

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 #3

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In this paper, Oyabu and coauthors investigate the chemical composition of nonvolatile (soluble and insoluble) particles at Dome C during the last glacial maximum, the last termination and Holocene (26.3ka to 6.8ka B.P.). The authors are undoubtedly among the maximum experts in the field. They present for the first time SEM-EDS compositional data from Dome C on a set of 30 samples. Because this type of measurements is extremely time-consuming, I think 30 samples is a reasonable number for a publication. The methodology followed in this work is the same as in Oyabu et al., 2014 (DOI: 10.1002/2014JD022030), thus data from the two ice cores drilled in different sectors of East Antarctica are fully comparable. This is an advantage.

While this kind of data is potentially very interesting, I found the discussion part rather

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weak: some statements are not suitably supported, and the paper in general does not add much to what is already known from Dome Fuji.

Therefore, I encourage the authors to re-structure the discussion part of this work including some major revisions.

Some detailed points:

Pg4, lines 20-25: the method used to determine particle size (although size is not discussed further in the ms) is not clear at all. If you regard each particle as a sphere of revolution around the major axis, you do not consider the dramatic difference between aspect ratios from top orientation and side orientation that is typical of clay minerals that we expect to find at DC. However, if I understand well, you overcome this problem by measuring the shadow area on a subset of particles selected at random, is this correct? Please explain your method in detail and add a reference.

If particle size determination with this method has been carried out on Dome C particles in the framework of this work, why you do not discuss in the paper the relation between particle size and composition, in order to highlight if there are some soluble salts that are clearly smaller/larger than others and/or give a size range to these soluble aerosols?

Pg6, lines 7-8: to support the statement “the ratio of insoluble dust to non-volatile aerosols appears to change with dust concentration” can you compare this ratio to dust concentration data from independent measurements (Lambert et al., 2008)?

Pg6, lines 31-32: I hardly agree that the pattern of changes during the last termination is similar between DC and DF. Actually, looking at fig. 4b, for example, it seems that only for major glacial-interglacial changes the two sites are in agreement, while (despite the few data) some differences arise during ACR and the minimum preceding the Holocene. It is surprising to see at Dome C a higher $\text{Na}_2\text{SO}_4/\text{CaSO}_4$ ratio during ACR and a lower ratio during the dust minimum. The pattern of Dome Fuji in this sense

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seems much closer to what is expected. Can you comment on this difference?

Pg7, line 30: here you suggest a post-depositional process for Na_2SO_4 formation. I admit I am lost because the (pre-depositional and/or post-depositional) processes involved in the formation of Ca and Na salts are not clear at all. CaCO_3 is believed to react with SO_4 in the atmosphere, whereas sulfatisation of NaCl is a post-depositional process? Or both?

Pg.8 lines 25-26: I think it is a risk with such few data to state that most NaCl sulfatized earlier at Dome C than at Dome F.

I am confident that a deep re-writing of this paper will lead to substantial improvements.

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