

Interactive comment on “Gas-Chromatography using ice coated fused silica-columns: Study of adsorption of sulfur dioxide on water-ice” by Stefan Langenberg and Ulrich Schurath

Anonymous Referee #3

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The study presents results from solid-gas chromatography of several trace gases to ice. The work covers a T range of 205 to 265 ÅK. From these, conclusions about the partitioning of SO₂ to ice in the upper troposphere and of the role of surface disorder on the partitioning are drawn. The partitioning of SO₂ to ice has raised quite a discussion in the community and is far from being understood. Further, the role of surface disorder is a key-topic. This makes the topic of the manuscript highly relevant for ACP.

I'm not convinced that this manuscript presents new and innovative results and fear the technical content is addressing only a very small and specific part of the atmospheric science community. I also find it difficult to capture the results based on the

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data presented. While I acknowledge the re-analysis and am enthusiastic about process modelling to derive fundamental data from chromatographic results, I'm sorry to suggest rejecting this manuscript.

* The gas-phase concentrations of SO₂ seem too high to me. Extrapolation from experiments at such high concentrations to environmental concentrations – what are typical concentrations of SO₂ in air masses in contact with ice or snow anyway, please specify in introduction – is highly questionable for a number of reasons as shown for a number of trace gases (see later work on HNO₃ by Abbatt group). This lets me question the environmental relevance of this work. * The high concentrations obviously results in very high formal surface coverages, so that SO₂-SO₂ interactions can not be excluded. I don't understand the use of the Henry or Langmuir parameterization in this context – which strictly speaking works best at low coverage. What is the surface coverage at the peak position in your columns? * I don't understand why your surface saturation capacity is so low? To me this looks like there is something odd with the analysis. Could you convince me with the acetone data that your approach is working? * May I ask you to stick to the IUPAC nomenclature. So, your Henry would become K_{LinC}, for example. * Working with SO₂ and in acknowledgement of Huthwelkers work highlighting the role of solvation into liquid pockets, I strongly suggest to discuss the phase diagram. Taken that the freezing point depression by SO₂ is rather modest, I do not expect a large impact but clarification is needed.

Last, the manuscript remains very technical without a clear discussion on what is to be learned from fitting the chromatographic results.

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