

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

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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 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. 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.

This brings me to my primary concern and suggestion. From the abstract and intro-

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duction, 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.

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.

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.

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.

I have attached a copy of the manuscript that I have annotated is addition comments

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that the authors can address and recommendations that they can implement should they so choose.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/acp-2016-940/acp-2016-940-RC2-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-940, 2016.

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