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Interactive Comment

Interactive comment on "The role of blowing snow in the activation of bromine over first-year Antarctic sea ice" *by* R. M. Lieb-Lappen and R. W. Obbard

X. Yang (Referee)
xinyang55@bas.ac.uk

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General comments: The authors' replies have addressed most of my comments appropriately. However, with new material (e.g. meteorology and raw ions data) supplied and revised text in the reply, here I post my further comments (shown below) for authors to consider about.

Specific comments:

1) Bromine depletion: Based on new meteorology data given, it is clear that the blowing snow event on 24 October near McMurdo Station was under a mild-strength wind

and a relatively dry condition with mean relative humidity(RH) of 69%. Under this unsaturated condition, blowing snow particles would sublimate and shrink, especially in aloft layer. We know, during blowing snow, RH near snow surface or within saltation layer is saturated and then reduces with increasing heights. This means there is more water vapour loss at a greater height than at a lower height, indicating an enhanced ion concentration condensation at a higher layer. The new Table 1 dataset clearly matches this prediction. The raw data in Table 1 shows that averaged Cl- concentration at 5.5m (based on all samples in both Icerberg and Butter Point) is ∼10 times of that at 0.3m. This means, assuming a spheric shape of suspended snow particles in blowing snow layer, the particle size (radius) at 5.5m is less than half of that at 0.3m. Although the reduction in particle size does not necessary mean an enhancement of photochemical reaction rate, it does prolong the airborne time of particles and eventually result in more bromine release (this is what Yang et al. (2008)'s mechanism about). Thus, the potential effect of sublimation process on reducing snow particle size as well as on bromine release should be mentioned in the revised version.

2) About salinity: in my first round comment I suggested authors deriving snow salinity from their data, instead of giving snow salinity, they supplied anions concentrations (in response to reviewer #2 request). Obviously, these anions data are quite useful and contain more detailed information, but snow salinity is still a quite useful parameter in many studies in both oceanic and atmospheric chemistry. Here I tried to work out some. Based on dominant anion Cl- concentrations, I derived an averaged blowing snow salinity of ∼0.25 PSU at 0.3m, while at 5.5m, it is ∼2.6 PSU (these data are quite useful for estimating/quantifying saline amount lifted and sea salt production from blowing snow). Thus, I again suggest authors thinking about deriving salinity from your raw data (including surface snow salinity).

3) About Chlorine: In your reply, you said 'the lack of an observed chlorine trend with height ...' (see 4th -5th pages from bottom in reply to reviewer #2). In contrast, I actually saw a clear increase trend of Cl-(and for other ions) with height. As I mentioned

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above, the increase in Cl- is very likely due to loss of water via sublimation processes, but others. Also in the same paragraph, you said ‘Interestingly, lower Br-/Cl- ratios also correspond to a decrease in chlorine release’. Do you want to address the effect of chlorine activation on the Br-/Cl- ratio? I do not feel you have enough data to support this exploration, and moreover in a clean polar regions, the effect of BrCl release (via HOBr uptake) on Cl- should not be important before complete bromide depletion in particles (which is not the case). Thus I suggest re-written of this paragraph.

4) New figure 4 (about DF): 1) it would be useful to include surface snow DF and ice core DF in same DF plot.

5) Definition of DF. I think your explanation of DF is not correct. As I mentioned in the previous comment, DF=1 means complete bromine depletion (all bromide in particle go into air); while DF=0 means no bromine depletion. For a negative DF, it signifies a net bromine uptake by particle, or bromine ‘enrichment’. Note, DF was firstly introduced by Yang et al. 2005 (doi:10.1029/2005JD006244), which is quite different from the ‘bromine enrichment’ factor introduced by Sander et al. (2003) in expressing bromine depletion in aerosols. I noticed that you cite Sander et al. 2003 paper for DF, which was not a proper one. Also, given a formula of DF maybe a good idea to avoid further confusion.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 11985, 2015.

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