

Interactive comment on “Photochemical chlorine and bromine activation from artificial saline snow” by S. N. Wren et al.

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The authors demonstrate and characterize in the laboratory for the first time a "halogen explosion" mechanism which may be key to understanding photochemistry in the polar boundary layer. The paper is very well written and the experiments are thoroughly done and convincing. While, in my evaluation, the manuscript may be accepted as is, I have a few minor suggestions for improvement:

Figure 5 - The authors may gain deeper insight into the temperature and surface-area dependence of their data by applying a thermodynamic model for predicting brine composition and volume as a function of temperature & total saline content in an ice-brine system (e.g. using Cho et al. 2002 or our model Kuo et al. 2011). I am happy to

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work with them on that.

14180 Line 19-26 Thermodynamic models like the ones described above show that, for nonvolatile solutes, brine *composition* is determined only by temperature. The total salinity (i.e. of the melt) then determines the *volume* of the brine. That is to say, neglecting the presence of other contaminants, the chemical composition of the brine in natural snow will be the same as that of the laboratory system at the same temperature. But the volume will likely be smaller and its microphysical location may differ. The authors may want to reword this passage in light of this.

Editing comments:

14165 line 25 'analogous' is misspelled

14171 line 13 Before this, I had never heard the term "icing sugar" (it's known by a different term in the US). To avoid ambiguity for the reader, perhaps it would be better to say "a fine powder" or relate it to another substance such as talcum powder.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 14163, 2013.