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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of the ice growth rate on the incorporation of gaseous HCl" *by* F. Domine and C. Rauzy

F. Domine and C. Rauzy

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Response to the comments of referee 3

Referee's comment: Xkin should be proportional to 1/PH2O.

Our response: This is indeed true. In the atmosphere, however, this will not be noticeable, as saturations only reach a few percent. In the revised version, we have added a short paragraph below equation (1) to mention this. This comment becomes important for some of our experimental conditions. In particular, at 82.5 % saturation, it should definitely be accounted for, and we had omitted to do that. Thus, the value we derive in our results section for $\gamma_{HCl}/\gamma_{H2O}$ is 0.0028 and not 0.0014 as we had initially written. We thank the reviewer for spotting this error. It does not, however, change any of our conclusions.

Referee's comment: It is difficult to derive a trend from 3 points. Our response: We

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could not agree more. And actually many analytical expressions could be fitted through those points. This is why we have to analyze the data in the light of a model, which here is eq. (3). Our conclusion is that our data are compatible with this equation, but as in many other studies, other analytical expressions could be proposed to describe fig. 2. However, why would we try to find a linear dependence here ? The points are not aligned, and moreover, what physical process or equation would that correspond to ? We believe that there is little meaning to a linear equation, that would predict negative HCl concentrations at very fast growth rates, and unrealistically high values at very slow growth rates.

Referee's comment: There is a problem with the calculation of $\gamma_{HCl}/\gamma_{H2O}$, as it should depend on the supersaturation. Our response: There is a significant misunderstanding here. Deriving $\gamma_{HCl}/\gamma_{H2O}$ using eq. (1) implies that we are in the conditions of validity of that equation, i.e. at very fast growth rates. According to Fig. 2 and eq. (3), at 15 and 40 % supersaturation, we are not under the purely kinetic regime and eq. (1) then does not apply. We cannot therefore apply eq. (1) to those data. To avoid such misunderstanding, we have reworded paragraph 3 of the results section. Referee 3 also mentions that more experiments would strengthen our conclusions. We do agree with that, but as mentioned in our response to referee 1, this project was funded for a limited amount of time and is now terminated.

Referee's comment: The experimental section needs more detail. Our response: This has been addressed in the response to referee 1. The upstream bubbler was kept in an ice bath in a dewar to maintain its temperature constant. The water content was not actually measured, but was calculated "from the data of Marti and Mauersberger (1993) and from the temperature of the stirred ethanol bath, regulated within 0.01°C". There was no ice forming on the glass sphere. Since our ethanol bath was stirred and regulated within 0.01°C, this should address the concern of the referee regarding lateral temperature gradients on the inner surface of the crystallization tube. We have never been able to detect any gradient in the bath. Analyzing ice from different parts

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of the tube was not done, and would be difficult, if only because it would significantly reduce the amount of ice available for analysis. Blank values have been added at the end of the experimental section: 5 ppb, with a range of 2 to 9.

Minor points: Which numerous tests and blanks ? This is standard in any trace analysis and each step of the protocol is tested independently to evaluate its contribution to contamination. Since, as stated in the response to referee 1, we wish this paper to remain short, we do not feel that these standard details would be of crucial interest and have chosen not to lengthen the paper with them.

γ_{HCl} would be affected by the same variables:...Referee 1 made a similar comment which has been addressed in the revised version. Other minor comments have been taken care of in the text of the revised version. We thank the referee for his time and his useful comments.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 4719, 2004.