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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 #2

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Review of "In-situ Observations of young contrails – Overview and selected results from the CONCERT campaign" by Voigt et al.

This paper contains an interesting description and discussion of contrails that were sampled over Germany during the recent CONCERT campaign. As there are few such detailed reports of the chemical and microphysical properties of contrails, I certainly believe that this manuscript should be published. However, I have a few suggestions that the authors should consider before drafting their final version of the manuscript.

1. The paper needs to be very carefully proofread. There are several places where there is a mismatch between singular nouns and plural verbs (or vice versa), or where the tense is incorrect.

2. Both the abstract and especially the conclusions need to be rewritten to highlight what are the major scientific findings of this study. Right now, the abstract and conclusions (and indeed much of the paper) read as a simple description of what was observed during CONCERT without placing the results in the proper context of what has been previously known about contrails. There needs to be a stronger scientific focus to the paper so that the paper is concentrated on showing how the data collected during CONCERT are being used to make certain scientific conclusions. Right now, the paper suffers from a lack of focus, concentrating on a description of CONCERT rather than a succinctly stated description of scientific findings that have been made with CONCERT data.

3. Right now there is some material that should not be included in the paper because it does not contribute to the scientific findings of the study. For example, section 9 is particularly lacking and needs to be much better developed, or preferably can be expanded into a separate manuscript on its own. Detection of cirrus clouds on its own is not an important scientific findingâĂŤhowever, if data collected in cirrus can be used to tell us a new conclusion about cirrus properties or formation mechanisms, then this should be included. Right now that is not the case with Section 9 (and a few other sections in the manuscript).

4. The microphysical observations in cirrus need to be much better described. In particular, what are the uncertainties associated with the derived concentrations, ice water contents and size distributions. Do these uncertainties allow the authors to make the conclusions that they do in the manuscript? For example, the authors claim in section 9.1 that the CIP and CPI show reasonable agreement in Figure 13. What is reasonable agreement? To me, it appears that the size distributions differ by almost an order of magnitude! How well do the size distributions and other quantities need to be known in order to evaluate the model? What is the definition of a similar result or how well do the modeled observed quantities need to be in agreement for the model results to be considered validated, and the conclusion that there are no indications of

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additional ice formation beyond soot induced ice particles to be verified. Right now, the comparisons are far too hand wavy to justify these conclusions, especially given the incredible amount of scatter in Figure 7.

5. Related to point 4, is the potential impact of shattering of large ice crystals on the inlets of microphysical probes in artificially amplifying the concentrations of small ice crystals. This is a very important issue, especially considering that observed concentrations down to 1 micrometer are being compared against model concentrations. This issue and how the microphysical probe data are analyzed needs to be described. Further, what is the justification for using the CPI to derive concentrations, especially given the poor agreement with the CIP that is shown in Figure 13. The CPI does not have a well defined sample volume so it would seem that there is large uncertainty in concentration, even if the size distributions are integrated over large time periods to compensate for the poor sampling statistics. Is the CPI concentrations affected by shattering? Similarly, work by Alexei Korolev has shown that the CIP concentrations may be affected by shattering for particles as large as 400 micrometers. How are these issues dealt with?

Additional comments:

FSSP300 description: How significant was the shattering problem on this probe? Was there a shroud used on this instrument? Given past studies showing the magnitude of the problem of shattering artificially amplifying small crystal concentrations, can this probe be trusted?

2DC, PN and CPI: description: More description of the data processing is needed, especially given that the derived products are being used to evaluate the performance of a model. Also, past studies have shown that the 2DC has a poorly defined depth of field for particles smaller than 125 micrometers, making concentrations of particles in these size ranges highly uncertain. In addition, as stated above, there may be shattering problems associated with the 2DC. Also, how are the CPI data processed given the

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poorly defined sample volume?

CIP: See above comment about uncertainties in depth of field for particles smaller than 125 micrometers, and also, how is shattering compensated for?

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