Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-149-RC2, 2017

Climate of the Past Discussions

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

Anonymous Referee #2

Received and published: 27 December 2017

In this manuscript Simonsen and colleagues tackle the long-standing problem that the Klotz Abakus particle counting device yields different results from the established Coulter 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 extinction 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

Printer-friendly version

Discussion paper

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? 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.

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. 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.

The authors claim in Page 10, line 2 that the Abakus counts 10 times more particles than the CC. 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 concentrations" and against the findings in Fujii et al., 2003 and Lambert et al., 2012 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.

Minor Comments:

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

Page 1, line 5: delete “leads” Page 1, line 6: What new calibration routine? Page 1, line 17: These references have nothing to do with climate models. 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. Page 2, line 3: CFA is not a technique to prepare samples. Page 2, line 32: Delete one occurrence of Bory et al., 2003 Page 3, lines 9-10: That is a big assumption. Either you show this is the case or you concentrate on the method. Figure 4: What’s the green shading? Uncertainty? If so, how was it defined? 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? Page 6, line 8: This reference does not support the assumption that the samples are dominated by oblates. There is one sentence about Antarctica, but I don’t think results from Antarctica could be extended to Greenland, see my main comments. 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.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2017-149, 2017.

Printer-friendly version

Discussion paper