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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.

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Subsequent to the Review process and Open Discussion phase for the ACPD version of this paper, a revised version was submitted for consideration for full and final publication in ACP (this version is not available on here, but is largely similar). The revised version was returned to the first referee for further consideration and comment. Their comments are given below. The Editor agreed with the assessment of this Referee, and has provisionally accepted the paper subject to further revisions as outlined by the Referee. The Authors have been notified, and their response is now awaited. On receipt of the revised manuscript another Editor Comment will be placed here.

Referees Comments:

This is a revised version of the paper I reviewed previously. Actually, it is not much

changed from the last version. The authors have tidied up the text, which makes it easier to follow. They have made many of the minor changes I requested, including changing the way the question is posed. I still have a few comments on the text (see below), which do need to be addressed.

The main area of disagreement is on the length. Both myself and the other reviewer felt that the paper was long considering the capacity of the present data to answer the research questions. This is mainly an issue of the tables and many figures. The author argues that, in a web-based journal, why not include the raw data so that others can interpret them. This would be a point, but I wonder if the editors want to use pages in the hard copy version - many of the data in the table are not used at all (no significant use is made of any of the nitrate, ammonium of potassium data for example), and most of the data that are used appear in the detailed figures. I would accept the data being in an online supplement but still would argue against including them as hardcopy - but this is an editorial decision now. I will also continue to argue against figure 10 and section 3.2.3 (see below). With these caveats, I would agree that the paper, while not a major breakthrough, warrants publication in ACP (after corrections below), as a useful started to a debate about the sources of sea salt.

More detailed comments:

Page 10, mid: “Fig 11 shows that equation (1)”; should be Figure 10 now.

Page 10, section 3.2.3, and Figure 10. I would still argue that there is insufficient data here to even have the discussion. The authors say that the equation holds only for Ny Alesund - how can they tell, since there is only one datapoint for Alert? The authors draw a line through zero, but no-one would believe you get no deposition in still conditions, and indeed many decades of work on dry deposition would confirm this. There are many previous datasets (ignored here) comparing snow composition with aerosol composition (for example at Dye 3, Summit, Halley, etc.) and the picture is clearly not as simple as the straight line through two datapoints would imply. Without

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far more data, the whole discussion is meaningless, and I would just delete it.

Tables 3 and 4: the authors seem to have reverted to an older version, in which some parts are still in French (Spitsberg instead of Spitsbergen, fevr and mai instead of Feb and May). Also, I still suggest adding a date for the Arctic Ocean dataset in the table.

Page 11, mid. The comparison between Dolleman Island and Ny Alesund: No-one ever said that sea spray would not be important if there was open water nearby. Indeed it is obviously the case (for example everywhere in the world where there is no sea ice), and is discussed for example in relation to Dumont d'Urville (Antarctica) in (Wagenbach, D., F. Ducroz, R. Mulvaney, L. Keck, A. Minikin, M. Legrand, J.S. Hall, and E.W. Wolff, Seasalt aerosol in coastal Antarctic regions, Journal of Geophysical Research, 103 (D9), 10961-10974, 1998.). In this case the authors compare a site at Ny Alesund which is "30 m from the shoreline", with a site at Dolleman Island, 20 km inland and 400 m above sea level. This difference should at least be mentioned if the authors want to make this rather simplistic comparison.

Page 11, near base of page. The budget calculation for frost flowers and snow on sea ice is an interesting one, and I am pleased to see it. However, it should be correct, and it is unfortunate that the paper and the response of the authors posted online are in gross error (2 orders of magnitude). Let's assume for now that the estimates of area, density and concentration are correct. Then the ratio of area (sea ice/frost flowers) is 99, that of thickness is 20, and of density 30, which multiplies up to 60000. But the concentration in the frost flowers is 6000 times higher than in the snow on sea ice. So the eventual result is that the marine snowpack contains 10 times more salt than frost flowers, not 1000 times. Given that 230 uM is the highest concentration found in this study, the surprising conclusion could be that the amount of salt in snowpack and frost flowers is actually quite comparable. However, let's not stretch this too far; of course I agree that current frost flowers do not form the bulk of salty material in the sea ice zone, but the authors and I probably do agree that the brine layer on sea ice, as the origin of both upward percolation and frost flowers, is the most important source of salt

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in the sea ice zone.

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