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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Aircraft type influence on contrail properties" *by* P. Jeßberger et al.

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Reply to Reviewer #3

We thank the author for his/her detailed review and the valuable comments.

Major Comments (MC):

MC 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 univer-

sally 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 (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.

Answer to MC 1

We agree that the observations are obviously limited in measurement time and we add a few sentence of caution in the conclusions as suggested.

In order to allow for intercomparability we selected contrail observations at similar contrail age and at specific atmospheric conditions in particular in terms of relative humidity. This selection of course reduces the number of data. Still we end up with several minutes measurement time in the different contrails.

We are aware that the results may vary for different atmospheric conditions. In particular the relative humidity is a critical parameter controlling the ice particle growth and ice particle lifetime, as pointed out in the paper.

Additionally, atmospheric parameters like the temperature, pressure, stratification and wind speeds may vary and influence the contrail properties. However, the measurements were taken well below the critical temperature according to the Schmidt-Appleman-Criterion where contrails can form, so for small variations in temperature we do not expect qualitative variations. Small differences in pressure will not change the qualitative results either. Large differences in the pressure are not to be expected as the altitude where the contrails were observed is a typical cruise altitude for aircraft. The wind speeds and the wind shear might vary, but in the early stage of contrail development the wind is not a major factor as the contrail development still gets controlled by the vortices of the aircraft. Differences in thermal stratification will change the sinking speed of the vortices and thus the vertical extension of the contrails.

For meteorological conditions close to ice saturation, we think that slightly different conditions will not change qualitatively the conclusions we found in this paper. Quantitative changes may obviously occur.

We agree that we need to be more cautious in the formulation of our conclusions regarding their validity for different meteorological situations. We have added a few sentences of caution within the conclusions:

“The results are obtained for the observed meteorological conditions with RHI close to ice saturation. The contrail properties may vary for different meteorological conditions, especially regarding ambient relative humidity and thermal stratification. For older contrails also wind shear may influence the contrail properties. Further observations in contrails at different meteorological would be helpful to confirm the dependence of the total extinction on aircraft and atmospheric parameters, especially at higher ambient relative humidity and in aged contrails.”

MC 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.

Answer to MC 2

We agree that the properties of the young contrails observed during our study cannot simply be extrapolated to the properties of persistent contrails. That is why we have put our focus on the investigation of the influence on the aircraft type on young contrails, as the atmospheric influence gains influence with the aging of the contrail, making it harder to separate between atmospheric and aircraft related impacts.

In addition, we perform a model sensitivity study to investigate the climatic impact of aircraft type dependent contrails in chapter 5.2.4 Importance of aircraft dependence

of contrail properties for radiative forcing. However, we do not simply extrapolate our results but use them to set the initial conditions in the model CoCiP. The CoCiP model is designed to assess the total climatic impact of contrails throughout contrail lifetime (Schumann, GMD, 2012) and the climate impact of a fictive fleet of only A380 aircraft operated during a 3 days period is compared to the climate impact of a fictive fleet of A319 aircraft with the model. To clarify this, we have included the following text in the manuscript:

“CoCiP is also applied to simulate the evolution of contrails for given air traffic and meteorology globally. The method and the approach have been described before (Schumann, 2012; Schumann and Graf, 2013; Schumann et al., 2013). The model is not restricted to young contrails but simulates the full life cycle of all contrails from formation until dissipation by subsidence or by mixing with ambient dry air or by particle sedimentation. The global model results, including the full lifecycle of contrails, are used to compute global mean contrail cover and radiative forcing (RF) by contrails.”

We also express caution using the following text in the manuscript:

“Since contrail cirrus modelling is difficult, these results should be seen as a qualitative indication of the importance of the aircraft impact on contrails.”

MC 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.

Answer to MC 3

Here we distinguish between the downward motion of the two sinking vortices, or the vortex descent and the vertical contrail depths, which is the vertical altitude at which contrail ice crystals are detected. We do not judge any model. The P2P model, which is also the base for CoCiP, uses a probabilistic approach to calculate the parameterized decent distance for the primary vortices, which is explicitly calculated in the LES setup of the EULAG-LCM model.

Results of the P2P model (which – besides LES and measurement results - is also the base for CoCiP results, see Fig. 3 in Schumann, 2012) of the vortex descent depths are given in Schumann et al., 2013, their Figure 1a. Good agreement between the vortex descent depths and the observed vortex depth derived from NO profiles in the contrails for the three different aircraft types A319, A340 and A380 is found till vortex decay. The vortex descent depth of the A380 contrail can also be directly compared to the observations in this study (see Figure 4), the 290 m vortex descent depth from the original flight altitude calculated with the P2P model for the A380 agree very well with the observed vertical profile of the ice particles of at least 270 m.

Also, the descent depths of the vortices derived from the EULAG-LCM model have been compared to the vortex descent derived from the observations for the A380 and the A319 contrails and they agree within 10 %. The 3D setup of the EULAG model exhibits a vertically inhomogeneous vertical vortex location, due to turbulent dissipation

of the vortex in its decay phase.

However, the calculation of the contrail depth differs in the models. While contrail depths with the P2P model is calculated along the total descent depth for the given contrail age, EULAG-LCM calculates the mean vertical depth only where ice particles are present. As described in the manuscript, in the EULAG-LCM no secondary wake develops for the A380, and the secondary wake is thin for the A319, due to reduced mixing but also due to unknown details in the model initialization of the initial ice crystal distribution in the vortex wake or atmospheric stability. In the observations, we see clear developments of secondary wake structures (see Figure 4 in the manuscript). As the secondary structures do not exist in the EULAG-LCM model, the computed effective contrail extension is much smaller than the "total depth calculated by P2P" as the regions without ice particles are not considered for this parameter.

To clarify, we have changed the output parameter of EULAG-LCM from "contrail extension" to "effective contrail extension".

Schumann, U.: A contrail cirrus prediction model, Geosci. Model Dev., 5, 543-580, doi:10.5194/gmd-5-543-2012, 2012.

Schumann, U., Jessberger, P., and Voigt, C.: Contrail ice particles in aircraft wakes and their climatic importance, Geophys. Res. Lett., 40, 2867-2872, doi:10.1002/grl.50539, 2013.

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

A: We have added the wavelength for the optical depth (550nm) both in the abstract and the manuscript.

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.

A: We have changed the text to "CoCiP model results suggest that the aircraft dependence of climate relevant contrail properties persists during contrail lifetime, adding importance to aircraft dependent model initialization." in this passage to clarify.

1396:22 RHI is not yet defined

A: We now have defined RHI in the abstract.

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?

A: We agree that the issue whether the environment is supersaturated or not during the time of contrail development is a very important one.

We do not give ECMWF analysis data the same value as the Falcon. However, there are indications that at some point during contrail development the air was supersaturated with respect to ice. In the paper (13927:23-25) we described that the ice water

contents observed inside the contrails is significantly higher than the engine emissions of water vapor alone diluted in the ageing exhaust. Hence additional water vapor is necessary for ice particle growth. In Gayet et al. (2012) the evolution of the A380 contrail (the same as regarded in this paper) is analyzed in more detail, showing an ice particle growth, which makes supersaturation necessary at least for some time during contrail evolution.

Further persistent contrails are visible on pictures taken onboard the Falcon at the time of observation. However, the other contrails might well have developed under somewhat different meteorological conditions and also at slightly different altitudes. The contrails were observed above of a cirrus region that was propagating in northeast direction over northwestern Germany. We know of no radiosonde data that is available from nearby positions at the time of observations.

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.

A: Thank you for this remark. The extinction for the observed A340 is 2.5, not 2.9, leading to an optical depth of 0.55. This has been fixed in the manuscript.

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).

A: After engine exit, the air is supersaturated due to the aircraft emissions, which initiates ice nucleation in the contrail. This supersaturation is reduced due to ice particle growth and mixing with ambient air.

The descent of the vortex results in higher temperatures, further reducing the relative humidity with respect to ice. Although small scale fluctuations cannot be excluded, we cannot explain the supersaturation with dynamical processes. That is why the ambient air needs to be supersaturated with respect to ice to explain the observed ice water contents.

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 “NOy mixing ratio” would do the job.

A: Thank you for the comment. We now distinguish between model inputs and model outputs in the tables.

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.

A: The values in the text have been corrected. We agree that there is disagreement between the models and the observations.

There are some structural differences between the simulations and the observations, especially concerning the development and evolution of a secondary wake. Here we see big differences both concerning the time scale of the evolution and the extent of the secondary wake. We emphasize in the text that this structural difference is a major factor contributing to differences between observations and models.

To clarify, we do not longer speak of “good agreement” between the model and the observations and put more emphasis on describing the observed differences and the need for further investigation in the text. We have also added additional calculations performed with the bulk model to further investigate the differences.

However, regarding the trends in the optical and microphysical properties with increas-

ing aircraft weight, both models can reproduce the trends in the observations.

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

A: This section has been shortened and rewritten to avoid repetition.

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.

A: We no longer use the word “observed” optical depth. We now speak of “derived from observations”, whenever appropriate.

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