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Interactive comment on "Seasonality of halogen deposition in polar snow and ice" *by* A Spolaor et al.

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We appreciate the referee's interest and helpful comments.

Some specific comments/questions:

1. The Conclusions section should do a better job of pulling together the Arctic and Antarctic results. The striking differences in behavior of I should be noted and explained. If the authors themselves don't have a clear explanation, that makes the paper more interesting and should be highlighted both here and in the Abstract. The sentence "In Antarctica, Bromine and Iodine seasonal cycles are clearly preserved..." gives the misleading (at least for I) impression that the atmospheric seasonality is preserved. That impression is strengthened by the following sentence, stating that "These

results illustrate that halogen production events observed by satellites are successfully preserved in polar snow and ice."

We agree that it has not yet been established that atmospheric seasonality of Br and I are directly preserved in polar snow. Indeed the results presented here (as well as the work of Frieß et al., 2010) indicate that there is remobilization of iodine and possibly also bromine in Antarctic surface snow in the presence of sunlight. We still need to establish the extent to which this remobilization occurs and how greatly this may affect the reliability of Br and I as quantitative proxies. We have rewritten the relevant sections of the abstract, discussion and conclusions to better summarize our findings and to highlight current gaps in the application of halogen proxies for paleo sea ice reconstruction.

2. Is it of any possible importance that the Arctic measurements were done on firn, while the Antarctic measurements were on ice? Is it possible that the apparent iodine seasonality evolves over time in the firn?

From the submitted manuscript this could be considered a possible explanation for the differences observed between Arctic and Antarctic sites. Fortunately, we have recently become aware of the related findings of Frieß et al. (Atmos. Chem. Phys., 2010), which allow a comparison of iodine in Antarctic coastal surface snow and show clearly that winter iodine peaks in Law Dome ice are also present in Neumayer surface snow. Hence it appears that different processes are at work regarding the retention and/or production of iodine seasonality in Antarctic and Arctic snow.

3. If the seasonal mobility of Antarctic iodine involves emission to the atmosphere, then variations in wintertime atmospheric transport could influence the amplitude of the local signal in ice year to year. Perhaps that could influence the interpretation of the signal as a paleoproxy.

We agree with the referee that atmospheric transport will influence the presence of iodine at the deposition site and may lead to variability from year to year, although

such effects must be considered for all of the aerosols and impurities that are found in polar snow and ice records. The best way to distinguish between transport variability and emission source strength would be to compare recent iodine and bromine fluxes with the satellite record of sea ice variability. In combination with such comparisons, chemical transport modelling is essential to accurately constraining the mass balance and seasonal variability of the studied halogens. These approaches are a priority for future work.

4. In this study, Br_enrichment is used to quantify the bromine signal and iodine concentration is used to describe the iodine signal. That is like comparing apples and oranges - one is a ratio and the other is a concentration. I can see the utility of Br_enrichment for detecting timing of seasonality, but not quantitative comparisons. For example, interannual variations in Br_enrichment could easily be influenced by changes in seasalt rather than reactive Br. Br_excess (Br_total minus Br_seasalt) is a better quantity to compare to bromine levels between various years or to compare bromine and iodine levels.

We agree with the referee that Br-enrichment is a sensitive indicator of seasonality, whereas nssBr is more suitable for evaluating trends in Br emission and/or production by processes such as Br explosion events. We have updated figures 3 and 5 to show both nssBr and Br enrichment, as well as explaining in the text how we have calculated these parameters. For the purposes of evaluating seasonality, Br-enrichment and nssBr are both effective at demonstrating that seasonal cycles of Br are present in Law Dome ice and hence the specific indicator used does not change the conclusions arising from this work. With regard to the Arctic samples, again we see good correspondence between bromine enrichment and nssBr. To be consistent with earlier work, we have decided to retain our presentation and discussion of Br enrichment but have also added nssBr where relevant. As regards the comparison of iodine and bromine results, we believe that our approach is valid because iodine and bromine are complementary and independent proxies. They are complementary because their emission

is linked to the presence of sea ice and they are both involved in photolytic reactions involving ozone destruction. They are independent because they have completely different emission sources - bromine is emitted from the ocean as sea spray and iodine is emitted from marine biota and algal colonies present under sea ice.

5. If the authors feel there is a compelling reason to use Br-enrichment, they should explicitly define it is calculated, how Na was measured, and how analytical errors were propagated. Also, the Na data itself should also be presented so the reader can compute Br-excess if they wish.

The full datasets presented in the manuscript are being made available to the public via the PANGAEA web archive (doi:10.1594/PANGAEA.833942).

6. Element names (bromine, iodine) should not be capitalized unless abbreviated

The text has been modified accordingly.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 8185, 2014.