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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A mechanism for biologically-induced iodine emissions from sea-ice" *by* A. Saiz-Lopez and C. S. Boxe

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Saiz-Lopez et al. propose that reactive iodine is produced biologically under the sea ice and diffuses to the surface, where it is emitted into the atmosphere. Although I think that this is an interesting idea, I see several problems with the model study that is used to support the hypothesis:

- Most importantly, I cannot see how an aqueous-phase diffusion coefficient of $D_{aq} = 10^{-2}$ cm^2 s^{-1} can be derived from Eqn. (9). I suggest that the authors explain in more detail how they derive their value of D_{aq}. I would expect that D_{aq} is about three orders of magnitude smaller. If we assume a value of $D_{aq} = 10^{-5}$ cm^2 s^{-1}, would the proposed mechanism still be relevant?

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- For the initial aqueous-phase concentration of chloride, the sea-water value of 0.5 M is used. However, liquid brine in contact with sea ice at cold temperatures should have a several times higher salinity (e.g. Richardson, J. Glaciol. 17, 507-519, 1976). This will affect the aqueous-phase significantly and should be taken into account in the model study.

- Finally, I have a comment about the terminology. The term "QLL" (quasi-liquid layer) is used throughout the paper to refer to the layer on top of the sea ice. As far as I know, the thickness of a QLL can only be several tens of nanometers (e.g. Voss et al., GRL, 10.1029/2004GL022010, 2005). In the present study, however, a QLL of 0.5 mm thickness is assumed (see Fig. 2) which does not seem to be realistic. I think "brine" would be the correct term. If the layer was really a QLL, then it would be quite uncertain if aqueous-phase rate coefficients could be used for the chemistry at all.

I have a couple of additional comments that I can upload later. However, I think that the questions raised here should be discussed first.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2953, 2008.