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Interactive comment on "The impact of ice uptake of nitric acid on atmospheric chemistry" *by* R. von Kuhlmann and M. G. Lawrence

Anonymous Referee #1

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The authors present results from a 3-D chemical transport model on the importance of the scavenging of nitric acid by ice clouds. While others (including one of the authors) have previously modeled this phenomenon in a less complete manner, this represents the most thorough job on this subject to date. The major results are that the scavenging process may have significant impacts on nitric acid levels in the upper troposphere and effects at the level of a few percent for ozone in that part of the atmosphere. I recommend publication, if the points below are addressed, based on the fact that this represents the best current modeling of the importance of this process. Overall, the authors appear to have addressed in their discussion the major uncertainties in the model and are realistic in their appraisal of the overall conclusions that can be made.

However, I raise a few points. First, perhaps unknown to the authors, there have appeared in the recent literature new laboratory studies on the uptake of nitric acid to ice conducted under partial pressure and temperature conditions of the upper troposphere and under conditions of growing ice (M. Ullerstam et al., Faraday Discuss., 130, 211-226 (2005); M. Ullerstam, J. P. D. Abbatt, Phys. Chem. Chem. Phys., 7, 3596-3600 (2005)). It is unrealistic that this new information can be fully incorporated into this present paper but perhaps the authors could at least address how the new laboratory studies fit into the range of model runs that constitute this work.

Second, considerable effort in the paper is given to comparison of model results to measurements, and the results are somewhat inconclusive as to whether a model with or without ice scavenging does a better job. Would a more "process-oriented" analysis have been better able to assess the degree of agreement achieved? For example, is better agreement made when the temperatures are low and/or when the ice surface areas are high? Both lab and field measurements agree that uptake is more substantial under these conditions.

Lastly, there are disagreements in the laboratory community on the value of the initial uptake coefficient for nitric acid uptake by ice. At low temperatures, perhaps close to 200 K, the uptake coefficients are all measured to be large, at least bigger than 0.1. However, at higher temperatures (say 220 to 230 K) the initial uptake coefficients are measured by some groups to be quite a bit lower than 0.1, whereas Ullerstam et al. (referenced above) claims the uptake coefficient is large at this temperature. Could the authors comment on how a kinetic limitation to the uptake of nitric acid would affect their results, in contrast to the "instantaneous" uptake model used in this study?

Small comments: 1. Clarification of the final sentence on page 7364 would be useful. At what altitudes is HNO3 underestimated? Under what seasons, etc. 2. Clarification of the statement on page 7366 would be useful: "is a more sophisticated treatment of gas-aerosol interactions Ĕ". I would hope this study is at least as good as the Liao et al. study, at least with respect to HNO3 interacting with ice.

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 7361, 2005.