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Atmospheric Chemistry and Physics Discussions

Interactive comment on "First-year sea-ice contact predicts bromine monoxide (BrO) levels better than potential frost flower contact" *by* W. R. Simpson et al.

W. R. Simpson et al.

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This comment is helpful with respect to considering where salts involved in halogen activation may be coming from. I certainly agree with the idea that most of the salt is fractionated in sulfate, which is a sign of brine processing. Our data from Barrow also point to this being the dominant salt source, as your work has clearly shown from Antarctica. Unfortunately, Arctic Haze adds sulfate to the Arctic samples at least partially masks the sulfate depletion signature in our samples. However, I would still agree that both the Arctic and Antarctic have sea salt aerosols (and snow) that is mostly sulfate depleted. Whether this depleted salt comes from frost flowers or wicking of depleted brine is less clear. I am certain there is a mix of both processing going on, and thus we separate the processes into two limiting cases – frost flowers and salty

snow. We mean these to be endpoints of a spectrum, and we have added text to the manuscript that expresses this concept.

The calculated PFF occurrence is very small (approximately 0.1%, as the comment notes) in our data, which is largely due to small fraction of open water and relatively warm temperatures. The typical number that is discussed by sea ice scientists is ˜1% of the pack ice is open water leads. If we examine typical temperatures, we find that the PFF parameterization of Kaleschke et al. gives ˜10% PFF coverage at our temperatures, leading to about 0.1% predicted PFF, in agreement with our findings. Only coastal polynyas (large areas of open water) really give much higher PFF values, and they seldom affect our trajectories. This means that PFF is so small that it is unlikely to be correlated to BrO in our data set. However, we do see high levels of BrO in our data, which indicates that Barrow is experiencing significant halogen activation. Therefore, there must be another source of BrO than PFF, and we find that FYI area contact is well correlated with the BrO. We have made extensive comments in the manuscript that this situation may be that of Barrow and not that of other locations.

As to why PFF is so small, it is possible that there were more frost flowers there than the PFF would indicate, and I suspect the temperature dependence of the ice growth curve is maybe to blame. We discuss this in the section "what is it about FYI that leads to halogen activation". In this section, we put forward that maybe FF may be present but undetected by PFF. That is why we also try to always speak about PFF as the calculated form because it might not be so well connected with FF. A last possibility is that the satellite-derived ice amounts are too high. If they are off by a few percent and indicate 100% ice where there is 98% ice, it affects the picture. The satellite ice maps essentially show 100% ice almost this whole period. In any case, the PFF method (which uses the ice maps as described in the manuscript) is not correlating well; maybe it can be improved.

For the paper, we calculate PFF just as described in Kaleschke et al. [2004] using the maximal curve. The Kaleschke et al. [2004] paper shows various integration times in

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which the FF grow, and to get the maximal amount, we use the long-time limit of his curves (the curves are quite similar anyway). That way, we get the most we could. In separate tests, we added up all the FF that would form on each of the five days prior to the airmass going over that area (essentially summing the open water fraction within a pixel over the prior five days). That raised the PFF amount, but didn't improve correlation with BrO, so we retained the originally described method for clarity. We did try a LOT of things to see if we could make something like PFF work, but were frustrated for a long time. It was only when Udo Friess suggested that we should try first year ice that we had success with correlation with BrO. That worked much better than I would have ever expected, and thus the paper.

Because PFF is not actually frost flowers, we have added comments to the abstract, text, and conclusions highlighting that our inference of salty snow being responsible for halogen activation is dependent on the degree to which PFF predicts actual frost flowers.

Overall, I feel that this community is moving towards a better understanding of what really is involved in halogen activation. I also feel that it is critical to understand the real limiters and promoters of halogen activation because they are somehow related to sea ice and sea ice is changing before our eyes. Better algorithms for prediction of frost flowers, along with both field verification of the algorithms and laboratory experiments examining their growth mechanism would be very helpful.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11051, 2006.

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Interactive Comment