

Review of “Bromine, iodine and sodium in surface snow along the 2013 Talos Dome – GV7 traverse (Northern Victoria Land, East Antarctica) by Maffezzoli et al.

This paper reports measurements of sodium, bromine, and iodine made in shallow cores in East Antarctica. The data are examined to assess, primarily, bromine enhancement, and potential as sea ice proxy. The subject area is very important and the data are interesting. Overall, however, I find that the paper is overly superficial, and lacks necessary supporting information, including references, to substantiate the arguments presented here.

Major concerns:

Analytical information is lacking. The authors present very few analytical details, instead referring readers to the Spolaor et al, 2013a paper. However, some information will be specific to the Maffezzoli paper and should be included, and some additional information would be useful. For example, what was the residual standard deviation for Br, I, and Na for the current work? How were the standards prepared – gravimetric or volumetric methods? Standard concentrations ranged between 10 and 4000 ppt, but for for which species? Spolaor 2013 refers to iodine and bromine being calibrated in this way – but what standards were used for sodium? Presumably halogen standards were separate from sodium standards – did this introduce any uncertainties into the analyses? I am not so familiar with this technique – does it analyse Na, I, and Br in a single run? If not, what uncertainties does this introduce in terms of instrument drift etc? Does the method use columns to separate out the elements of interest? If so, what columns were used? Was any reference material used to really pin down the analytical technique, given the extremely low concentrations being measured? Exposure of samples to direct light was minimised, but how long were they actually exposed for? How long were the samples melted for before they were analysed? Were there any repeat analyses of samples carried out? How much liquid water was needed for each analysis? Presumably the samples were re-frozen before being shipped to Copenhagen for isotopic analysis. Is this likely to have introduced any problems/uncertainties into the analyses? Finally, the manuscript states that during sampling, every tool was repeatedly cleaned with ultrapure water – how many times is “repeatedly”. What effort was made to ensure that the tools were clean?

Dating: The conclusions drawn in the paper rely heavily on correct dating of the cores, i.e. correctly allocating samples to specific seasons. This is done using stable water isotopes, and the manuscript states (line 140) that “isotope ratio minima (representing mid-winter) can be easily identified”. But the authors must give the criteria used for such identification, if other than just by eye. As the authors state, there is clearly a question over assignment of the 2013 mid-winter – how did the authors select the one chosen? I also wonder what happened to the top layer of core 6..? Was it damaged during sampling? The accumulation rate of core 10 is less than core 9, so one would expect the snow at 2m depth to be older in core 10 than in core 9; however, this is not the case as presented in the paper. It makes me wonder whether mid-winter assignments in core 10 are out by a year? Certainly, more information on the criteria used to allocate winter minima must be given for cases where it is not completely clear.

Please explain fully how accumulation rates were calculated, rather than just saying (line 153) that measured density profiles were accounted for – explain the method.

Was only 1 core taken in each site? How do you account for variability between cores drilled at the same site? If this is not considered important, then please say so and explain why.

The authors must provide justification on why they chose to use the stake farm data for Talos Dome over their own method. It is not enough to reject your method, which is used on all other sample sites, just because it does not agree with stake farm data at TD. This selection raises concerns over the validity of the method used for all the other sites.

Supporting information/background text: Contextual information is not of sufficiently high standard for The Cryosphere. Information provided is at time simplistic, and at others erroneous. Referencing is poor and must be improved. Many statements are unsubstantiated.

Below are more minor comments:

- Line 16: halogen chemistry does not only occur through release of sea salt rich aerosols; various saline condensed phases have been suggested;

- line 18: the statement “halogen species in polar snow samples are shown to be closely related to sea ice extent” is too strong – there is clearly a link to sea ice, but this is far from quantified, so how close it is related is not yet known.

-line 25: the transect revealed homogeneous fluxes” – what type of fluxes? Air-to-snow..? Snow-to-air..?

-line 27: “flux measurements are consistent with the uniform values of BrO and IO”... uniform in time or space..?

Line 36 – there is no tropospheric ozone layer; remove the word “layer”

Line 36 – there are many other papers that should be referenced here other than Barrie et al. There are also some excellent reviews, e.g. Simpson et al. 2007, Abbatt et al. 2012, that should also be included here.

Line 38 – explain why young sea ice surfaces have high salinity, and how they are thus a source of bromine compounds (presumably you mean to the atmosphere). Also, what are “bromine halides”..??

Line 43 – include the reactions and improve the wording here

Line 45- MAX-DOAS generally refers to a ground-based instrument technique; SCIAMACHY is a satellite-borne uv-vis-nir spectrometer that quantifies BrO and IO columns using the DOAS technique. Please re-word. Also state which satellite instrument you refer to.

Line 47 – “from halogen-rich condensed phases (e.g. sea salt aerosol)” – there are likely to be others as well.

Line 49: Such reactions ... lead to enhancement of bromine in the deposition in the surface snowpack - give a reference to support this statement.

Line 51 – is the Spolaor reference 2016 a or b?

Line 52 – the Vogt paper is a modelling study, so does not include primary information on iodine sources

Line 66 – DMS is produced from DMSP, not directly from phytoplankton

Line 70- I believe Mulvaney and Pasteur were the first to report post-depositional movement of MSA in ice cores.

Line 78 – “Back trajectory calculations show that favourable events of air mass advection from the sea ice surface to TD are rare but likely to occur” – what calculations are these? Which model? Did the authors run them, or are they in a published paper? If the latter, then give references. If the former, give more information. An example plot would be useful with some sort of analysis of air mass origin.

Actually the point being made in paragraph of line 72 to line 82 is not clear.

Line 90 and 93 – “Indian **ocean** sector”

Line 92 – Friess et al did not report Br in their snow pit.

Line 104 – it would help the reader to give the distance between the cores.

Line 184 – The description of bromide release needs improving. e.g. “bromide ... is recycled over halogen-rich sea ice surfaces” – these are the sources; recycling is different. Please clarify what you mean.

Line 187 – give the phases of the reactants and products in reaction 1

Line 191 – add reference to Simpson et al. GRL (32), 2005

Lines 198 to 200 should be used as the figure captions in Figs 4 to 7 – what is currently used is inadequate.

Line 205 typo: “maximum values ~~in~~ during late”

Line 209 and Figure 9a – how was sea ice area calculated; did it rely on the threshold of >15% sea ice in a pixel? More details are needed. But further, and importantly, there is no justification given of why the 130 to 190° sector is chosen. This is why proper assessment of air mass origin and history (as detailed above) becomes important.

Line 215 – explain why you used the data from Syowa and why they are the right data to use (presumably because they are similar latitude, but you need to say that)

Line 216 – Figure 9b relies entirely on correctly dating the cores, and correctly attributing the months to the measurements. This goes back to my concern about dating, above. There needs to be complete assurance that this has been done correctly.

Line 237 and Fig 10 – the figure shown originates in a paper by Anja Schoenhardt – the original reference needs to be provided, not the secondary one (Spoliar et al). Also, these data are not tropospheric measurements – they are vertical columns from space that have not been adjusted for any stratospheric component.

Line 244 – the polar night does not “start” in winter – please re-phrase

Section 4 – the Conclusions need to be re-visited in light of the above comments. In particular, the “Uniform satellite values of BrO and IO over Victoria Land confirm the snow measurements” is far too strong. They might “be consistent” with the snow measurements, but they do not confirm them. Finally, line 291, the halogens are not yet “proxies” as they are not rigorously demonstrated – they are potential-proxies, but not yet proven.

Table 3 – include also the median, given the statement in the text about high episodes, the median then becomes important.

Fig 1 – it would be useful to have latitude/longitudes on the lower maps or the inset maps to clarify how the zoomed-in map relates to the continent-wide ones.