

Interactive comment on “Frost flowers and sea-salt aerosols over seasonal sea-ice areas in north-western Greenland during winter–spring” by Keiichiro Hara et al.

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This paper presents a wealth of information on composition of frost flowers and aerosol from an Arctic field campaign, with a detailed discussion of results. It is very well written except for the trivia listed under Editorial Comments below.

Major Comments:

A. Except for a couple of lines buried on p13, the authors ignore the discovery in 2008 and 2010, in field measurements and separately in laboratory measurements, that frost flowers can be of non-fragile structure not dispersed by wind, and in the laboratory not producing aerosol (Obbard et al. 2009, Roscoe et al. 2011). This discovery was

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consistent with the then new theory that wind-blown snow played a major role in the mobilisation of Br to the atmosphere in the sea ice zone (Yang et al. 2010). Although many previous field measurements had been consistent with the scenario presented by the authors in their Introduction, others had not - instead they had been consistent with the discovery in 2008 and later. Before publication, the Introduction must be revised to discuss the ambiguous nature of frost flowers and their mobilisation; Yang et al. (2010) must be included in the References; and the discussion on p13 lines12-16 must be enlarged.

B. There is no major scientific conclusion in the paper - it is an extensive and comprehensive report of the measurements and what they might mean in detail, but not what they might mean to Atmospheric Chemistry and Physics in general. Without resorting to a new set of work with a model, I doubt that there could be such a conclusion. It is up to the Editor whether or not this is a bar to publication.

Minor Comments:

1. Despite the text on p3 and elsewhere throughout the manuscript, we discover from Figure 1 that Site III is actually split into two. It would help if some reason for this was given, and if later text and captions said whether the result being described was from Site IIIa or IIIb .

2. The authors describe the good practice of storing brine samples in Japan frozen until analysis (p5 line11), but it seems that they were transferred to Japan unfrozen - presumably because of the difficulty of carrying frozen samples by air. How long were the samples left unfrozen - 4 days, or several weeks?

3. It is also good practice to keep aerosol samples collected in polar regions frozen for shipment and storage until analysis. This is not mentioned on p4 lines27-29, so we assumed they were not frozen, in which case more should be made of the potential for change during storage than the one line given here, and the unfrozen time between collection and analysis should be stated.

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4. The description on p6 of Figures 3(a) to (e) bears little relation to the order of (a) to (e) in the Figure itself. Nor does the caption of Figure 3, which does not even mention Figure 3(e). This should be sorted out.
5. From the scenario of frost flower formation given in the introduction, we would expect sulphate depletion in surface brine to be equal to that in the frost flowers - the flowers are expected to wick this brine, so if sulphate deposition occurs only in the colder temperatures above the surface then the frost flower total sample would contain the original brine sulphate. The fact of depletion in frost flowers but not brine in Figure 6 is a strange and new result, which deserves comment in p9 para1. Might it be related to the sampling protocol for surface brine given in p4 lines5-6? This suggests a large amount of solid was removed together with any liquid, thereby possibly incorporating ice into which sulphate had precipitated. This should be discussed, as it may also affect some of the discussion about changes in Mg, Br and Ca after 26 Feb.
6. The discussion about possible removal of Br and I from frost flowers by heterogeneous reactions on 27 Feb (p10 lines21-21) stretch the apparent accuracy of the measurements - the difference between 0.00206 and 0.00214 for Br is surely not significant, given the scatter in Figure 4b. If the Br and I differences are significant, error bars should be derived.
7. The statement on p17 line23 that sea salts (Mg etc) were “remarkably enriched” is not borne out by Figure 6. Depending on the error bars, they may not be enriched at all. If “remarkably” is removed, the sentence can probably stand, but better would be a derivation of error bars.
8. The caption to Figure 1 must contain some details of the broken lines described on p3. It is not acceptable to have figures with important features that are not described in the caption.

Editorial Comments:

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- p1 line12 - replace “sea salts” by “sea-salt constituents”.
- p3 line23 - either “Sea ice forms gradually from October” or “Sea ice formed gradually from October 2013”.
- p3 line25 - delete the second “the fjord”.
- p5 line1 - insert “a” or “the” before “data logger”; replace “in” by “with a”.
- p5 line3 - replace “slash” by “slush”.
- p8 line22 - replace “Site I” by “Site”.
- Fig1 - must be rotated and “Siorapaluk” made legible.
- Fig5 - shows values at Sites A, B and C which should presumably be Sites I, II and III.
- Fig5 caption - define the red line in Fig5(b).
- Fig9 caption - does not say which of the two lower figures is coarse and which is fine.
- Fig S2 caption - does not agree with the axis legends of the figures.
- Figs S3, S4, S5 - must be rotated.
- Fig S5 caption - only describes one of the two images and spectra shown in the figure.

Additional Reference:

Yang, X., J.A. Pyle, R.A. Cox, N. Theys, M. Van Roozendaal, “Snow-sourced bromine and its implications for polar tropospheric ozone”, *Atmos. Chem. Phys.*, 10, 7763–7773, doi:10.5194/acp-10-7763-2010 (2010).

Interactive comment on *Atmos. Chem. Phys. Discuss.*, doi:10.5194/acp-2016-1094, 2016.

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