

Interactive comment on “Sea ice dynamics influence halogen deposition to Svalbard” by A. Spolaor et al.

Anonymous Referee #1

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A review of “Sea ice dynamics influence halogen deposition to Svalbard”

This paper presents an intriguing data set and is generally well written. It could have interest to members of the fields of sea ice biogeochemistry, atmospheric chemistry, and snow physical and chemical processes. The authors present a set of data spanning a ~eight year period for which they relate bromine and iodine concentrations in snow and firn to sea ice dynamics. Their methodology is sound overall but I have some comments about how they selected using the pan-Arctic sea ice coverage to present their findings. I also would urge them to investigate whether winds (blowing over open leads, water, or ice) could play a role in the signals they present. They likely have more chemical composition data to contribute to this story (ie the 28 trace elements they measured). The total lack of investigating wind fields, back trajectories, etc. is a

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weakness in the paper. Even a wind rose from an area nearby for each year would be worth presenting. I suspect this paper's results could yield intriguing discussions about how relationships between snow and the snow chemical archive can be used to "back calculate" sea ice presence, absence, and dynamics. But some more support from meteorological, sea ice, and chemical composition data would make it a far better paper.

General comments keyed to the text: Line 29: "the" is not needed after "and"

30: They mention a "shallow ice core" but in fact they were really drilling into firn, yes?

35-37: I am not aware that the bromine explosion is associated only with first year ice? This may be the case but I am not aware that it does not occur over multi-year ice and/or over some inland locations near the coasts. Linking it to first year ice (only) may not be appropriate.

46: "spring and summer" ? "Spring-Summer" is not correct

59: There are a lot more recent references than this 1995 paper to exhibit "recent. . . . observations"

62 and 63-65: there are references for these statements that are more valuable than web URLs.

67: exceeds

75: of Arctic and

80 and most other locations where multiple references are presented: there seems to be no space between the " ;" and the first letter of the next reference.

102-103: Results from many studies have shown that blowing snow over sea ice is another way to move ions around from ocean to land. See Simpson et al. JGR 2005. Have you explored the sulfate to sodium ratio on the samples to investigate the potential frost flower signal? Ie whether it exists or not? Or is related to specific ice conditions?

Many of the frost flower versus sea ice surface papers use this ratio as a tracer.

Regardless it is not as simple as “sodium comes from two sources”

What do iodide and bromide do in the dataset investigated?

154: “Sample”

164: due to sample processing

167: snow pit samples or core samples or both?

237-240: I do not buy it that the atmospheric iodine is associated with air flow over open water. What about locations where there was open water and nilas, centimeters thick, forms? This would be thin ice that could be closer to the Antarctic situation mentioned. This situation, close to the idea of “potential frost flowers” posed by Kaleschke and others in the mid-2000s, would create locations where iodine could be sourced through the ice rather than open water. Yet they were recently/formerly open water.

Have you looked at MODIS images for the times represented in the data to see what the ice looked like in terms of leads?

What does the ice typically look like upwind of the sampling location? Are there polynyas, areas of open rubble, areas of dynamic ice, and/or areas of landfast ice? This would also be related to the applicability of the “greater Arctic Ocean sea ice” extent as per processes occurring near the field sampling site.

263-281: The above comments are related to this section.

272: Greenland Sea

275-276: I am not sure I follow this. Because the winds and ice dynamics are so complicated the authors decide to use the overall Arctic sea ice area? This is a severe weakness of the paper. They either have not looked at winds, imagery of ice dynamics and morphology, and back trajectories (which would be overlooking major drivers of

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these data/information) or they looked at these data/information and did not find it “fit” their dataset (bad science). At the least they need to expand this justification of why they are picking the overall Arctic sea ice area. Otherwise it looks too good to be true. Maybe some Supporting Information data and plots?

276: Did the Comiso paper look at the area of interest? If not then it may not be relevant. If so then state that.

279-281: good job acknowledging this.

288: why are frost flowers referenced here? It may be a diversion/confusion for people since they are not part of the conclusions of the work.

299: in the surface

298-303: Again I wonder about winds. Also the “bromine explosion” is but one piece in the halogen chemical reactions that occur during Polar springtime. The bromine explosion events are an intriguing process and may be part of the story here but they are also part of the more complicated and more spatially diverse ozone and mercury depletion events. Obviously, where bromine production can/does occur this provides a potential bromine source for the snow record. However, this only occurs in wintertime.

How does the data account for years like 2006 and 2009 and 2010 when there is more bromine present in summer deposition than during the winter time of 2003-2005? To invoke sea ice as the source in late spring then how could some summer and fall deposition patterns have higher bromine values when bromine explosion (and sea ice) are not present?

Same type of question but for iodine: where does the iodine come from during non sea ice conditions? If it is related to open sea water in the pack ice then why are the summer time deposition values not higher than winter?

These aforementioned questions may be attributable to firn melting during the summer perhaps and smearing the winter time signal (?). Is there temperature data from the

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site or nearby to provide information on what summers experienced melt and to what degree did downward percolation affect the firn record? Can the information in Figure 2 on ice lens presence and size be used to provide insight into this? A statistical analysis of concentrations above, within, and below ice lenses may shed some insight?

311-312: Is there data on precipitation or snow layer concentrations from this or other studies? Are cooler seasons associated with more or less snow precipitation events (?) and could this affect concentrations?

316: “to Svalbard glaciers”

325-328: same comments as before: it seems to easily explained away that the local ice and meteorological conditions are ignored. If you are going to do this I would recommend at least some Supporting Information showing the local conditions and how/where they are the same (ie are represented by) or different from the total Arctic sea ice areal extent.

344 and onward: Looking at the “winter” time periods identified in Figure 2D and the peak sea ice information and I concentrations in Figure 3 I wonder why some years have their highest I concentrations in winter and some have their lowest I concentrations in winter. This would be in disagreement from the hypothesis that the I comes from open water during the sea ice time (winter). Lines 261-262 suggest that ozone could play a role but the ozone fluctuations are likely in late spring (when bromine explosions are likely and ozone depletion events likely) so is there any link there?

Lines 353-358: the statistical testing to identify winter time periods from the oxygen isotope data may be overkill. It also may ignore the fact that the peak identification issues in 2005 are associated with a period of large snow fall, low density snow, and a large ice lens. Perhaps this is associated with a downward smearing of the oxygen isotope results?

365-367: Where are these statistical data?

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367-368: given the short data series acknowledged is it appropriate to apply the various statistical treatments to the data? I pose this somewhat rhetorically but a N of 10 or so (ie the # of winter or summer periods) may not be sufficient and/or the distribution of the data may not be correct for the statistical analyses. Some acknowledgement of the applicability of the statistical analyses to the N and distribution of data should be provided. Could be a few sentences here and there.

Figures: 2 the “medium lens” and “large ice layers” are the same sized closed symbol in the legend but are different sizes in the graph.

3 the blue circles for “spring sea ice extent” are blue squares of larger size in the actual plot

4 Blue circles denoting Average extension of seasonal sea ice are larger in the plot than the legend.

Interactive comment on The Cryosphere Discuss., 7, 1075, 2013.

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