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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.

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This paper provides some nice, and much-needed, evidence about the trajectories of airmasses containing BrO arriving at Barrow. I commend the authors for this work, and agree that it is a very useful contribution to the debate about just what type of surfaces lead to activation of Br and to ozone depletion.

I have firstly to say that, despite appearances to the contrary, I have a rather open mind about whether the Br activation takes place on frost flowers (FF) themselves, or only on salty surfaces. Salty surfaces would then include the brine layer on sea ice where FFs do not form, and snow that has become salty as a result of salt transport. Our

Antarctic studies suggest that, at least in Antarctic coastal regions, most of the sea salt aerosol and the deposited snow is depleted in sulfate, and therefore must arise from a cold salty surface: ie not seawater, but brine or FFs. In this sense, all the salty surfaces we are discussing probably arise in a secondary way from the brine on the sea ice surface. Somehow in the past few years, our group has become associated with a view that only primary production of Br on the FFs themselves will do, but most of the time we have intended our use of the term FFs to incorporate these secondary surfaces as well.

The issue then has revolved in my mind over the relative concentrations and surface areas involved for the different surfaces. I should add in passing that although I accept that the FF surface areas measured by Domine et al (2005) were very low, I do not accept that they are typical. We have many photographs of dense FF fields where it is quite obvious that the whole surface of the sea ice is covered by a number of layers of FF "leaves" (and remember that even a single layer implies a relative surface area of 2). We therefore feel that, although our original guesstimates were probably significantly too high, the low estimates of Domine et al are far from typical for areas where FF are mainly observed.

Now, moving to the present observations: in the cases discussed here, the calculated PFF occurrence is extremely low (a few minutes in a few days, implying $<$0.1% coverage even on new sea ice). It is rather obvious that, if there are no FF, they cannot be the source of the BrO. However, this does seem a remarkably low amount of FFs, especially when compared with the high occurrence of new ice. (The implication is that open water and cold temperatures are rarely found together at this location and time of year). In other locations, observers report frequent sighting of FFs on new sea ice, and I am tempted to wonder if we are really learning only that the PFF algorithm is simply too stringent, and that FFs actually form at higher temperatures than the lab experiments used as the basis for the PFF algorithm suggest. We are currently starting some laboratory experiments ourselves, so I cannot say yet whether this might have

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any basis in reality; I only comment that based on ! these calculations, it would be surprising if FFs were ever observed near Barrow at this time of year (perhaps they are not - do you have local observers o confirm this?).

I would seek clarification on one point. I am not really clear how you calculated the PFF coverage. How long did you allow the PFF to form for in order to calculate the PFF coverage as a function of open water percent? You imply that you tested BrO against PFFs with different time lags. However of course in reality, FFs do persist for typically a few days, so the correct calculation is to seek areas where there was open water anytime within the last several days, and allow the FFs to grow for several days. I doubt this would significantly effect the result, but it would be helpful if you clarify whether you did tests where FF were allowed to persist.

In summary, this paper gives the encouraging result that new sea ice, with its brine-laden surfaces, is indeed the likely ingredient for Br activation. On the other hand it clearly also suggests that PFF, as currently defined, is not a good indicator for Br activation. The next step would be to reassess whether PFF really signify FF: do ground truth data really suggest that FFs are so rare in the vicinity of Barrow, and can lab experiments provide a better indicator of FF occurrence?

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

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Interactive Comment