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Atmospheric Chemistry and Physics Discussions

Interactive comment on "In-situ observations of young contrails – overview and selected results from the CONCERT campaign" *by* C. Voigt et al.

Anonymous Referee #1

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This paper provides an interesting overview of results from the CONCERT campaign, with a focus on the microphysical properties and chemical processing in young contrails. The paper is well written and interesting; it provides a useful contribution to our understanding of contrails and their climatic effects. I believe the paper will ultimately be acceptable for publication in ACP, however, I have several comments that I think the authors should consider.

Major Comments

1. The discussion of microphysics instrumentation and measurements is not as clear or complete as it should be. The only description of the 2DC is line 3 on page 12723. More information should be provided. From Figure 9, it is apparent that the authors decided not to present 2DC data in channels smaller than 100 μm. This choice is

reasonable given the large uncertainty in sample volume for the smaller channels; however, some discussion of this issue should be provided. The lack of information about ice crystals in the 17-100 μm size range is unfortunate since it seems clear from Figure 9 that crystals larger than 17 μm likely exist even in young contrails and can contribute to area and mass.

The CPI and CIP are described in Section 2, but the only data shown from these instruments is the comparison shown in Fig. 13 and discussed in section 9.1. Since data from these instruments is otherwise not used in the paper, I suggest that section 9.1 and the discussion of the instruments should be removed.

A number of recent papers have suggested that ice crystal shattering on instrument inlets and probe tips can results in artifacts that overwhelm the natural concentration of small crystals in the observations [e.g., McFarquhar et al., 2007; Jensen et al., 2009]. This issue is apparently what is being referred to on lines 15-17 of page 12733. The authors state that Fig. 9 shows that "inlet effects from the FSSP do not significantly disturb the cirrus and contrail measurements." I would agree that Fig. 9 indicates that the small-crystal concentration in the contrail is not strongly affected by shattering, but there's every possibility that the small crystals indicated by FSSP in the cirrus are strongly affected by shattering artifacts.

2. I believe that the authors should include comparisons with previous measurements of young contrails. In particular, the 1996 SUCCESS experiment included extensive young contrail measurements. The 1998 GRL special issue included a number of papers reporting on the SUCCESS contrail microphysics measurements [e.g., Goodman et al., 1998; Jensen et al., 1998; Heymsfield et al., 1998; Lawson et al., 1998; Kuhn et al., 1998; Baumgardner and Gandrud, 1998]. Comparisons between the contrail measurements reported in these papers and the CONCERT results should be included.

Minor Comments

1. Page 12716, lines 25-26: "...ice crystals can grow by condensation of entrained

water vapor and aggregation of aerosol". I do not believe that aggregation of aerosols contributes significantly to ice crystal growth. Perhaps the authors meant aggregation of ice crystals?

2. What is the particle size cutoff for the rear-facing NO_y inlet?

3. Fig. 3 shows that the mean RHI in contrails and cirrus is $\simeq$80–90%, whereas one would expect it to be very near 100% (particularly in young contrails with numerous ice crystals). This suggests to me that there may be a low bias in the water vapor measurement (or a high bias in the temperature measurement). Perhaps some discussion of this issue could be included. Related to this issue, on page 12727, lines 1-2, the authors state that the cloud-top sampling strategy may explain why the maximum in the RHI distribution is found to be below 100%. I don't understand why this is the case.

4. The gray bars on Figs. 4 and 5 are very difficult to see.

5. Addition of an RHI scale on Figs. 4, 5, and 8 would be very helpful.

6. Would it be possible to include a statistical comparison between CoCIP results and the observations for the entire campaign? This would provide a much clearer indication of the performance of the model.

7. I'm puzzled by the units on Figs. 9 and 13. Are these distributions normalized by bin width, or perhaps by log(bin width)? What is dm^{-3}? If it is (bin width)$^{-3}$ then apparently the distributions are not normalized by bin width. Some clarification would be helpful.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 12713, 2010.