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# *Interactive comment on* "Long-lived contrails and convective cirrus above the tropical tropopause" *by* Ulrich Schumann et al.

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Response to Referee #2 (Darrel Baumgardner)

We thank the Referee for his comments. The comments help us to strengthen the paper.

We repeat the comments after "C:" and add replies after R:

C: The material presented in this manuscript represents a very detailed case study of the cirrus clouds above the Hector convective system. Technically all the "T"s are crossed and the "I"s dotted. A very clever methodology is employed to ascertain the location and movement of the contrail(s) that were created by the Geophysica aircraft. The manuscript is rather lengthy and perhaps more of the technical details could be



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Discussion paper

moved to the Appendix or supplementary material than is already there. If, however, the authors feel that these details need to remain in the main text, that is their prerogative.

R: We considered shortening and moving parts of the material into the supplement. However, we prefer to keep the basic structure for reasons given below.

C: I only make this suggestion as it took me, as a reviewer a number of readings to make it through the main text and glean what I think are meant to be the primary results.

R: We agree that the material is demanding and requires careful reading. We now describe the strategy of the paper's roadmap in the Introduction. This paper separates the description of the results from the discussion. The individual results can be understood adequately only after the complete (and still limited) set of facts has been described. Our picture on the convective cirrus and contrail situation is the result of a quasi-forensic investigation of many details, and it seems unavoidable that the total picture can be fully grasped only after a second or even third read.

C: This brings me to my primary concern and suggestion. From the abstract and introduction it seems that the primary objectives of this study are to: 1) present a methodology that will be used to extract contrail evidence from the obscuring natural cirrus, 2) demonstrate this technique with a case study of contrails near or mixed with cirrus, 3) corroborate the results with in situ and remote sensing measurements. From my perspective as the reviewer, these three phases are not delineated clearly enough from the beginning.

R: Obviously, we did not make clear enough that this paper deals two topics. As the title says "Long-lived Contrails AND convective cirrus". We now try to make this clearer in the changed abstract and added a roadmap at the end of the Introduction. See reply to Referee 1.

C: Hence, my strong recommendation is that the final section of the introduction should be amended to include a road map that clearly describes the objectives of the study and a step by step elucidation of how the authors plan to achieve those objectives.

R: Thank you. We follow this suggestion. See reply to Referee 1

C: My second suggestion, perhaps less strong, is that the sections that discuss the lidar, radar and satellite measurements with respect to the contrails should be shortened as none of the remote sensing results make a strong case for the presence of the contrails. The text and figures in Sections 3.2.2-3.2.5 occupy a fairly large fraction of the paper without seeming to be tied that strongly to the contrails themselves other than demonstrating that there were a lot of cirrus accompanying the contrails.

R: This should now be clearer. We are not only discussing contrail properties; we are also discussing convective cirrus (see Title).

C: My final point is that in the section on in situ measurement methodology, more needs to be said about the potential for contamination from ice crystal shattering on the FSSP and CIP and the uncertainty due to the very small sample areas of the two instruments. A great deal has been discussed in the references that are cited, but at least a paragraph is needed to explain why it is likely that shattering is not an issue here and that the low concentrations measured by the FSSP are at the measurement threshold of this instrument. For example, A concentration of 0.01 cm-3 shown from the FSSP represents a single particle in the one second measurement interval, assuming that the aircraft is flying at 150 ms-1. The uncertainties and limitations do not change the results or conclusions but they do underscore the difficulty of extracting information from measurements like these.

R: We agree: We cite Frey et al. (2011, ACP, doi:10.5194/acp-11-5569-2011, 2011, reference now added to the paper): A "widely discussed problem for in-situ ice particle measurements is the shattering of ice crystals on the probe’s arm tips and shrouds or inlets (e.g. Field et al., 2006; Lawson et al., 2008; Jensen et al., 2009; Korolev et al., 2011; Lawson, 2011).“

Shattering is a minor issue in these measurements above the tropopause because of the relatively small particle sizes and the low IWC at the low temperatures. The data show no indication for an overestimate of particle concentrations.

We refer to de Reus et al. (2009), Figures 4 and 11, and the extensive discussion in Frey et al. (2011), including its supplement. The AMMA and Hector clouds are similar in respect to ice microphysics. The data sets have been carefully screened to identify and filter out potential shattering events. The comparison between in-situ and MAS backscatter signals in the paper by Cairo et al. shows that shattering cannot have played a major role, because shattering should have become obvious from differences in MAS and FSSP data. These conclusion for the previous studies was that shattering effects for cloud IWC smaller than 0.1 mg m-3 should be small (de Reus et al., 2009; Cairo et al., 2011).

We agree with Darrel Baumgardner, that the small sample areas of the two instruments affect the resolution and accuracy of the ice particle measurements. The uncertainty of the small sample areas; has been considered in the careful error analyses of the corresponding publications and the underlying Ph.D. theses. The reviewer knows these studies as reviewer or co-author. As we mention in our paper, also we see problems in comparing IWC from local measurements with small sample cross-sections with the far more integral results from satellite footprints, radar returns and model grid points results.

Hence, we now write:

The sampling volume of the FSSP100 limits the detectability of ice particles with sizes > 2.7 ïĄ■m to concentrations > 0.003 cm-3 for 1 Hz data (de Reus et al., 2009). Shattering aspects for the FSSP/CIP instruments are of lower importance for this study because of low temperatures, low ice water content, and low fraction of large ice particles (de Reus et al., 2009; Frey et al., 2011; Cairo, et al., 2011).

C: I have attached a copy of the manuscript that I have annotated is addition comments that the authors can address and recommendations that they can implement should they so choose.

Essential comments in the text

This Abstract seems overly descriptive and more like a summary and conclusions. Recommend shortening substantially with only a brief description of the objectives, the methodology used to address the objectives and the significant results.

R: The abstract has been restructured to make the objectives clearer. See reply to Referee 1.

C: High-flying aircraft : There is nothing in principle incorrect about this term, but in the aircraft research community I think it is more common to use the term "High altitude". Just a suggestion.

R: “High-flying” was used in previous papers, e.g., by Peter et al (GRL, 1991). Nevertheless, we follow your suggestions and changed the text accordingly.

C: A one or two sentence explanation of what a self-match experiment is would be helpful. I have never heard this term used and I am fairly knowledgeable of airborne research.

R: We add an explanation: self-match experiment, characterizing the change in composition of an air mass between two measurements.

C: It would be helpful to label with a number or letter where the various contrails are as I see two maybe three possible contrail signatures.

R: This is now done. We add labels U1, U2 and L1, L2 for upper and lower contrail parts computed without and with wake descent.

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