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

Anonymous Referee #3

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This manuscript shows observations of halogens (bromine and iodine) in a firn core extracted from a glacial cap on Svalbard. The manuscript is generally well written and readable and of interest to ice core and atmospheric scientists. The firn core is dated by oxygen isotope measurements as well as glaciological methods. Core extraction, sample preparation, and analysis of bromine and iodine are well documented and appear sufficient. The paper makes some interesting direct findings, such as: Bromine is enriched in nearly all samples, although at lesser values than prior snow work (see below). Iodine is greatly enriched compared to sea salt tracers, as the authors indicate that "...sea salt I was found to consistently account for less than 2% of iodine concentrations". While it is exciting to see these observations from glacial records, interpretation of the manuscript appears overreaching given the lack of understanding of post-depositional processes for these species, use of pan-Arctic ice indices for

what are likely regional processes, and indications in the data that post-depositional processes may be active.

Major points:

The lack of seasonality in the halogen data is of concern in the light of the fact that the oxygen isotope data indicate that seasonal layers exist and are not sufficiently influenced by snow melting and percolation to destroy seasonal information. Specifically, the authors state on page 1083, line 11 that "melt events have only a minor influence on the seasonal climatic signal determined from the delta-18O signal in Svalbard cores". This statement seems to say that seasonal information should exist in the firn core for halogens. The fact that seasonal cycles are not very evident for the halogens then seems to indicate that post-depositional processes may affect the halogens, which would cloud the interpretation here. Specifically, Fig. 3 doesn't seems to show any annual cycling, and Fig 4 is said to show annual cycling in "in the upper part of the core". The annual cycling seems to be two years, which is not very convincing given the apparent lack of cycling in prior years. If the argument being made is that Br and I are enriched during early springtime (March-May), why doesn't the data show that? Toom-Sauntry and Barrie (Atmos. Env. 36, p2683, 2002) show a very strong seasonal peak for bromide enrichment in March-April-May, with enrichment factors compared to sea salt ratios using sodium as a sea salt tracer having values up approaching 100x sea water ratios, while fall and winter values are much closer to sea water ratios. It seems surprising that such a strong seasonal cycle should not be retained in the snowpack. Possibly in the case of I, the season is longer, and arguments are made for Autumn I production, so one might expect I to show less annual cycling. Overall, my point is that why should water show annual cycling but halogens, which are hopefully faithfully archived, not show annual cycling?

The authors attempt to minimize the possible influence of depositional and post-depositional processes. However, we are aware of significant snowpack chemistry of bromine species and snowpack chemistry is also likely for iodine species. Pratt et al.

(Nature Geoscience, DOI: 10.1038/NGEO1779) document that irradiated snowpack can produce halogen gases, and observations of bromide ions in snow indicate that some samples can be depleted in bromide (e.g. Krnavek et al., Atmos. Env. 50, p249, 2012). The observations of the highest amounts of iodine in the snowpack near the surface, and lower values in buried snow might also be an indicator of post-depositional loss of iodine. These known post-depositional processes seem likely to be modifiers of snow / firn composition.

The use of pan-Arctic tracers of the state of the sea ice seems like a poor choice, at least for bromine chemistry, which is probably much more regional in nature. The general reactivity of iodine, and the observations of Mahajan et al., 2010) that iodine chemistry is associated primarily with upwind open leads or polynyas also indicate a regional nature to iodine chemistry. Therefore, it would seem useful to produce regionally representative sea ice products to compare with the pan-Arctic metrics used here. The authors make what is likely a good argument that Comiso et al. (2002) find that trends in various sectors of the Arctic approximate the overall trend. If this is the case, then regionally representative sea ice metrics would be correlated to the pan-Arctic metrics shown here and the authors would have a stronger argument than what is presented here. Given the availability of high-time-resolution sea ice data for this period, calculation of regional sea ice indexes is achievable.

Minor / technical points:

The manuscript makes use of seasons (e.g. early spring). These periods are not always consistent between past papers, so use of the term plus month (or month range) from the referenced papers would be of assistance in determining that part of the annual cycle is being discussed. Please consistently use either month names or season description with month names in brackets after the description.

p1077, line 15: It would be useful to mention the early satellite period average summer minimum sea ice extent. The September 2012 value was just about 50% of the early

satellite period, which is an importantly large change.

p1084, line 7: If Na is not to be used as a conservative tracer here, should some other normalization be used?

p1084, line 25: I think it would be more accurate to say that "It is possibly due to the greater thickness....", as questions of how iodine escapes sea ice are not well established at this point.

p1085, line 3: This section is a bit confusing because the maximum I concentration is not seen as peaking in the springtime from the firn core data – no seasonal cycle is evident in the I concentration – but instead, a month-selected ice extent is being compared. One could read this to say that I peaks in the firn core data. Please reword.

p1086, lines 14-16: I don't see how one can argue for in-situ smoothing of I without in-situ smoothing of water isotopes.

p1087, lines 14-15: The fact that Br is generally enriched indicates that enrichment dominates over depletion, not that depletion doesn't sometimes exist. In fact, enrichment on the order of snow observations from Toom-Sauntry et al. (2002) are much higher than shown here, so the lower values seen here may indicate some depletion in addition to the enrichment process.

p1087. lines 23-24: This statement is not well supported in the data. Annual cycling that would indicate cool season versus warm season is really only evident in two years of data from Fig. 4. The caption somewhat captures this idea, but the text seems to say that there is general cycling with temperature, which is not seen.

p1087, line 25: The seasonal peak of Br enrichment is likely due to the high latitude site of Alert and the seasonally later polar sunrise at that location, not really a direct function of temperature.

p1088, lines 2-4: This sentence is awkward and should be reworded.

p1090: The conclusions should more clearly point to post-depositional processes as possibly being evident in the data and the need to understand them to interpret these data more fully.

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