Clim. Past Discuss.,
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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.

EW Wolff (Editor)

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You now have 2 review comments on your paper. Both are supportive of the idea behind the paper, but referee 2 in particular raises some important issues that need to be addressed. As you are aware the procedure is that you must respond to all the review comments (within 4 weeks of the discussion closing), and then I am asked to provide editorial guidance on whether you should submit a revised paper. However in this case, since the reviews are in early, it may be helpful if you have time to respond to the major comment about the difference in particle number between Coulter and laser methods early, as this would give time for a genuine interactive discussion between you and the reviewer, which might resolve the problem before you prepare a new version.

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I would like to add my own comment on the issue raised by referee 2. From Figure 5 it is clear that your sttaement about Abakus giving 10 times more particles than Coulter is not a typo, and needs some clarification and explanation above what is in the paper now. It is, as the reviewer points out, much at odds with the finding for EDML (Antarctica) in Ruth et al (2008) who said (my annotations in [])"The [CC and LPD] data [for particle mass] have a very high correlation (Rlog) 1.00); and the clog of 0.96 is very close to 1. Good correspondence (Rlog) 1.00 and clog)0.92) is found also between the respective number concentrations (data not shown)."

If we compare in Fig 5 your Coulter and calibrated laser data, it seems as if the laser counts way more particles right across the size spectrum, so this is not just a question of it classing them in the wrong size range because of the particle shape. However then I am very confused by Figure 7. This seems to show a ratio (between calibrated Abakus and Coulter for the glacial) of a maximum 1.5 at about 5 microns, and below 1 at 2 microns and 8 microns. But in Figure 5 (which is apparently the same data), the ratio is clearly more than 10 at 5 microns, and more than 1 at all diameters. Please explain the apparent discrepancy between the two figures. Perhaps you have written calibrated and uncalibrated the wrong way round in the caption to Fig 5, although this cannot explain the glacial factor (>1 in Fig 5, «1 in Fig 7) at 10 microns? If the very large (factor 10) difference between the methods actually applies only to the uncalibrated Abakus, then at least this makes better sense, but leaves two issues for you to comment on:

(1) Assuming coincidence counting is not a huge issue in either method, the total number of particles should be the same in the uncalibrated Abakus, the calibrated Abakus and the Coulter. Is that the case? - the fact that one Abakus curve is ALWAYS above the Coulter curve makes me doubt that but it could just be a subtlety of how the integration works when one curve goes to lower diameter than another.

(2) You need to discuss why this problem (between uncalibrated Abakus and Coulter) apparently didn't show up at EDML (Ruth et al 2008). Does it only apply to Greenland?

I hope your comments on this might clarify at least the technical point about which curve is which and how to reconcile Figs 5 and 7, and thus allow the reviewer and me to understand the work better.

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