

Interactive comment on “FTIR spectroscopic studies of the simultaneous condensation of HCl and H₂O at 190 K - atmospheric applications” by I. Xueref and F. Dominé

Anonymous Referee #1

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FTIR spectroscopic studies of the simultaneous condensation of HCl and H₂O at 190 K - atmospheric applications Authors: I. Xueref and F. Dominé

General comments

The paper presents a FTIR study of co-condensed HCl and H₂O at temperatures relevant of the stratosphere that is 190 K. The authors claim that their results give a strong evidence of HCl ionisation when incorporated into ice and form chloride and hydrated hydronium ions and that HCl strongly perturbs the structure of ice. The paper is well written and the subject matter is of great interest for the atmospheric chemistry com-

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munity. The results are of interest and their interpretation appears reasonably correct. However, at least two relevant references are missing which should be put in the paper, first in the introduction and second in the discussion of the results. The paper should be published after the authors take into account the comments below.

Specific comments

Indeed, although it is not a co-condensation experiment, an experimental and theoretical study of HCl adsorption on an ice film has been published which is relevant of the work presented by Xueref and Dominé. The temperature range is from 190 to 270 K and HCl/H₂O ratios of 1/5 and 1/10. Di-hydrate and amorphous ice have been observed in agreement with the authors results. I strongly recommend the authors to discuss their results taking into account these published works whose references are : B. Demirdjian, D. Ferry, J. Suzanne, C. Toubin, S. Picaud, P.N.M. Hoang and C. Girardet, J. Chem. Phys. 116 (2002) 5143 C. Toubin, S. Picaud, P.N.M. Hoang, C. Girardet, B. Demirdjian, D. Ferry and J. Suzanne, Chem. Phys. 116 (2002) 5150

For instance, Demirdjian et als. observe the HCl dihydrate at T= 220K and HCl/H₂O about 1/5 and amorphization of ice at 250K but no dihydrate is observed at 190 K.

Besides, the authors should be careful when they claim that their results show that HCl is dissociated ionically (page 4050, line 17). Although it agrees with model calculations of Gertner and Hynes and other experimental results, I think they can just say that their results are consistent with a ionization of HCl. It is one possible interpretation (or suggestion) like they write it page 4046, line 22 and 28.

Page 4052, line 27: how is the HCl:H₂O mixing ratio 1/2500 determined. The authors should explain or quote a reference paper. Is it an experimental measurement? I guess that besides co-condensation of HCl and water in the stratosphere, HCl may be adsorbed on the ice surface from the gas phase at 190K during the lifetime of an ice crystal and thus increase the amount of HCl, at least at the ice surface. What would be the impact of this HCl adsorption compare to HCl in bulk ice? Does it modify the HCl

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bulk concentration?

Page 4053, line 13-16: "Furthermore, type Ia...of crystalline solids:..". The notation is misleading since Ia means usually amorphous ice, how can it be a crystalline solid. I suggest the authors to be more clear about what they mean.

They claim in the discussion that "Crystallization is almost complete in the 5:1 mixture..." page 4051, line 7. Indeed they observe an IR spectra assigned to the dihydrate. Why do they write in the same paragraph that "In any case, no ... HCl hydrates has been detected...". This looks like a contradiction. They should write this paragraph differently to be more coherent. Also, in the abstract they write that "...HCl and H2O do not form crystalline HCl hydrates,..." which is rather overstated owing to the experimental results as said above.

Technical corrections

1) Quote the reference "Gertner and Hynes, 1996" in the introduction page 4040, line 20, after "...HCl is expected to be ionically dissociated..." 2) Page 4047, line 22 "...formation of a solid close whose IR...", remove "close"

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