

## ***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 #2**

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The paper is focused on the study of the chemical composition of soluble and soluble particles in the period 6.8 – 26.3 kyr BP in Dome C ice core by using EDS analysis upon sample sublimation and on the comparison of the achieved results with Dome Fuji ice core. This paper strongly resembles in outline a previous paper dealing with Dome Fuji ice core by the same leading Author (Oyabu et al., 2014, J. Geophys. Res.) which is mentioned often in the present paper but actually adjusted to Dome C ice core both as text and as figures (for instance, figures 2, 3, 4 in this paper are just specular to figures 4a, 3, 4b of Oyabu et al, 2014). The paper is filled with very general and hardly meaningful or poorly referenced statements (see examples below) and, although SEM-EDS compositional data from EDC had never been presented before, they do not add

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significant information to the current knowledge of the chemical composition of the soluble and insoluble particulate deposited onto the East Antarctic plateau during the Last Termination. Moreover, the scientific design is not described clearly and also some methodological choices (used data sets for comparison with sublimation-EDS method, number of data acquired by the latter method, etc.), do not appear suitably supported (see detailed comments below). Finally, the drawn conclusions are whether already well known from similar and ion composition and dust measurements (f.i. when reporting the main soluble salts as  $\text{CaSO}_4$ ,  $\text{Na}_2\text{SO}_4$  and  $\text{NaCl}$  and a lower fraction of  $\text{NaCl}$  during the LGM or when inferring two different transport patterns during the Last Termination, respectively) or scarcely meaningful (invoking both a “uniform” marine environment in the sectors affecting Dome C and Dome Fuji sites or that both sites are influenced by a large part of the Southern Ocean). Hence, for all these reasons, I believe that unfortunately the paper cannot be accepted for publication on “Climate of the Past” journal.

Here below some specific remarks are reported as examples but others can be found along the text.

Abstract Page 1, lines 21-23. “However. . . .  $\text{Ca}^{2+}$ ”. Such a conclusion could be easily drawn by comparing already published sulphate, sodium and calcium records from Dome C and Dome Fuji ice cores. It certainly does not take a time-consuming and detailed EDS analysis to claim this. The fact that the EDS analysis on single particles in the end lead to already well known facts is one of the main flaws of the paper.

Introduction Page 2, lines 26-31. This is an example of a mix of uncorrect and obvious statements. The different amount of  $\text{Na}_2\text{SO}_4$  found in Talos Dome and Dome Fuji cannot tell by itself on the different yield of reaction R2 (line 22) since it depends also on the actual total amount of each components deposited at the two sites and this is likely to be discriminant for determining the final content of sodium sulphate particles. Moreover, it is way too simplistic and uncorrect justifying the claimed different sulfatization contribution of  $\text{NaCl}$  at the two sites by the supposed larger time needed to

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reach the plateau. The fact that Dome Fuji is about 1000 km inland and Talos Dome around 250 km far from the coastline does not mean that marine air masses necessarily take a shorter time in reaching the latter since it depends on transport routes (not necessarily and often not in straight line!) and on the speed of air masses, in turn depending on zonal/regional atmospheric circulation, following different patterns in the different climatic regimes. Finally, R2 is supposed to occur in the atmosphere, so what do “post-depositional effects” (line 29) have to do with it?

Methods The sample data set (n. 30 samples as a whole) is not large enough, considering that they span periods with dramatic differences in load and chemical composition, to assure a statistical significance.

This chapter shows many unclear and uncorrect statements and descriptions. Here below a few examples. Page 3, line 20. “pulverized on of the faces. . .range” is perfectly unclear. Page 3, line 31. Acceleration voltage is in kV, not keV. Page 3 line 31 – page 4, line 1. The sentence is not clear at all. Maybe it is supposed to be explained by lines 23-24 in the same page but there is no clear information on the approach used to determine the particle diameter (measured or “equivalent diameter” calculated on the basis of the section area?) and error. Paragraph 2.3. I see no point in taking two different dat sets for Ca<sup>2+</sup>, Na<sup>+</sup> and SO<sub>4</sub><sup>2-</sup> (Ion Chromatography) and Cl<sup>-</sup> and NO<sub>3</sub><sup>-</sup> (Continuous Flow Analysis) while the same parameters were also available from the Ion Chromatography measurements on the same samples.

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