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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The origin of sea salt in snow on Arctic sea ice and in coastal regions" *by* F. Domine et al.

F. Domine et al.

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Response to comments by Reviewer 0

Reviewer's comment: Question not posed right. Upward migration of sea salt in the marine snowpack should not be placed in opposition to the frost flowers discussed by Rankin et al.

Our response: We agree with the reviewer that the question was not posed in the best possible manner and we will totally reorganized this aspect. In the revised version, we say in the introduction that there are 3 processes that can transfer halogens from the sea to surface snow in polar regions : - Wind blown frost flowers - Upward migration of sea salt from sea ice into marine snow - Sea salt aerosol generated by sea spray and wind-transported to snow.

We have attempted to evaluate from our data the importance of the last 2 processes,

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the first one having been studied by Rankin et al. in Antarctica. In the conclusion, we try to evaluate the potential source strength of upward migration and conclude that it appears to be 1000 times stronger than frost flowers. We do not anymore try to present our findings in opposition to those of Rankin et al., as recommended by the reviewer, but in complement to them. We note, however, that the reviewer is incorrect in stating that our work only deals with marine snow. Our Figs 6, 8 and 9 deal with continental snow, and actually allow a comparison with Antarctic continental data. Our limited data suggest that sea spray is more efficient to transport sea salt to (Arctic) snow than frost flowers to the site of the Dolleman island core, and we discuss that.

In the conclusion, we also place our result in the context of the recent model data presented by Kaleschke et al. (2004), that found a good correlation between frost flowers and ODEs. A possibility is that, although frost flowers may not be the most important source of sea salt to polar regions as a whole, they may be able to produce locally high concentrations of halogens, more likely to trigger ODEs.

Reviewer's comment: Idea in the air is that frost flowers (FF) themselves (or the aerosol derived from them) are the source of the bromide activation. This is because they have a high salinity, which combined with a high surface area, can lead to efficient reaction.

Our response: In our conclusion, we present a calculation showing that upward migration will actually supply 1000 times more halogens to the marine snowpack than frost flowers. Even though some of the numbers used in this calculation are open to debate, it is hard to see how a factor of 1000 could be overcome. As mentioned above, halogens from FF may be more accessible to the atmosphere, but this calculation clearly demonstrate that, when it comes to quantifying sea-atmosphere transfer of halogens in polar regions, upward migration from the sea ice to the snowpack must be considered.

We also note that even though FF have a high salinity, most of this salt cannot be available on the very surface of the crystals, but is distributed over a very thick layer. It is reasonable to assume that in snow, where halogens are more dilute, they will be

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distributed over a shallower depth and that a greater proportion will then be availbale for reaction. Also, Albert and Schultz (2002) have shown that ventilation in the snowpack can access reactants down the whole polar snowpack.

Reviewer's comment: Fractionation.

Our response: Reviewer 0 mentions that the issue of fractionation is not discussed clearly enough in the text. He mentions that Br fractionation is expected only if T$<$-23°C, which is not likely to happen. Temperatures do drop below -23°C in place like Alert even in early fall. However, the temperature of the ice surface may not drop that low and we agree that Br fractionation is not a likely process. We have changed the text accordingly, and to shorten the paper, we have deleted any reference to the work of Koop et al. (2000) does not actually apply to our case studies.

Reviewer's comment: Quantitative calculation of Br deposition.

Our response: Reviewer 0 suggest that we perform calculations to verify that ODEs can lead to enhancements of bromide in surface snow layers. That would indeed be a valuable addition. Ozone depletion is a chain reaction whose chain length will depend on a number of conditions, such that any estimate will require modeling, rather than just a simple calculation. This is clearly beyond the scope of this paper, and would lengthen the paper in an unreasonable manner. We much prefer to leave this matter for future investigations. In any case, it would be easy to come up with order of magnitude estimates that do give the correct answer, but the numbers chosen would be all open to debate and we do not believe that this would serve our cause much. At this stage, we believe that our observation is the important aspect.

Reviewer's comment: Ny Ålesund data not very helpful because of analytical problem.

Our response: There are 2 types of data that were obtained at Ny Alesund. 1- The sea ice data. There were analytical problems. The main point is that very saline, yet low density snow (meaning it was not slush and air could permeate through it) was found

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near the ice surface. Since the snowpack was thin (10 cm), this layer was also close to the snow-air interface and was thus accessible for interactions with the atmosphere. So even though the data are not of the best quality, they do point to the availability of large concentrations of halogens that are not produced by FF and that are available to interaction with the atmosphere. 2- The land data, where no analytical problems were encountered. We believe that those data are highly informative and reliable. Apparently, as mentioned above, the reviewer has overlooked this whole data set, that shows that wind can deposit large amount of sea salt to snow, and that the depth of penetration of wind-transported sea salt is only a few cm. There are no FF and no sea ice at Ny Ålesund, but we believe that this is an advantage here, as we are absolutely certain that the sea salt came from sea spray.

Reviewer's comment: Section 3.2.3 not convincing.

Our response: This section was not meant to be convincing, but to suggest that it may be possible to quantify the deposition of sea salt to snow using commonly measured variables such as wind speed, aerosol loading, and duration of the wind event. This will be useful when we try to model the transfer of halogens to snow. It is preliminary, and therefore not convincing, because we have few data points, but we have to start somewhere. Nothing is proved, but we never claimed to prove anything. Reporting those data will allow other scientists to add further data points to confirm or infirm our suggestion. We strongly think that it would be a mistake to remove that section and the corresponding figure, as we feel it represents a good start toward quantification, and this may be useful to other investigators in the future. We have modified the text to stress the preliminary character of our data.

Reviewer's comment: Abstract and ODEs.

Our response: Reviewer 0 notes that we should not mention that FF are not needed to supply the bromide needed for ODEs, and stresses that we did not see any ODEs in Ny Ålesund at that time. We agree that we may have gone a bit too far in our conclusion.

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We have therefore removed this sentence from the abstract. However, we mention the main point of our conclusion that upward migration of sea salt from the sea ice is a potentially much larger source of halogens to the snowpack than FF, and that, based on our data and those presented by Rankin et al. (2002), sea spray appears more efficient than FF to transfer halogens to snow.

Reviewer's comment: Paper too long for the limited conclusion.

Our response: The referee also wondered whether having both tables and figures reporting essentially the same data was justified. We have addressed this issue by: ů Significantly strengthening the conclusions. ů Making efforts to reduce the text. ů Eliminating Figure 10. Strengthening the conclusion First, we have addressed specifically the contribution of sea spray, which was not treated as a process in its own right in the previous version. Then we have compared quantitatively or semi quantitatively the efficiency of upward migration and of sea spray to frost flowers, in their ability to transfer halogens to the snow. We have concluded that both these processes appear more efficient than frost flowers. We also stress that FF may still be more efficient at triggering ODEs, because they may locally lead to higher halogen concentrations. If a threshold is needed, it may well be that FF are still the main process that drive ODEs. Presence of both Figures and Tables It is true that the data appear both in Tables and Figures. Figures are necessary in most cases to make the data legible and understandable in a minimum amount of time (consistent with the time a busy reader is willing to spend on a paper). Yet, a lot of data is lost in the figures that cannot represent all the chemical data obtained. We feel that basic chemical data is at least as important as the conclusions. Our conclusions may be questioned in the future, but reliable data will remain available for re-interpretation by others, in the light of new ideas. For this reason, we believe that reporting data in the form of tables is a service to future researchers. Of course, we would be ready to have the tables published as supplementary data, but our view is that for a web journal, the tables can be left as part of the paper. In any case, we are sure that the reviewer will agree that it is better to meave the data accessible to

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the community. Reducing the length Our efforts consisted in eliminating Figure 10, in removing non essential text, in text condensation, and in the significant reorganization of the introduction and the conclusion (this later part was almost entirely rewritten). We hope that the strengthened conclusion and reduced length will satisfy the reviewer. The remaining disagreement is over the inclusion of the Tables. We have made our case, and ask the editor's opinion.

Response to comments by Reviewer 1

Reviewer's comment: For the most part, reviewer 1 agrees with the comment of reviewer 0, that have been discussed above. The one extra comment is about the composition of fresh snow. Reviewer 1 believes that we should have discussed this aspect, and compared our results with those of Toom-Sauntry and Barrie (2002).

Our response: We are surprised by this comment, as we present hardly any data on fresh snow. Toom-Sauntry and Barrie sampled falling or very recently fallen snow. On the contrary, we made no effort to sample fresh snow, and made vertical profiles of mostly aged snow. When we focused on surface snow, even that snow was for the most part aged. Toom-Sauntry and Barrie concentrate on the atmospheric processes that led to the observed composition of fresh snow. On the contrary we focus on post deposition processes, that essentially erase the initial chemical signature. Thus, what the reviewer suggests is a totally different orientation and we are sorry to conclude that we do not have the data to address his interesting request.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 4737, 2004.

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