We thank Reviewer 2 for their detailed and helpful comments.

Major Comment 1: Dust in ice cores (and other paleoclimatic archives) usually produce aggregates where smaller particles stick together to form larger ones. These aggregates may have irregular and hollow shapes. Aggregates are usually broken apart using an ultrasound bath, but no such procedure was described in this manuscript. The authors mention large particles in relationship with drill fluid contamination, but some of those may also be aggregates. If possible, it would be good to get some SEM images from these sections. If not, I think aggregates and the potential effects on the presented measurements should be at least discussed. Note that this is different from the coincidence that the authors mention in line 139, which is a bit confuse. Coincidence is when for example two small particles are counted as on large one, as the authors mention in line 141, but when small particles coagulate (for example at the boundary of ice cristals) the we are talking about aggregates which is a different problem.

This comment has been addressed by the addition of Lines 148 – 151 and 223 – 226. While some of the deeper core (samples from Heinrich Stadial 4 and 5) do show evidence of possible aggregation, we check the impact on our Abakus and CC samples. We detail our methods of checking by adding the following (Line 149 – 152), "While we are aware of particle aggregation in deep sections of ice cores, we also access the impact of aggregation on PSD values. Baccolo et al. (2018) identified particle aggregates in the deep sections of the Talos Dome core PSDs which were characterized by low fine particle concentration and a coarse (~4.4 µm) mode. We look for similar features in the deep South Pole Ice Core."

In Lines 224 – 227, we also add the results of our particle aggregate test, "Furthermore, we find that the deeper core samples (Heinrich Stadial 4 and 5) PSD mode values compared to Heinrich Stadial 1 and LGM mode values are statistically similar (student t-test; p-value > 0.05). Therefore, while there may be particle aggregation, it is not having a significant effect on the deep South Pole samples."

Major Comment 2: There is a bit of confusion on the binning and size distribution in the manuscript. In many places, the authors mention the bins of the Abakus, but in most places they seem to work with a coarse vs. fine fraction only. I think the manuscript could be made clearer about when the whole size distribution is used, and when only the coase vs. fine fraction. Related to this, the authors use the size fraction between 2 um and 6.4 um for their correction. However, it is not addressed in the text how these bins are to be found in the Abakus since there is no good way to attribute a specific voltage to a specific particle size, as the calibration with spherical latex particles does not work well. This should be clarified in the text, in particular the calibration of the Abakus particle sizes.

Major Comment 2 Response: This comment has been addressed by the addition of Lines 366.

We add the following to describe the calibration of the Abakus in Lines 138 – 140, "The Abakus was calibrated using CC techniques and then successfully tested against latex spheres of 1, 2, 5, and 10 µm diameter to assess accuracy (Koffman et al., 2014). Therefore, our size range of 1.1 – 6.4 µm have been successfully used and are considered accurate."

For clarity of when we are referring to coarse vs fine particles or PSD_{Abakus} and PSD_{CC} we add the statement (Line 371 – 374), "Because of these differences in distribution and the non-linear offset between PSD_{Abakus} and PSD_{CC}, we explore the differences between fine and coarse particles. For clarity, we refer to PSD_{Abakus} and PSD_{CC} when referring to the entire distribution of particles by size and fine and coarse particles when referencing generally trends based on distribution differences in aspect ratios."

Major Comment 3: Chapter 3.1: This subchapter is unclear. It is not clear if this comparison in Fig S4 was made using the corrected Abakus PSD or the original. Also, Fig 3 appears to show different Abakus PSDs based on corrections, but these are not discussed at all in the text. One possibility would be to merge this chapter with chapter 3.3, for example.

We add to the caption of Figure S4 detailing that the figure was calculated using spherical assumptions. We also add statements discussing changes relative the effect of shape in Figure 3. In Section 3.1 we add to the sentence in Line 320 - 322, "Furthermore, the distribution differences between PSD_{Abakus} (under spherical shape assumption) and PSD_{CC} are not temporally consistent (Figure 3 and S5), potentially indicating changes in particle shape over time.", by specifically stating the shape assumption and Figure 3.

We also add references to Figure 3, specifically stating under spherical assumption when discussing total offset in Lines 314. In Section 3.3, we add references to Figure 3 (Line 389), which are specifically discussing the effect of particle shape and reduction of our total offset parameter.

Minor Comments

Line 40: The parenthesis with ~490 years per sample during the LGM is out of place. Put this information somewhere else.

Line 40: Added "temporal resolution…" before ~490 years per sample during the LGM. We respectfully keep the placement of the 490 years to convey the average resolution of our samples.

Lines 40-41: These are not period of rapid global climate reorganization (that would be the glacial termination), they just include some millennial scale variability, just as pretty much any other period except the Holocene.

Line 40 – 41: Changed text from "…rapid global climate reorganization" …to "millennial scale climate variability".

Line 42: number of samples is missing for HS4 (n=?)

Line 42: Heinrich Stadial 4 and Heinrich Stadial 5 were grouped together in this analysis, where the n = 16 value represents the number of samples in both. The line has been changed so that now the number of samples in both Heinrich Stadial 4 and Heinrich Stadial 5 represents the number samples in each of those time periods for clarity.

Line 70: I would say the Abakus provides theoretical millimeter-scale resolution, as the mixing of water in the tubes dilutes the original signal. Rasmussen et al. tried to deconvolute that back in the days (https://doi.org/10.1029/2004JD005717), but it was not really widely applicable.

Line 70: Added that the Abakus provides theoretical millimeter-scale resolution (Line 71).

Line 88: Why is the period 0-10 ka not included?

Line 88: Due to sampling aliquots during melting, we had limited sample volume between 10 – 0 ka, which was used from trace element geochemistry analyses.

Line 92: What's the accumulation rate during the glacial?

Line 92 (Line 92 - 93): Added LGM accumulation rate (3.7; 27 – 18 ka) from Kahle et al. (2020).

Figure 1: Maybe add EDC or Siple Dome CO2 to complete the record in panel B?

Line 106: We add the WAIS Divide CO_2 record to Figure 1 since the South Pole Ice Core is stratigraphically tied to WAIS Divide (Epifanio et al., 2020; Winski et al., 2019).

Line 175: Here there is some information missing. The CC is usually calibrated using commercially available spherical latex particles. How were the Abakus size bins calibrated and can they be directly compared to CC bins?

Line 175 (Line 138 - 140): Added sentence describing Abakus calibration technique described in Koffman et al. (2014). "The Abakus was calibrated using CC techniques and then successfully tested against latex spheres of 1, 2, 5, and 10 µm diameter to assess accuracy (Koffman et al., 2014). Therefore, our size range of 1.1 – 6.4 µm have been successfully used and are considered accurate."

Line 188-187: Similar comment here: The Abakus size ranges are notoriously difficult to calibrate. Are the FlowCAM aspect ratios really comparable to Abakus size ranges?

Line 187 – 188: Per the FlowCAM user manual (Fluid Imaging Technologies, 2011; pg. 136); "The calibration constants are determined during the manufacturing of the instrument and should never be changed. Fluid Imaging Technologies, Inc. uses a proprietary optical calibration device to determine the calibration factor of each objective of the instrument. Added sentence in Line (now 195 - 196) stating "We use the default factory settings, which have a predetermined calibration factor, which converts pixels to micron measurements (Fluid Imaging Technologies, 2011)."

Line 193: Figure S2 does not show particle counts by Abakus bin size.

Line 193 (Line 202): Changed from "particle counts by Abakus bin size" to "particle counts by coarse particles (5.1 – 6.4 µm and total counts and Figure S3 for representative FlowCAM particle images from a single sample)."

Line 210: Again, how were the Abakus size distributions calibrated? Could the offset come from an uncertain coarse:fine particle size threshold?

Line 210: We refer to corrections of Line 175 (Line 138 – 140) and Line 187 – 188.

Line 228: This is the crux of the problem. How can you be sure that length measurements of particles are equal between Abakus and DPI?

Line 228: We refer to corrections of Line 175 (Line 138 – 140) and Line 187 – 188.

Line 251: Again, how do you know which Abakus channels corresponds to the exact sizes 2 and 6.4 um?

Line 251: We refer to corrections of Line 175 (Line 138 – 140) and Line 187 – 188.

Lines 255-263: The mention of Eq.3 in the text could be a bit confusing since only eq. 3a and 3b are listed. Rephrase for clarity.

Line 255 – 263 (285 - 295): Clarified in-text references to Equation 3 by adding a and b to respective statements.

Line 286: In Figure S4 it says the slope is 1.81, not 2.3.

Line 286 (Line 314): Corrected slope value from 1.81 to 2.32.

Line 287-288: Could we please see the histograms in the SI?

Line 287 – 288 (Line 317): Added figure to supporting information showing distributions of PSD_{Abakus} and PSD_{CC} (Figure S6).

Line 295: Table 1 is first mentioned here, but appears only much further down in the document. I suggest moving it closer to the first mention.

Line 295 (Line 339): Moved Table 1 from Line 394 to Line 339, so that it corresponds better to Section 3.1.

Figure 4: The title says Particle aspect ratio as a function of time, but this is only true for panels b and d. Also, the y-axis line and title colors for Particle Concentrations and CO2 appear to correspond to the wrong curve in the plot. The last two sentences of the figure caption are redundant.

Figure 4 (Line 356): We add to the opening sentence of Figure 4 caption that "Particle aspect ratio data as a function of aspect ratio (a and c) and time (b and d)." We also re-color the y-axis labels for clarity, and we remove the second to last sentence because of redundancy.

Line 337: If you interpolated width values in bins 2.1, 2.2, 2.4, and 2.7, then why do these widths have multiple measurements and standard deviations in Figure S8?

Line 337: Our initial attempt was to show the similarities between each of the three time periods under investigation. For clarity, we only produce figure now without subplots. The standard deviations represent the spread in the data from each of the time periods since multiple samples were taken during each period under investigation. We believe that the reviewer meant to say Figure S7 and have replaced the original figure with only one figure with 2σ error bars. If the reviewer was referring to Figure S8, there are no width measurements for bins 2.1, 2.2, 2.4, and 2.7µm but added the following sentences for clarity as to what error bars represent (Figure S8), "Error bars represent the variability of average width measurements throughout the record by particle size."

Line 346: Closing parenthesis missing after 1c

Line 346 (Line 387): Closed parenthesis after 1c.

Line 443: This last phrase should be rephrased as particle numbers were not published in Ruth et al., 2008. Although the correlation between the two was very high, that does not imply the same absolute values.

Line 443 (Line 504): Replaced the word value with relationships.

References

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