Dear Referee #2

We thank the referee for the fast response and thorough remarks on the manuscript. It has greatly improved after we have adapted it according to the recommendations. Below follows a detailed response to the comments.

Interactive comment on "Particle shape accounts for instrumental discrepancy in ice core dust size distributions" *by* Marius Folden Simonsen et al.

Anonymous Referee #2

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In this manuscript Simonsen and colleagues tackle the long-standing problem that the Klotz Abakus particle counting device yields different results from the established Coul- ter Counter method for ice-core dust. They argue that because of the asymmetric shape of natural dust particles, the Abakus sensor has to be calibrated using the ex- tinction diameter and not the geometric diameter of particles. Since the CC measures the true particle volume but the Abakus a two-dimensional cross section, they combine the two measurements on ice core data from Greenland to infer the average aspect ratio of dust particles during Holocene and LGM sections of the record.

The method described in the manuscript is innovative and a logical continuation of the studies previously published by the author groups. I am not quite happy with the ice core data application in its current form, though. The authors seem to mix and match parameters taken from various Antarctic and Greenland ice core dust publications. In addition, several assumptions are not well justified. This ultimately creates a result that may be very dependent on the specific parameters used. I therefore recommend major revisions before I can support the publication of this manuscript.

Major Comments:

In Chapter 3.1 you define a PDF that attributes a probability range of the extinction diameter as a function of the Volumetric diameter. How does this PDF come into play in the rest of the manuscript? Is it needed for the calibration? If not, it doesn't seem to be of use after that and maybe this chapter may not be necessary?

The concept of a PDF representing the probability of an extinction diameter given by a volumetric diameter is described in more detail in Section 3.4. There it is used in the simplified 2D model, but it could in principle also be constructed for 3D particles, if their shape and orientation probability were known. A reference to Section 3.4 has been added to the text.

In general, the method is a bit confused. Think of other groups that own an Abakus and want to calibrate their instrument using your method. Provide them with a clear set of instructions on how to do this.

We have added a section 3.5, which gives a calibration instruction for the Abakus. We have also merged figure 5 and 7 into a new figure 5, which hopefully clarifies both figures. This includes a plot of a calibrated versus an uncalibrated Abakus volume size distribution compared to the corresponding Coulter Counter volume size distribution.

I have the feeling that Chapter 3.3 is too short. There is very little text to explain a lot of material and as a consequence it is very difficult to understand. I think this section should be greatly expanded.

We have now expanded it.

But more concerning is the authors' claim that ice core dust refractive indexes vary between 1.52 and 1.55, citing Royer et al., 1983. These are not two limiting values, they are just two values found for Holocene and LGM ice. Moreover, they were calculated for Antarctic dust at 546 nm wavelength. This manuscript deals with Greenland dust and the Abakus laser has a wavelength of 680 nm. If the simulations are not too computationally intensive one could make a Monte-Carlo run with a whole range of values. Else, refractive indexes measured from RECAP particles should be used.

In atmospheric dust studies such as Otto 2007, Highwood 2014 and Shettle 1979, they use a refractive index of dust at 670 nm of 1.53, but give no reference to where it is measured.

Sokolik, Andronova and Johnson 1993 measures 1.53-1.57 with a mean of 1.54 for atmospheric dust samples from Tadzhikistan, largely independent of wavelength within the visible range. They mention that Wahlstrom 1974 measures 1.55 at 633 nm. Grams 1974 measures a mean refractive index of 1.525 at 488 nm, but writes that there is some variance among the particles. Carlson and Benjamin 1980 find a refractive index of 1.54 for Saharan aerosols. Patterson and Gillette 1977 find a refractive index of around 1.547 at 670 nm of Saharan aerosols.

Some of these references have been added in the text. As they support using 1.52 and 1.55, we have not run new simulations.

The authors claim in Page 10, line 2 that the Abakus counts 10 times more particles than the CC.

The Abakus counts 10 times more in some size bins because the size bins are misaligned:

There are many more counts in the small than in the large bins. When the Abakus measures larger sizes than the Coulter Counter, all the small particle counts will be binned in a large particle bin, where there are only few Coulter Counter counts. Comparing the Abakus and Coulter Counter will show many more counts in the Abakus, simply because the bins are shifted.

That goes against the findings in Ruth et al., 2008: "Good correspondence (Rlog = 1.00 and clog = 0.92) is found also between the respective number concen- trations"

In their plot 1a, Ruth et al. show that CC and Abakus measure the same concentration. However, they have already shifted the Abakus bins empirically to make the size distributions fit the Coulter Counter distributions ("using CC data for the size calibration of the LPD"), as described in Ruth et al. 2003. This means that they cannot extract any information about absolute concentration from the Abakus, and they consequently do not do this. Instead, they comment on the "clog". The "clog" is the proportionality constant between the logarithm of the

Abakus and CC data. Any multiplicative factor between Abakus and CC does not affect “clog”.

A constant scaling of the Abakus data would also not change the correlation, “Rlog”. “good correspondence” therefore only means that the Abakus concentration is proportional to the Coulter Counter concentration, but not that they have the same value. They further write “the LPD is a reliable method to quantify variations of insoluble particle concentrations in ice cores”, ie. it is good for variations, but they do not state whether it is good for absolute concentrations or not.

We have added the following line on this in the beginning of the Discussion section:

“This result is consistent with the findings of Ruth et al. (2008, Figure 1a) who demonstrated that the data produced by the two instruments are proportional over four orders of magnitude, even if the absolute concentration results do not agree. “

and against the findings in Fujii et al., 2003

Fujii does not use an Abakus, but a different laser particle counter. His observed coincidence effects are therefore not directly comparable to the Abakus.

and Lambert et al., 2012

Lambert et al. write: “The regression between logarithmic CC concentration and logarithmic Bern LPD data is

$\log_{10}$ (CC mass concentration) = $(0.9084 \pm 0.0309) \cdot \log_{10}$ (Bern LPD data) − (1.3276 ± 0.1076) with $r^2 = 0.85, n=519$,

where CC mass concentration is given in ng g^{-1} and the Bern LPD data in particles ml^{-1}. The regression between the logarithmic CC concentration and rescaled logarithmic CPH LPD data is

$\log_{10}$ (CC mass concentration) = $(1.633 \pm 0.089) \cdot \log_{10}$ (CPH LPD data) + (3.136 ± 0.128) with $r^2 = 0.80, n=273$”

They compare logarithms like Ruth et al.. Furthermore, they compare mass concentration with number concentration, so they cannot compare absolute concentration.

who claim that coincidence loss will result in lower counts for the Abakus than the CC due to several particles passing the laser beam at the same time. If the authors measured 10 times more particles with the Abakus than with the CC in the RECAP ice core, then they should explain why they get such opposite results from previous studies. I will assume that this is a tipo and the authors meant they measured 10 times less particles with the Abakus. This brings up another problem though. The much higher counting efficiency of the CC suggests that coincidence loss in the Abakus is the norm rather than the exception, and this will distort the size measurements in the Abakus. This aspect should be addressed in this paper as well.

We have added a supplementary section showing that coincidence effects are negligible, and referenced it in Section 2.1.

Minor Comments:

Introduction: English is sub-standard. Please revise language.

The introduction has been revised based on the comments from Referee #1.

Page 1, line 5: delete "leads"

OK.

Page 1, line 6: What new calibration routine?

Changed to:

"The irregular 5 shape means that a new calibration routine based on standard spheres is necessary for obtaining fully comparable data. This new calibration routine gives an increased accuracy on Abakus measurements, "

Page 1, line 17: These references have nothing to do with climate models.

No, We have now cited Mahowald et al. 1999 and Lambert et al. 2015.

Page 2, line 2: "...due to its sensitivity to electrical noise." That is the problem with coupling it to a CFA system? Please explain in more details.

Yes. It was found by Tobias Erhardt, but has not been described in a peer reviewed article. We have removed the sentence instead of expanding on the details.

Page 2, line 3: CFA is not a technique to prepare samples.

Changed to:

"CFA systems (Röthlisberger et al., 2000; Kaufmann et al., 2008) on the other hand are a common technique for analysing impurities in ice core samples, offering faster measurement speed and often higher resolution."

Page 2, line 32: Delete one occurrence of Bory et al., 2003

OK.

Page 3, lines 9-10: That is a big assumption. Either you show this is the case or you concentrate on the method.

Yes, the sentence is deleted.

Figure 4: What's the green shading? Uncertainty? If so, how was it defined?

The green shading is the uncertainty arising from the fit based on the uncertainty on the data points. It is used for calculating the uncertainty on the calibrated Abakus data in figure 5. We have added a line in the caption explaining this:

"The uncertainty on the fit (shading) is based on the uncertainty on the data points."

Figure 5: I don't see how the calibration improves the Abakus data if the CC is the reference. The calibrated curve seems worse than the uncalibrated to me by eye. Maybe a plot of residuals and a SSE could provide a quantitative measure of improvement?

No, it does not make the Abakus and CC data more alike. When the calibration is applied, the Abakus gives the extinction diameter. We see why this is confusing. We have merged figure 5 and 7 into a new figure 5, which hopefully clarifies this.

Page 6, line 8: This reference does not support the assumption that the samples are dominated by oblates.

No, it describes the measurement procedure. We have moved it to the previous sentence.

There is one sentence about Antarctica, but I don't think results from Antarctica could be extended to Greenland, see my main comments.

I assume you refer to page 11, line 5. We agree that one cannot assume that aspect ratios measured in Antarctica should be the same as in Greenland, as the source regions and transport processes are different. However, in our comparison, we merely state that the more extreme aspect ratio of Antarctic dust agrees with our hypothesis of aspect ratio fractionation during transport. We have removed the sentence about Antarctic dust provenance.

Also, how would the method perform if the sample was not dominated by oblates? Page 8, line 1: Again I don't think excluding prolates is justified unless you show the an analysis from the RECAP ice-core. Anyway, the method to calculate aspect ratios for both types of particles has been established by Potenza et al., 2016, so why exclude the prolates? Page 11, line 7-8: You only calculate aspect ratios of oblates in this study.

We have now explained why the samples are dominated by oblates. We have further added an appendix (G) discussing prolate particles, which show that the results do not differ significantly if prolate particles are hypothetically assumed:

"By comparing to SPES measurements of oblate and prolate particles in Villa et al. (2016) and Potenza et al. (2016), it was found that the samples are dominated by oblates. Prolates have a much narrower distribution of optical thickness than oblates, since their orientation is fixed by the flow. The absence of a superimposed prolate distribution indicates that no more than than 15% prolates are compatible with the measured SPES results. The following analysis therefore only focuses on oblates. For a similar analysis of prolates, see supplement G."