

Clim. Past Discuss., referee comment RC1
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Comment on cp-2022-36

Anonymous Referee #1

Referee comment on "Non-spherical microparticle shape in Antarctica during the last glacial period affects dust volume-related metrics" by Aaron Chesler et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2022-36-RC1>, 2022

GENERAL COMMENTS

I carefully examined the manuscript entitled "Non-spherical microparticle shape in Antarctica during the last glacial period affects dust volume-related metrics" by Chesler et al.

I find the manuscript very well structured and written, addressing an issue of utmost importance in the field of dust characterization and bringing to results of importance for the community.

The manuscript deals with the effects of microparticle geometry on the calculation of dust metrics, such as mass and particle size distributions. Coulter counter and Abakus methods are used on the same samples and independent information about shapes is obtained by a commercial particle imaging method.

The main goal of the work is to improve the Abakus measurements by using imaging and Coulter counter information.

Different methods for calculating the particle size distributions are compared, with the spherical assumption always bringing to overestimate the particle volume. Finally, the authors correctly conclude that shape can also be used as an additional piece of information

about changes of dust over time. Sect. 4.5 gives an example of this analysis. A detailed analysis is provided for a number of samples from Antarctica, providing the most extended and time resolved data set of particle shapes so far in Antarctica. Three paleoclimate periods are considered and the results from 41 samples are reported.

The overall manuscript gives an important contribution to scientific progress in the field. Scientific approach is rigorous and valid, the results properly discussed and references properly cited.

I consider the manuscript warrant of publication with minor revisions.

SPECIFIC COMMENTS

I find only three weak points in the manuscript to be considered by the authors before submitting the final version.

1) About the procedure through which the results are obtained. As suggested by the authors in the conclusion, shape should be considered for dust analysis and implications. Therefore, a reader could better exploit the results of this work if the calibration procedure will be more explicitly presented in the text.

I find the explanation about "calculations" and "calibrations" and the description in sect. 3.3 maybe too short for a reader who wants to exploit the method.

2) I find the discussion about the 2D images and the impossibility to differentiate between prolate and oblate to be correct but too short to be appreciated by a non-expert in the field.

More precisely:

Line 187: please introduce here the limitation imposed by the 2D measurement

Line 225: please discuss briefly this limitation in the text. As it is now I find the key point to be much more clear in the figure caption.

I think better to explicitly name "oblate" and "prolate" the shapes in the text.

Figure 2: I suggest not to use the same image as an example of particles (a) and (b). It generates confusion. I understand the author's idea, that I find very fair, but I think it could be better explained instead of showing the same image.

A brief discussion of this issue could also be inserted in Sect 4.4 (line 467).

On top of that, I respectfully suggest the authors to check for numbers a and b in Fig.2. The aspect ratio of the imaged particle is apparently much larger than 2. Could it be due to a bias in the method to obtain the Feret size from the images?

3) I think something is wrong, or wrongly explained, about the measurement of "width" and the interpretation (lines 335-343).

Apparently, the authors are able to characterize particles better than the ellipsoidal description, but this is not really clear. Also the difference between width and height is not clear, because their definition should depend on orientation.

Moreover, the conclusion that "width measurements are not statistically different between each of the three time periods" at variance with the shapes, appears very peculiar. Also peculiar is that "changes in the aspect ratio are primarily driven by variations in particle length".

What I respectfully suspect is a kind of bias or oversimplification in this part of the analysis.

If "widths" would really be independent of time a specific careful interpretation should be given, or at least attempted on the basis of independent information. It would be an outstanding discovery indeed, motivating to re-consider all the distributions with the width as the key parameter to characterize dust.

I list here below some minor points I suggest the authors to consider before submitting the new version.

- line 68: Abakus does not measure backscattered light

- line 72: "it only provides particle size measurements in one dimension". Please clarify this sentence. I can guess the meaning, but I find it quite misleading: apparently the idea is that it measures one parameter, that is true (but it is also true for Coulter counter and other instruments) and this is not the concept the authors wants to discuss here. "extinction length" is a property of a cloud of many particles impinged by light: please change.

- line 82: "absorption intensity" is meaningless, please change. "light scatetring and absorption is proptional to the extinction cross section". More precisely, the extinction is the sum of scattering and absorption. please change.

- line 138-146: Why not comparing to a pure statistical estimate, based on the probability to have two particles in the Abakus scattering volume (if known)?

- line 170: Is Figure 1 actually S1?

- line 182 and below: "Feret" with capital letter

- line 203: "The flowCAM does not produce size estimates for size bins": please explain.

- line 205: Figure S8 and S9 are mentioned before S7 (S6 is not mentioned)

- line 210-211: If possible, this hypothesis should be better explained: it appears to be quite strong and requiring better support, especially in connection with the discussion about coincidences (line 138-146).

- line 228: "binned by particle length under the assumption that length measurements of particles are equal between the Abakus and DPI". I would suggest the authors to slightly extend the discussion about this point: this assumption could generate a further bias, since Abakus is assumed here to give a well defined "length". Actually, it provides a much less defined "size" from the extinction cross section, that could be affected by shape and other particle features.

- section 4.1, line 379: I expect that the drilling fluid form a kind of water emulsion. Do the authors have images obtain with these samples that can support this interpretation? Anyway I agree to remove the corresponding data from the analysis.

- line 391: Figure 6 is missing: is it actually Figure S6. Notice that Figure S6 is not mentioned in the text.

Finally, no mention is there of the unavoidable differences in the particle statistical orientation between DPI and Abakus. Works considering the bias in determining aspect ratios of non-spherical (mineral) particles observed at microscope could be a reference for a brief discussion of this point and its consequences.

As reported around line 420, clays typically dominate with an expected prevalence of oblate shapes. Is it possible to consider this piece of information to interpret data?

In general, I suggest the authors to shift the equations before the corresponding discussion in the text.

Supplementary material:

Figures should be presented in the order of appearance in the text. S8 and S9 appear before S7, while S6 is not mentioned in the text.

About Figures S6 and S7, the meaning is not clear. What are the distributions and the "kernel density estimate"? A brief description could be beneficial for the readership.