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

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Received and published: 17 May 2013

We'd like to thank the referee for the kind and constructive evaluation of our manuscript and for suggesting further studies.

It is mentioned in several places that nitric oxide (NO) was used as an inert tracer to test the system response for a gas that does not interact with the snow. However, no data or comparisons are provided for the reader to evaluate how the behavior seen in methanol and acetone compares with NO.

The key difference between the two VOC's and NO is that the diffusivity of NO does

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not slow with decreasing temperatures. Observed profiles are comparable to those of methanol or acetone at 263K. Only in rare cases, a significant loss of NO to the snow sample was observed. We have attributed this to cracks in or other damages of the snow pack sample. And in those cases, the VOC profiles were discarded.

As the set-up and experimental procedure was optimized to analyse the diffusion of trace gases that show a slow diffusivity, the observed diffusion profiles of NO do not give much quantitative information and we thus prefer not to show profiles of NO.

The role of a liquid layer surrounding ice crystals well below the freezing point has been speculated to play a determining role in snow photochemical processes. Secondly, the behavior of this liquid layer and its forcing on the chemistry appears to be to a large extent determined by trace impurities in the snow. This study touches this topic only marginally, which is somewhat regrettable as it diminishes the value of this study for snow photochemical interpretations.

Indeed the role or the quasi-liquid layer (qll) on chemical processes in snow and its response to the presence of impurities is one of the most pressing questions in cryospheric and atmospheric research. In this study we focus on whether or not the presence of the qll justifies or even requires the use of parameters taken from liquid water to describe the interaction of snow with trace gases. This is clearly not the case. We focus on the consequences of the qll on the parameterisation of snow chemistry, because we believe that this is an important finding. To allow conclusion about photochemistry in snow more fundamental study on ice - trace gas interactions would be needed, which require the ability to probe and characterize the ice surface's structure.

We have rewritten and focused the section about surface disorder (or qll).

In particular, it's a petty that experiments were only conducted at three distinct temperatures. It would be highly desirable to have a more continuous and higher resolution record of the behavior of these two soluble gases at a range of sub-freezing conditions, in particular for the range of residual liquid layer conditions

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(i.e. from 0C to -20C).

Indeed, such a study would deliver very interesting and highly relevant data. In this particular set-up, we are not sensitive to the fast diffusivities expected at high temperatures. And, at temperatures approaching the melting point liquid may form due to melting point depression by impurities or by geometric effects at grain boundaries. Studies at higher temperatures would thus require the ability to detect the presence of liquid for a sound interpretation.

For these two reasons we have focused on investigating dry snow. To further illustrate the debate about using Henry or Langmuir partitioning coefficients at such low temperatures, we have added a reference to a recent paper by Neu and Prather, a modelling study that discusses this topic for upper tropospheric conditions.

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

ACPD

13, C2462–C2464, 2013

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