

## ***Interactive comment on “Aircraft type influence on contrail properties” by P. Jeßberger et al.***

### **Anonymous Referee #3**

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This paper presents important and novel measurements and analysis of the properties of contrails, and their dependence on aircraft type. It clearly should be published. However, in my opinion the paper requires considerable revision to clarify some important issues before it can be accepted.

#### **Major issues**

1. This paper needs some caveats. The observations are, of course, very important, but the whole structure of the paper is based on around 20 minutes of measurements, with one set of measurements per aircraft type. The assumption (supported by model simulations, although the evidence is that the models do not do a very good job in several respects) is that the general conclusions and dependences are almost universally valid. But, if one were to measure an A319 and A380 on another day in slightly different conditions, could we be sure that the conclusions derived here would be valid

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(both qualitatively and quantitatively)? I really just ask for a “normal” level of scientific caution, given that it is hard to get any sense of the reproducibility, variability and representativity, over a wide range of conditions, from the presented results. A few sentences of caution within the conclusions may suffice.

2. Similarly, I was concerned that the properties of rather young contrails are being extrapolated to the properties of the more climatically important persistent contrails. Again, I do not want to devalue the measurements – I just want to see more scientific caution expressed when such extrapolations are made.

3. On a more substantive issue, I have serious concerns with the way the contrail vertical extension (I will use “depth” here) is handled. I am not equipped to understand why the P2P model is assumed to be superior to the EULAG-LCM model (and insufficient information is given to make any assessment) but the factor of 3 or so difference in the derived depths is disturbing. I did not appreciate that there would be so much uncertainty in this parameter in the CoCIP model, and indeed that it might be incorrect by a factor of 3 (can this really be so?). Because this parameter directly impacts the optical depth, and hence the climate impact of contrails, it is a very serious issue and needs a much more thorough treatment here. The authors need to spell out why they believe P2P is superior and they need to elaborate on the vague statements in 13927:1-3 that the observations support the P2P values. I also believe that Table 2 should be amended to make clear that the values of depth and optical depth in the table are model-derived, rather than observed. It seems that the young contrail depth observations reported by Sassen and Hsueh (GRL 1998) are more consistent with the lower values from CoCIP and EULAG-LCM although I understand this comparison is very loose.

#### Other comments

13916:11 (and elsewhere) Optical depth is a meaningless quantity in itself, unless the wavelength is specified. In passing (at 13926:11) it seems that 550 nm is assumed

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throughout the paper, but this needs to be stated clearly when optical depth is presented.

13916:17-19 Since the observations were for a few minutes, it needs to be made clear that this conclusion is based solely on the model results. Given that those models apparently seriously underestimate the actual and optical depth of the contrail, it is not clear how reliable they would be for such a statement to be made, nor is it apparent that the conclusions here (for probably non-persistent contrails) can be carried over to persistent contrails, which are of more interest for climate.

1396:22 RHI is not yet defined

13924:13-20 The issue of whether the environment is or is not supersaturated at the time of the measurements is quite a serious recurring issue in this paper, and I nearly promoted this comment to being a major one. In this section of the text, it seems that ECWMF analysis data is being given equal value to the direct and detailed observations from the Falcon, in justifying the statement that the air might indeed be supersaturated. This is somewhat surprising. Could more be said? Were these observed contrails indeed persistent? Were other persistent contrails observed in the vicinity of the flights (they are not immediately obvious on the (literally) snapshots shown in Figure 1)? Do nearby radiosonde ascents provide evidence of a deep layer of supersaturation?

13927:55 and Table 6: Could the authors check the A340 optical depth calculation? From the information provided I get 0.64 not 0.55.

13927:23-25 Is this supersaturation due to the ambient air (see above) or due to the dynamical processes in the vortex from the aircraft? (See also 13930:20).

13931: Table 4 (and Table 5) Could these tables be restructured so the reader can easily distinguish between inputs TO the model and outputs FROM the model? Maybe just a horizontal line below "NO<sub>y</sub> mixing ratio" would do the job.

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13932:25-27 First, the values in the text for the extinction do not seem to agree with the values in the table. Second, “good agreement” on line 27 seems too optimistic. The difference between the model and observations seem to exceed one standard deviation (if I interpret the uncertainties in Table 2 correctly). So, the models appear, on the evidence presented, to seriously underestimate both the extinction and the contrail depth and both contribute to the serious underestimate of the optical depth (although I still find it difficult to understand how the models could be so systematically incorrect). This seems to be one of the most important conclusions of this study and yet it is not represented in the abstract and is rather underplayed in the conclusions.

13941:29 and 13942:15 These sections seem to be a bit repetitive

13942:6 “observed” – since the optical depth is not directly observed, but is heavily reliant on a model for the contrail depth, I suggest avoiding the term “observed” here.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 13915, 2013.

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