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The Cryosphere
Discussions

Interactive comment on "Bromine, iodine and sodium in surface snow along the 2013 Talos Dome – GV7 traverse (Northern Victoria Land, East Antarctica)" *by* Niccolò Maffezzoli et al.

Anonymous Referee #1

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It is well-established that the sea ice zone is a hotspot for Br (and with less certainty, I) chemistry. This has been shown by the high concentrations of BrO observed from satellite data in spring, and from ground-based data. The mechanism is understood to be that inorganic Br is activated from salty material (sea ice or aerosol). With this in mind, Spolaor et al (2013b in the present manuscript) found that Br was enriched in interglacial ice and depleted in glacial ice at Talos Dome. They proposed that this resulted from the halogen chemistry over sea ice, and that the enrichment or depletion might be used as an index of sea ice extent in the past. Their mechanism relied on their suggestion that Br/Na would reduce with distance from the sea ice edge, because the enriched material (as gas phase HBr) was deposited faster than the depleted (NaBr)

sea salt aerosol. This was a surprising suggestion because previous work (Simpson et al, 2005) had suggested exactly the opposite (for the Arctic): that inland snow would be enhanced due to a longer lifetime of HBr.

Clearly it is impossible to consider Br as a sea ice proxy until at least a reasonable understanding of the mechanism leading to temporal changes is understood, so studies attempting to elucidate this are very welcome. The present paper is aimed at doing this, by making a spatial transect (including seasonal information) of Na, Br and I in a part of East Antarctica. Unfortunately the location of the traverse is particularly badly chosen for such a study, because the sites sit between two marine areas, the Ross Sea and the main Southern Ocean. This has the effect that sites that are further from one potential source are nearer to the other, so that even if the data showed very clear trends, opposing interpretations would have been possible. As it happens there is little clarity in the data, which is not entirely the authors' fault but does mean that this is a paper which advances knowledge only incrementally. It is probably justified to consider publishing it after significant changes have been made, if only to indicate the complexity of the problem, and to show how premature it is to consider Br as a sea ice proxy until far more detailed and well-designed experiments and sampling campaigns have been carried out. The paper itself is relatively short but with a very high number of tables and figures that really don't add to it, so among other things I would recommend losing some of these in the next version. There are also some unjustified interpretations (such as that in the abstract regarding Fig 9), which definitely must be modified.

Comments:

Abstract line 27-28. The last sentence is not justified. This is based on Fig 9c, with 4 data points. No statistics are given but I see no correlation at all, and a rough attempt to plot the data gave an r^2 very close to zero, utterly insignificant.

Sections 1 and 2 are generally OK, with two minor comments:

Line 36, remove the word layer. There is a "layer" of ozone in the stratosphere but there is no layer in the troposphere.

Line 68. The Rothlisberger article in a newsletter is not a good reference here. Better would be reference 3 or one of the other papers by Abram.

Line 162-167. While I agree that, taking old and new data into account, there is a tendency (as one would expect) of lower accumulation rates as we move further from the ocean, obviously this is somewhat undermined by the high value of 185 kg mˆ-2 aˆ-1 at TD. In the table the authors try to mitigate this by putting asterisks on "uncertain years", saying that their value is uncertain because the isotope signal is less clear. I don't think Fig 2 really justifies this – the least clear assignment at TD (2012) is at least as obvious as the one for 2011 at site 8 for example, and yet this has no asterisk against it in Table 2. I think a better way to handle this would be to remove the asterisks from the table, but to say that the inconsistency between the accumulation rate derived from the core at TD and that derived from the stake farm and previous measurements suggests that the isotopic assignments of years may be incorrect at TD, and that the profile contains more years than have been assigned.

Table 3 is unnecessary. Most of the information is anyway given in the text, but anyway a table like this that mixes different sites has no value that I can see. The table should be removed.

Line 189. This sentence is not really correct. Either enrichment or depletion can indicate that the reactions, believed to be focussed on sea ice, have taken place (not just enrichment). In fact the reaction (1) as shown leads only to a depletion of Br from the sea ice. It is only if the Br2 is eventually converted back to HBr that enrichment can occur, if more of this end product gas phase HBr is deposited nullifying the depletion in the aerosol phase. In addition, there are other ways to get such enrichment, as we know from the case of Cl, which is enriched or depleted compared to sodium due simply to production of HCl from the reaction between sulfuric or nitric acid with sea salt (e.g. H2SO4+2NaCl-> Na2SO4 + 2 HCl). A sentence that would be defendible

would be "Therefore sea ice presence should lead to Br enrichment (after conversion of activated gas phase Br back to HBr) or depletion, depending whether deposition is dominated by the depleted sea salt aerosol or by the enriched gas phase HBr."

Fig. 3 is OK, but I would have found it more useful if you had shown the distribution for individual sites or groups of sites. As a suggestion, you could show one distribution for sites GV7/8/7/6 (less than 100 km from ocean) and another distribution for the other sites.

Fig 4, TD plot is obviously a problem, since you seem to believe that (Table 2) the years may be misassigned. This undermines your interpretations for this site. I'm afraid you can't have it both ways – either the TD accumulation is really high in which case the statements made about how accumulation varies with distance are not supported, or it is not in which case the year assignments shown in the TD section of this figure are wrong.

Fig 8 is really confusing because the map is upside down compared to that used in Fig. 1. In any case a similar figure is shown in Fig 1b. therefore please remove Fig 8, simply incorporating the additional information (basically the red box showing the 130-190E band) into Fig 1b.

Line 215 – why is insolation from a site on the opposite side of Antarctica shown here? It is not even at the latitude of the sites here. Furthermore total solar radiation is very unlikely to be relevant, rather it is the radiation at the UV or near-UV wavelengths that might promote the relevant photochemistry. I suggest using a code such as TUV to calculate available radiation at relevant wavelengths.

Line 205, and Fig 9b. It's hard from Figs 4-7 to really see the seasonality, which you describe as max in late spring/summer for Br. Since you must have calculated it to get to Fig 9b, please add a plot of this sort for each site so we can see how consistent the seasonality is at different sites. This could be shown as individual lines underlying the error band in Fig 9b for example, or as a separate panel if this is less confusing.

Line 221-3. The comparison of the seasonality with the product of radiation and sea ice extent is interesting but it does not “demonstrate” the dependency of Br enrichment on their combined effect, rather it is “consistent with” the idea that there is such a dependency. Please adjust the text.

Lines 225-7. As in the abstract, the correct characterisation of Fig 9c is that, with only 4 years of data, no relationship can be discerned. It is not scientific to say that there is a relationship for 3 years and not for the fourth – statistically this means there is no relationship. You just have to admit that there are not enough data here to know whether Br_enr can be used as an indicator of FYSI. In fact (and related to the next comment) nothing you have written here says why you would expect the enrichment to be related to the FYSI area; what mechanism are you envisaging? If, as in the earlier Spolaor paper, it is via distance from the ice edge (through whatever process) then this should be somewhat reflected in the difference between sites, hence my next comment.

Section 3.3 and Fig. 11. I agree that the Br pattern looks similar to the Na pattern. But this then begs the question, what is the pattern of Br_enr. My impression from this figure and the data in Figs 4-7 is that Br enr is probably rather flat with distance. Since this was the issue that Spolaor suggested controlled the TD glacial-interglacial change, it deserves to be shown and discussed in that context. Please add a discussion.

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