

## ***Interactive comment on “Airborne measurements of the nitric acid partitioning in persistent contrails” by D. Schäuble et al.***

**Anonymous Referee #2**

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The paper presents a compilation of observations from a number of aircraft missions that show the enhanced presence of nitric acid in contrail cirrus particles compared with background cirrus ice particles. The results from the CIRRUS III project alone represent a valuable dataset. The paper presents the results and demonstrates that the HNO<sub>3</sub> in ice is enhanced in contrails compared with background cirrus and a greater fraction of the HNO<sub>3</sub> present is in the ice phase in the contrails. The data are a significant resource but as they arise from only 3 flights during which the contrails were sampled on an ad hoc basis the authors rightly caveat the presentation of the data as not necessarily being representative of HNO<sub>3</sub> in contrails in general. However, they go on to investigate how the ratio of HNO<sub>3</sub> in ice to the water content of the particles varies as a function of age and use a model of the HNO<sub>3</sub> uptake onto ice to explain their findings. Their hypothesis is that a large amount of HNO<sub>3</sub> is incorporated into the

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particulate phase at the point of formation in the near field plume and this subsequently forms the ice particles in the contrail. As the contrail develops further is added, diluting the initial HNO<sub>3</sub> and only slow addition of further HNO<sub>3</sub> occurs from entrainment from the surrounding air. This modelled behaviour can explain the measurements within their uncertainty and is carefully discussed in the light of previous work. The paper is well presented, the methodology is sound and the results are clear. I certainly recommend that the paper is published in ACP. I have some comments on what I believe is a very good paper. 1) I would like to see a summary describing the model of HNO<sub>3</sub> onto ice that was used in the paper. Whilst I recognise that the model is discussed elsewhere, it would help the reader if a brief description was given here. 2) Similarly, though following a referenced method for calculating contrail age there is no description and a short summary should be included.

In addition, I have some minor comments. Page 14168 line 12: unintentionally

Page 14168 line 14: “Contrail ages were up to about eight hours estimated. . .” should read “Contrail ages of up to about eight hours were estimated. . . .”

Page 14168 line 20: It may be worth stating how the calibration of NO<sub>y</sub> was performed or provide a reference for such a description.

Page 14169 line 9: “dependent relationship EF. . .” should read “dependent relationship for EF. . .”

Page 14170 line 16-18: “The OJSTER instrument was adjusted to the FISH instrument”. How large was this adjustment and what is the estimated uncertainty of this correction?

Page 14171 lines 7-9: This sentence is very hard to read. I suggest: “. . . and decrease within a few seconds. Lower limits of 0.1 nmol/mol for NO<sub>y</sub> and 100 cm<sup>-3</sup> for CN were used as thresholds in this identification and correspond to a contrail age of. . .”

Page 14171 line 27: “. . .our data set does not allow (us) to compare..”

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Page 14172 line 1: The first sentence doesn't say anything, I suggest removing it.

Page 14172 line 2: (and elsewhere. Several times the authors refer to the top regions of frontal cirrus, it may be better to refer the upper regions rather than the top.

Page 14172 lines 23-24: The references to the red circles when talking about the averages is incorrect and the phrase "compare black and red squares" is difficult to follow. I suggest removing this phrase and referring directly to the colour of both black and red squares in parentheses where appropriate in the sentence.

Page 14174 line 12: "..and a reduced IWC (arises) while crossing..."

Page 14175 line 17: How robust is the relationship between the estimates of contrail age and ice particle diameter. This is not discussed in the paper.

Page 14176 line 1: "...would not significantly (increase)..."

Page 14176 lines 2 and 3: This sentence is very hard to read. I suggest: Hence, trapping of the low levels of entrained ambient HNO<sub>3</sub> by the growing contrail ice particles is not capable...

Page 14176 line 6: "...of HNO<sub>3</sub> (had already) entered the ice particles during contrail formation"

Page 14177 lines 7-10: The description of the model curves in figure 4 is not well described. I assume that the solid curve in the figure represents the full expression in equation (1). However, this is not made clear in the text and it should be at this point.

Page 14177 lines 21-23: Do you have any other evidence to support this hypothesis? They appear to occur at low IWC values (figure 2).

Page 14184: Figure 1: It appears that there is a difference between the gas phase NO<sub>y</sub> and the total NO<sub>y</sub> channels out of cloud in the time period 43500 to 45000 UTC (seconds). If so, this may represent a significant difference to several of the small plumes that were observed during the period.

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Page 14184: Figure 1 is rather small and could be enlarged in the vertical to make the figure clearer.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14165, 2009.

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