

Interactive comment on “Frost flower chemical signature in winter snow on Vestfonna ice cap (Nordaustlandet, Svalbard)” by E. Beaudon and J. Moore

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We are grateful to the referee for his suggestions that will help us to improve our manuscript. Below we try to address each point.

REF: Section 2.1 notes that Pit 1 was sampled at 5 cm intervals while Pit 2 was sampled at 20 cm intervals. The Pit 2 chemistry record in Figure 3, however, appears to exhibit variability at a < 20 cm scale at times (i.e. the 5 cm wide "bump" in NO₃ concentrations at 100 cm depth). Was there sub-sampling at higher resolution at points along the Pit 2 profile?

ANSWER: We will be more precise in our revised manuscript that the layers containing thin ice layers have been sub-sampled with 5 cm resolution.

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REF: If the frost flower layer is only 20 cm thick in Pit 1, is it possible that this signal was homogenized in Pit 2 due to the combination of a coarser sampling interval and lower ion concentrations?

ANSWER: A thin FF signal homogenized due to low resolution sampling is possible, though, as we can show it is unlikely. Even if the concentrations are lower in Pit 2 than in Pit 1, a 10 cm FF layer (up to 6 times more concentrated than the rest of the snow pack) would still appear 3 times (roughly) more concentrated than the layers above and below if the sampling resolution is 20 cm. In Pit 2, there is no sample systematically 3 times more concentrated than the samples adjoined. If a concentrated layer is less than 5 cm thick in Pit 2, I would not interpret it as a frost flower layer (for example fog deposit can amount to 3 percent of the total accumulation)

REF: Section 2.2 says that ten major water-soluble ions were measured, but it only lists four anions and five cations. Figure 3 only shows eight unique records.

ANSWER: Sorry, MSA record is missing in the figure because it does not show any peak at 1m in Pit 1. We will include the MSA record in our revised paper.

REF: Section 2.2 would benefit with an identification of the error associated with the Dionex concentrations. Presumably the uncertainty in concentration is small in comparison to the difference in concentration between frost flower and non-frost flower layers, but this is not stated explicitly.

ANSWER: Indeed the error percentage for the concentrations is low (for instance mean blank values: 0.04 ± 0.02 microEq.L for Cl, 0.05 ± 0.01 microEq.L for Na, 0.16 ± 0.13 microEq.L for SO₄) compared to the difference between the frost-flower layer and the non-frost flower layers. To facilitate that comparison we will remake the table 2 and include the average ion concentrations for the FF layer.

REF: Section 3 leads me to believe that the top of the 2006 summer layer is interpreted to be at 160 cm depth in both pits (p. 164: 17). Subsequently, both pits are

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calculated to have the same accumulation rate. This interpretation places the top of the 2006 summer layer above the strong $[\text{Na}^+]/[\text{Mg}^{2+}]$ melt layer in Pit 1 but below the $[\text{Na}^+]/[\text{Mg}^{2+}]$ melt layer in Pit 2 (Figure 2). This would suggest that Pit 2 experienced an extremely strong fall melt event that Pit 1 did not experience. This inconsistency is resolved if the top of the 2006 summer layer is taken as 100 cm in Pit 2. Naturally, this would mean that the accumulation rate at Pit 2 is significantly lower than Pit 1, which would have significant implications for the parallel comparison in Figure 3. If the maritime effect is truly large at Vestfonna, might the spatial variation in snow chemistry be mirrored by similar variation in snow accumulation?

ANSWER: We based our estimation of the summer surface depth on the stratigraphy description. We assume the top of the firn layer to be the summer surface, this is what we find at 160 cm not 100-110 cm depth. A hard layer of coarser grains and low density is the typical structure of a summer surface (recognized and described by Schytt, 1958). Snow at 110 cm deep has experienced melting which can be due to a very local event such as autumn rain (Schytt, 1968).

REF: In addition to identifying the depth of the frost flower layer, is it possible to associate an approximate month with this depth (Section 3, p. 165:1)? "Winter" is rather general. Are the data sufficient to suggest frost flowers are significant ions sources in the month of February for example?

ANSWER: We will explicitly define winter as the months January, February and March. Since we dug the pits in April and taking the accumulation rate into account, the layer located half way in the snow pack (the frost flower layer) corresponds to "winter". Unfortunately we do not have temperature data for Rippfjorden in winter 2006-2007 and we also miss meteorological data for Vestfonna before spring 2007. Thus, we can not attempt to find the best conjunction of favorable parameters (temperature below -8 and SSW to SE winds) for frost flower formation. That is why it is quite difficult to precise which month the frost flower have been deposited.

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REF: Section 4 states that: "mirabilite is the only salt to precipitate out when sea ice is formed between -8.2_C and -22_C" (p. 166: 18). The molecular formula for mirabilite is given as: $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$. I interpret this to suggest that frost flowers would only expose Na and SO_4 on the surface of newly formed sea ice. How does this explain the elevated K^+ , Mg^{2+} , Ca^{2+} and Cl^- concentrations in Figure 3? Perhaps I am missing something, but where are these ions coming from if mirabilite does not contain them? As presented, the mirabilite hypothesis does not acknowledge that frost flowers may be important sources for a wide range of species (i.e. observations confirm they can be important sources of halogens, which mirabilite does not contain either: Rankin et al., 2002; Kaleschke et al., 2004).

ANSWER: Perhaps the referee has misinterpreted the mechanism we explain for frost flowers on p 162, 5, so we paraphrase it here: mirabilite removes SO_4 (and Na) from the brine, so the remaining components are enriched in concentration. The brine is sucked by capillary action into the frost flowers which are then blown onto the ice cap. The mirabilite salts are left in the sea ice and so are relatively depleted in the ice cap deposits.

REF: Snow chemistry variability is the result of: (i) emissions variability, (ii) transport variability, (iii) deposition variability, and (iv) storage variability. This paper assumes that the snow pit records reflect variability in emissions. This paper only implicitly rejects transport variability and does not address the remaining two sources of variability at all (deposition mechanism and storage). Is it at all possible that if Na^+ deposition occurs primarily via dry deposition and SO_4 deposition occurs primarily through wet deposition, a seasonal change in snow fall rate could affect the ratio of these two ions? (i.e. taking these deposition mechanisms into account, a decrease in winter snow fall would increase the relative Na^+ concentration and decrease the relative SO_4 concentration in the snow pits. Would this affect the nss-component calculation?) transport and depositional variability are invoked to explain differences between pit 1 and 2. The issue of storage variability of post-depositional change is a complex issue, but which

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can occur much more significantly when melting occurs (e.g. Pohjola et al., 2005; Moore et al., 2005) the winter snow pack is studied here because these difficulties are minimized and hence the interpretation of signals is much safer.

ANSWER: There is no evidence we are aware of that Na comes from dry deposition in a maritime, high accumulation site such as Vestfonna. While Na may be dry deposited in remote, inland, low accumulation and high elevation sites in Antarctica, these sites are utterly different from Svalbard.

If we consider a different deposition mechanism for Na and SO₄ and a seasonal change in snow fall rate, the ratio of these 2 ions would be affected for the whole winter snow pack and this is not the case in Pit 1 (the ratio varies within the winter snow pack). And why would be SO₄ the only ion to precipitate through wet deposition? If Na and SO₄ would have radically different deposition mode (as suggested by the referee), it would indeed affect the nss-component calculation but how would we explain the difference between nss- SO₄ (negative peak) and nss-Ca (positive peak) then? (In addition, the positive nss-Ca peak calculated with Na is also caused by Na depletion due to mirabilite precipitation).

REF: Section 4.2: The discrete coupling between the atmosphere and snowpack is not discussed. Is it more likely that the frost flower layer was deposited instantaneously in a single storm event, or by many subsequent events over a long period? The temperature record in Figure 4 could be augmented with additional timeseries to better constrain exactly when during the year, and how long, the period amenable to frost flower formation exists. An annual sea ice concentration timeseries, not necessarily from Hinlopen Straight, but perhaps extracted from a nearby grid cell in a gridded dataset set, could be used so establish the approximate period of regional sea ice formation in the fall/winter of 2006. An accompanying wind direction timeseries would then indicate if there were indeed winds from the Hinlopen Straight during this time period. Do the speculated temperature, wind direction and sea ice formation optima really line up with the timing of the observed frost flower layer?

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ANSWER: This is an interesting idea, but strongly constrained in practice by a sub-seasonal resolution of the snow pit record. As explained earlier we cannot specify to better than a few month interval the timing of the FF layer. We feel that further work on this would require a program of air-snow sampling.

REF: The introduction suggests that part of the motivation for understanding frost flowers is to elucidate their potential impact on ice core records. The influence of frost flowers on ice core chemistry records, however, is not explicitly addressed anywhere in the paper. Perhaps the authors could speculate how an ice core SO₄ or marine ion record under the influence of frost flowers would be expected to differ from similar ice core records which are not under their influence?

ANSWER: We will address some remarks on this in relation to the comments of Mauri Peltó: MP: A point that I would suggest elaborating upon: In the conclusion it is noted that frost flowers would form preferentially in areas lacking multi-year ice. Is it then likely that the appearance of a frost flower chemical signature layer is indicative of nearby open water? If so, in an area that is dominated by multi-year ice, say northern Ellesmere Island, would such a layer be a good indicator of a period of reduced multi year ice? To me this is the potential value of the identification of this frost flower chemical signature.

ANSWER: The FF would form preferentially in area of young sea ice formation. The detection of more FF chemical signatures in e.g. ice cores from areas around which present-day multi-year pack, could indeed indicate that young sea ice was more frequently formed at that time in the past.

We are grateful to the referee for the technical mistakes he pointed out in the manuscript. The references will be easily corrected. We will add a rose compass on the Figure 1 and draw a red line on Figure 3 to mark the summer surfaces.

REF: Table 2: Is the variable number of significant digits intentional?

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ANSWER: Sorry, this table has been prepared with Excel which automatically suppresses the last digit when it is a zero.

Interactive comment on The Cryosphere Discuss., 3, 159, 2009.

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