

References:

Grieman MM, Hoffmann HM, Humby JD, et al. Continuous flow analysis methods for sodium, magnesium and calcium detection in the Skytrain ice core. Journal of Glaciology. 2022;68(267):90-100. doi:10.1017/jog.2021.75

Philippe, M., Tison, J.-L., Fjøsne, K., Hubbard, B., Kjær, H. A., Lenaerts, J.T.M., Drews, R., Sheldon, S.G., DeBondt, K., Claeys, P., and Pattyn, F.: Ice core evidence for a 20th century increase in surface mass balance in coastal Dronning Maud Land, East Antarctica, The Cryosphere, 10, 2501–2516, https://doi.org/10.5194/tc-10-2501-2016, 2016.

References:

Erhardt, T., Jensen, C. M., Adolphi, F., Kjær, H. A., Dallmayr, R., Twarloh, B., Behrens, M., Hirabayashi, M., Fukuda, K., Ogata, J., Burgay, F., Scoto, F., Crotti, I., Spagnesi, A., Maffezzoli, N., Segato, D., Paleari, C., Mekhaldi, F., Muscheler, R., Darfeuil, S., and Fischer, H.: High resolution aerosol data from the top 3.8 ka of the EGRIP ice core, Earth System Science Data Discussions, 2023, 1–21, https://doi.org/10.5194/essd-2023-176, 2023.

*Gkinis, V., Jackson, S., Abram, N.J. et al. An East Antarctic, sub-annual resolution water isotope record from the Mount Brown South Ice core. Sci Data **11**, 986 (2024). https://doi.org/10.1038/s41597-024-03751-w*

Grieman M. M., Hoffmann H. M., Humby J. D., et al. Continuous flow analysis methods for sodium, magnesium and calcium detection in the Skytrain ice core. Journal of Glaciology, 68(267):90-100. doi:10.1017/jog.2021.75, 2022.

Vance, T. R., Abram, N. J., Criscitiello, A. S., Crockart, C. K., DeCampo, A., Favier, V., Gkinis, V., Harlan, M., Jackson, S. L., Kjær, H. A., Long, C. A., Nation, M. K., Plummer, C. T., Segato, D., Spolaor, A., and Vallelonga, P. T.: An annually resolved chronology for the Mount Brown South ice cores, East Antarctica, Climate of the Past, 20, 969–990, https://doi.org/10.5194/cp-20-969-2024, 2024a.

Vance, T. R., Abram, N. J., Gkinis, V., Harlan, M., Jackson, S., Plummer, C., Segato, D., Spolaor, A., Vallelonga, P., Nation, M. K., Long, C., and Kjær, H. A.: MBS2023 - The Mount Brown South ice core chronologies and chemistry data, Ver. 1, Australian Antarctic Data Centre, https://doi.org/doi:10.26179/352b-6298, accessed: 2024-09-07, 2024b.

Zhang, L., Vance, T. R., Fraser, A. D., Jong, L. M., Thompson, S. S., Criscitiello, A. S., and Abram, N. J.: Identifying atmospheric processes favouring the formation of bubble-free layers in the Law Dome ice core, East Antarctica, The Cryosphere, 17, 5155– 5173, https://doi.org/10.5194/tc-17-5155-2023, 2023.