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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diffusion of volatile organics through porous snow: impact of surface adsorption and grain boundaries" *by* T. Bartels-Rausch et al.

Anonymous Referee #1

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This very nice study, reports the experimental results of gas-phase diffusion of methanol and acetone through the interstitial air of snow. All experiments were carefully performed and analyzed to give insights into the interaction of methanol and acetone with porous ice under different temperatures. Two types of porous ice were used to study the effect of grain boundaries on the diffusion of acetone and methanol through porous snow. The diffusive loss of these volatile organics was measured using a chemical ionization mass spectrometer. The structure of snow samples was characterized using X-ray computed micro-tomography. The results of this paper showed that the

diffusion profiles of these volatile species can be well described based on gas-phase diffusion at temperatures $\geq$ 253 K. At temperature around 223 K, surface interactions start to dominate the diffusive transport. Additionally, results of this study showed that, the uptake of these species into grain boundaries does not play a role on the time scale of diffusion through porous snow. The effect of grain boundaries and liquid layers at the ice surface, on air –ice interactions is a topic of a substantial ongoing debate, and this study adds new evidence to this discussion. The manuscript is well written, and the authors did a great job explaining the goals of this study. I have few specific comments, which might be considered for the final version of this paper:

1) In the abstract the authors stated that the structure of snow was analyzed by means of X-ray computed micro-tomography. Why later in the experimental method section 2.1 a polarized microscope was used and what is the role of diethyl phthalate?

2) Can the authors provide a sentence or two to explain the purpose and why NO was used in their experiments?

3) In section 2.2, the authors mentioned that before entering the snow diffusion chamber, the gas flow was cooled by passing it over an ice surface. How sure are the authors that a portion of their acetone or methanol was not adsorbed on this ice surface prior to entering the snow diffusion chamber? And did the authors accounted for this loss?

4) A number of groups using different experimental techniques have shown that liquid like layers can exist at temperatures as low as 243 K. At a temperature of 263K, a liquid layer has to be present why did the authors kept referring to their ice surface as dry ice? With this layer present, I would guess that the solubility of methanol and acetone will be different, why did the authors explain the fast recovery of methanol at 263 as an artifact by CIMS (p:6144) and not due to presence of the liquid layer (solubility, chemistry, etc).

5) Henry's coefficient can change with temperature; did the authors take this into account when doing their simulation –Langmuir vs. Henry?

6) Section 3.3, p 6146 lines 25 -28, since the authors did not characterize the total grain boundary of the snow-maker snow samples then how valid is the comparison between snow-maker snow and shock frozen snow?

7) In section 3.2 (P: 6145, lines 10-15), the authors stated that the dissolution of these volatile species in the disordered layer on the surface of ice is not an important factor; however, in section 5 (p:6150, lines 6-7) the author surface adsorption dominates as long as water layers are not present. I think the authors here are giving contradictory statements.

8) Since ice phase diagrams for a number of volatile species do not exist, do the authors know if acetone and methanol are in ice regime at 223 K? HCHO for instance is not.

In summary, I would suggest this manuscript for publication in ACP once this manuscript is improved. I hope that my suggestions were helpful.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 6131, 2013.