Atmos. Chem. Phys. Discuss., 3, S1626–S1629, 2003
www.atmos-chem-phys.org/acpd/3/S1626/

Atmospheric
Chemistry
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Discussions

Interactive comment on “FTIR spectroscopic studies of the simultaneous condensation of HCl and H_2O at 190 K - atmospheric applications” *by* I. Xueref and F. Dominé

I. Xueref and F. Dominé

Received and published: 1 October 2003

Reply to our referees (from I. Xueref) —————————————————————-

Dear Referees,

first of all, we would like to thank you for the interesting and constructive comments you provided us. We took those into account to produce a revised version of our manuscript, which we hope will be appreciated by the community.

Furthermore, we would also like to answer you about some specific points :

-in both reports, we have been recommended to quote the references Demirdjian et al.(2002) and Toubin et al.(2002), and so did we in the Introduction of our revised version. We would also like to emit some more comments about these studies. Demirdjian et al. (2002) observe little perturbation of the ice structure at 190 K . However, at 250 K, they mention that "The ice/HCl film probably becomes an amorphous solid phase, which coexists with a small amount of liquid phase". It thus appears that, besides an expected surface perturbation, the presence of HCl does perturb bulk ice structure under their experimental conditions. This observation is consistent with our own observation of the perturbation of ice structure by HCl at 190K. Demirdjian et al (2002) did not observe any perturbation at 190 K, while we did. This can be explained by the different conditions used in both studies. They exposed an ice film to HCl vapor, and solid state diffusion was then necessary, in their case, to produce perturbation. Diffusion is a thermally activated process and is thus much slower at 190 K than at 250 K, and it is reasonable to suggest that this process could have taken place at 250 K and not at 190 K. In our case, we co-condensed HCl and H2O, so that HCl was present throughout the solid and no solid state diffusion was necessary for the perturbating effect of HCl to manifest itself. In the experiments of Livingston et al. (2000), exposing ice to HCl at 190 K resulted in a perturbation of bulk ice structure, as we argued earlier (Dominé and Xueref, 2001). This may be because Livingston et al. (2000) used higher HCl concentrations, that resulted in extensive perturbation of the ice surface, that could then propagate in the ice bulk. However, at this point, we are not capable of explaining quantitatively the effect of HCl concentration on the perturbation of ice structure at 190 K. Toubin et al. (2002) perform some molecular dynamics calculations of the effect of HCl on ice structure. However, a fundamental hypothesis of their work is that HCl remains in teh molecular form, while it is now widely accepted that HCl is dissociated into ions on ice at 190 K (Horn et al., 1992 ; Delzeit et al., 1993). It is thus not clear to us how their conclusions can be applied to our system.

- We now answer the following question from our first referee : "What would be the impact of HCl adsorption on ice surface comparing to the presence of HCl in bulk ice? Does it modify the HCl bulk concentration?". Although this is true that HCl can be adsorbed on ice, the amount adsorbed is negligible in comparison to the one present within our samples. Furthermore, Demirdjian et al. (2002) do not report any major modification of the ice surface due to adsorption of one monolayer of HCl at 190 K, so that HCl adsorption on PSCs particles should not be a major process comparing to bulk incorporation.

- Our second referee voiced reservations about the fact that we associated automatically an absolute crystalline connotation to any solid called "hydrate". Indeed, we think this is a question of definition, and we therefore precise more explicitly in our revised manuscript that to make it simple, we decided to call "hydrates" only pure crystalline compounds, and to use the term "non-crystalline" solids for compounds those structure reveals a disorganized feature.

Thank you again for your comments.

Best regards,

Irène Xueref

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