AUTHORs RESPONSE

Thank you for the revised version of your paper. Overall, the reviewers performed a positive evaluation of your paper and your response to their comments appears adequate. I am therefore inclined to accept your paper but I would first request that you please consider the following minor changes.

All the changes in the manuscript are highlighted in red

Lines 161-163. That Br and I concentrations are not much affected by photochemistry would be better understood if the fairly high accumulation rate were mentioned at the same time. Presently, it is only mentioned in the subsequent paragraph, too late to be convincing upon reading.

We modify accordingly.

Line 292. Cannot "Heterogeneous reactions on sea salt particles and seas-salts on sea ice" be part of the bromine explosion?

We modified the sentence to be more clear.

Line 297. Please consider replacing "proportion to" by "factor by", if I understand you correctly.

Modified accordingly

Section 4.1. Would it be useful to discuss also the fraction of first year sea ice as an important determinant of the Br excess? See [Simpson et al., 2007], whom you cite.

We add a sentence to be more clear (line 310)

In section 4.1 we consider the spring sea ice in the Laptev sea because almost all sea ice in this basin in spring time is first year sea ice. Sea ice in the Laptev basin is almost complete removal during summer and is also in the middle of the transpolar drift that promote the sea ice recycling. In any case we add a sentence to be more clear. Line 309

Figure 5. How about adding a Na smoothed line for homogeneity and ease of comparison?

We add the smoothed line

Figures 6 and 7. How about XY graphs of Br and I vs. sea ice area? These would be informative to visualize the scatter. Indeed, although interesting and significant, the correlations you obtain are not very high and seeing the error made in using these correlations would be very useful. I am sure you realize that many other factors determine excess Br, and that, although very interesting, excess Br is a good but not excellent proxy for sea ice area. I believe it is important not to mislead the quick reader as to the quality of the relationship.

We include a new figure (Figure 4) as suggested.

In that vein, I would also highly recommend to modify your abstract and indicate the value of the correlation coefficients that you obtain. You state "These two halogens are therefore good candidates for extended reconstructions of past sea ice changes in the Arctic." This may be perceived as somewhat exaggerated. I am convinced of the interest of your study but please do not

try to oversell it. Given the correlations found (which are quite interesting), you will probably agree with me that using excess Br or I as a proxy for sea ice extent will bear a significant error. This has to be made clear in the abstract.

We modify the abstract accordingly.

Thank you for you attention to these few points.

Reference

Simpson, W. R., D. Carlson, G. Honninger, T. A. Douglas, M. Sturm, D. Perovich, and U. Platt (2007), First-year sea-ice contact predicts bromine monoxide (BrO) levels at Barrow, Alaska better than potential frost flower contact, Atmos. Chem. Phys., 7, 621-627.

The reference has been included