

Interactive comment on “The calcium-dust relationship in high-resolution data from Dome C, Antarctica” by F. Lambert et al.

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This paper is potentially very interesting. If there are major changes in the ratio of soluble calcium to insoluble dust, then it has important consequences for how we interpret some ice core records, as well as for the changing geochemical sources. However, before analysing these changes too strongly, there are some questions about the analytical methods and data treatment that the author needs to address.

The Bern LPD device counts particle numbers in different size bins; however the authors have chosen to use a single number for each measurement, which is the number of particles (above a size limit, about 1 micron) per unit volume. In order to turn this into a mass per unit volume, they have simply regressed LPD particle numbers against Coulter Counter (CC) mass (or actually the other way round, which is slightly surprising).

ing). This has several problems.

Firstly the result is rather scattered. A previous paper (Ruth et al., 2008), was dedicated to understanding the different signals that relate to mineral dust in Antarctic ice cores including Dome C. As I understand it the device they called an LDS is essentially identical to the LPD used in the current paper. They used a more sophisticated data treatment of the LPD dust, where they calculated mass from the LPD, and got a much better result when calibrating against CC mass (R^2 of 1 and a 1:1 calibration).

A second issue relates also to the Ruth et al paper. The present authors make the statement (p 1118) that the LPD has a 4 x lower counting efficiency than CC. This is completely against what Ruth et al say, so it really needs clarifying.

Most importantly (and probably covering also the last two comments), is it reasonable to expect that an LPD NUMBER vs CC mass calibration is stable across glacials and interglacials? For this to be true, the LPD mass must be linear with LPD number across different climates, which is equivalent to saying that the size distribution is identical (not just the same mode, but identical across the range). To first order this seems unlikely. But if it might not be true then the rest of the paper (where the different Ca/LPD number ratio between interglacial and glacial is the main result) becomes hard to sustain.

In summary, Lambert et al need to explain in much more detail how their work relates to that of Ruth et al (2008), and why they have chosen to use a calibration of particle number against mass, rather than mass against mass, which should be available from their dataset. If they cannot do the full mass calibration, then they need to provide much more explanation as to why they think that they can justify the approximation that mass is proportional to number. This is not trivial, because they are assuming that the size distribution stays the same across periods when they are asserting that the relative proportions of two end member sources is changing.

I have no doubt there can be changes in the Ca/dust ratio, but I am not convinced the current paper has established this quantitatively here.

