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The Cryosphere
Discussions

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

Anonymous Referee #1

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GENERAL COMMENTS

This paper purports to be the first identification and description of frost flower chemistry in an annual snowpack in the Arctic. This paper is a good example of the conclusions that can be reached from careful analysis of a limited dataset. I believe that Pit 1 has indeed clearly identified a frost flower signal in a 20 cm layer of depleted SO4 and elevation marine ions. I am less confident in the interpretation of Pit 2. This, however, does not change the main finding of the paper. This paper adds to the small but important pool of literature on the influence of frost flowers on snow and ice core chemistry. I recommend it for publication following the revisions suggested below.

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SPECIFIC COMMENTS

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 exhibits 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 a points along the Pit 2 profile? 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?

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.

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.

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 calculated to have the same accumulation rate. This interpretation places the top of the 2006 summer layer above the strong [Na+]/[Mg2+] melt layer in Pit 1 but below the [Na+]/[Mg2+] 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?

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?

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: Na2SO4*10H2O. I interpret this to suggest that frost flowers would only expose Na and SO4 on the surface of newly formed sea ice. How does this explain the elevated K+, Mg2+, Ca2+ and Cl- concentrations in Figure 3? Perhaps I am missing something, but where are these ions coming from if mirabilite doesn't 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 doesn't contain either: Rankin et al., 2002; Kaleschke et al., 2004).

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 SO4 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 SO4 concentration in the snow pits. Would this affect the nss-component calculation?)

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 Hin-

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lopen 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?

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?

TECHNICAL CORECTIONS

References are in need of serious attention. There are citations which appear in the manuscript but not the reference section (i.e. Wolff et al., 2003). There are also references which appear in the reference section but are never cited in the manuscript (i.e Sinkevich et al., 1989). Within the reference section multiple papers by the same author do no appear in chronological order (i.e. the Kekonen, Moore and Rankin papers). Within the manuscript when multiple references are cited they do not appear in chronological order (i.e. "Kekonen, 2005; Moore, 2009; and Grinsted, 2006": p. 168: 25). Also, papers with multiple authors are often cited as if they only have one author in the text (without an "et al.").

Figure 1: A compass rose might be useful in order to facilitate the transfer of wind patterns in figure 5 on to the geography.

Figure 3: It would be helpful to clearly indicate the assumed summer surface with a horizontal line.

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

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

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