

Interactive comment on “Chemical composition of soluble and insoluble particles around the last termination preserved in the Dome C ice core, inland Antarctica” by Ikumi Oyabu et al.

Anonymous Referee #1

Received and published: 6 May 2016

In this manuscript the authors compare the chemical composition and concentrations of non-volatile aerosols from the EPICA Dome C and the Dome Fuji ice core. They use SEM and EDS analysis to estimate the concentrations of the main soluble salt particles from 6.8 kaBP to 25 kaBP. They then infer how the ratios changed during the termination to identify two different regimes of transport between the two periods.

The manuscript is generally poorly written and lacks scientific rigor, particularly in the statistical analysis of the data. The main weakness of this manuscript is that the authors have very few data points and make many statements based on one or two data points that may or may not be outliers and/or statistically significant. As an example, in chapter 3.3, page 7, lines 12-18 the authors compare their analyzed top 5 cm with

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the bag means and select one section to discuss where the top 5 cm does not agree with the bag mean and to justify a low ratio of NaCl/Na₂SO₄. However they completely disregard other LGM samples that also have low LGM NaCl/Na₂SO₄ ratios but where the top 5 cm agrees with the bag mean. As another example the authors suggest in chapter 3.4, page 8, line 25 that there is a 600 year shift between the maximal sulfatization at Dome C and DF. Such a shift is completely unsupported by the data in Figure 4c. There are many other examples. These unsupported claims, combined with statistical mistakes like giving averages and standard deviations of clearly non-Gaussian data or identifying maxima/minima to a precision of 100 years with 30 data points in a period of 20,000 years only underline the flawed analysis of the dataset. I do not doubt the profound chemical knowledge of the authors, but I cannot believe their conclusions based on such poor analysis of the data. Finally, although the salt ratios have never been measured before, there is nothing new about the conclusions of this paper. For these reasons I suggest to reject this manuscript.

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-42, 2016.

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