

***Interactive comment on* “Multiphase modeling of nitrate photochemistry in the quasi-liquid layer (QLL): implications for NO_x release from the Arctic and coastal Antarctic snowpack” by C. S. Boxe and A. Saiz-Lopez**

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Received and published: 28 April 2008

I thank the reviewers and R. Sander for their insightful and constructive comments, and I look forward to seeing their suggestions incorporated into the revised manuscript. I also have a few of my own comments to add, which are intended to help the authors improve the manuscript by strengthening the connection between the model presented here and laboratory studies in the literature characterizing the properties of the QLL and its chemistry.

A) Chemistry of QLL vs. liquid water.

In line 28 of page 6012, the authors state 'As the melting point is approached, the QLL appears to be indistinguishable from the liquid phase in its uppermost layers.' The authors should cite examples of measured physical parameters from the literature for which this is true (e.g. refractive index, conductivity, etc). This clarification is important because the experimental techniques used to study the QLL in laboratory settings vary in sensitivity to surface properties and each intrinsically employs a different definition of disorder. Furthermore, one way in which the QLL is likely *not* identical to liquid water is in its ability to take up trace gases. We have observed that the solubility of HCl in the QLL, rather than being truly liquid-like, is intermediate between that in bulk ice and that in liquid water (McNeill et al., 2006), which reinforces the observations of others that uptake of gases to snow cannot be perfectly described by Henry's law uptake to a liquid water reservoir.(Conklin and Bales, 1993, Couch et al., 2000, Herbert et al., 2006, Hoff et al., 1995, Huthwelker et al., 2001, Roth et al., 2004) Given the overall shortage of physical chemistry data describing the uptake of trace gases to snow and ice at temperatures relevant to the polar snowpack environment, it is understandable that the authors made the assumption that the Henry's law constants for liquid water are applicable to the QLL. However, these issues and the possible errors introduced by the assumption should be addressed in the text.

B) Representation of QLL volume.

The manuscript would be improved if the authors were to add a section discussing their reasoning for using a parameterization that was derived for sea ice/brine systems to calculate the 'liquid water fraction' represented by the QLL in snowpack. As the authors mention in the introduction, abundant laboratory data exists in the literature showing QLL thickness as a function of temperature on water ice. This data could have been parameterized, or an existing model of QLL thickness on water ice could have been used (for references, please see McNeill (2005)). Another significant improvement would result if the authors compared the results of the parameterization they used with

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available data & existing models of QLL thickness on ice as a function of temperature and the presence of impurities.

C) Trace gas-induced disordering of ice.

Page 6013 line 6: The hypotheses you refer to (HCl adsorption on ice surfaces under polar stratospheric conditions induces surface disordering, and this HCL-induced surface disordering is responsible for the high affinity of PSC surfaces for HCl and their role in chlorine activation) were delineated in Molina (1994) - please add this reference. It also is worth mentioning that these hypotheses were confirmed experimentally in McNeill et al. (2006). A mention of trace gas-induced QLL formation would also be appropriate in your discussion of the role of impurities in determining QLL thickness on p. 6012.

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8, S2128–S2131, 2008

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