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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Large-eddy simulations of contrails in a turbulent atmosphere" *by* J. Picot et al.

Anonymous Referee #2

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Review of "Large eddy-simulations of contrails in a turbulent atmosphere", by J. Picot, R. Paoli, O. Thouron and D. Cariolle, ACPD.

The paper studies the impact of ambient turbulence, wake vortex turbulence and microphysics on contrail evolution. The paper is interesting, because it addresses the relative importance of atmospheric turbulence and wake-instability induced turbulence on contrail properties. However, the paper still leaves many questions open, as explained below.

There are many minor text issues and a few but possibly important basic questions that need to be addressed before the paper may become acceptable. The questions may require further analysis of the simulations, and, hence, may trigger major revisions.

Title: The paper does not simulate the contrail over its full life time. Hence, I suggest changing the title to be more specific, e.g., "Large-eddy simulation of contrail evolution in the vortex phase and its interaction with atmospheric turbulence."

Abstract, line 14: replace "global" by "mean". The word "global" suggests a relationship to the global atmosphere which is not discussed in this paper.

Line (L) 19: Delete the word "ongoing". One cannot refer to ongoing studies in the literature.

Page (P) 29501, line 8: replace "made" by "given" – this classification was given in earlier studies.

L 14/15: This is an often stated misconception: The vortex descends because it carries the downward momentum needed to balance the weight of the aircraft. The concept of mutual induction is a picture which results from potential flow simulation of the wakes. The potential flow model describes the flow in a kinematic sense but does not explain the dynamical reason for downward motion of the vortex as a whole, i.e., the aircraft weight.

L 20: add comma after "humidity"

P 29503, L 1: add "axial" before "range"

L 5: (here and at later places) replace "global" by "mean"

L 14, insert comma after "time"

Introduction: the introduction misses to introduce previous 3d LES of non-contrail wake dynamics (e.g. Misaka et al.).

You might also relate your LES to other LES work and cite the resolution reached elsewhere. In view of the recent GRL paper by Lane and Sharman (GRL; 2014, doi:10.1002/2014GL059299), who simulate the troposphere and lower stratosphere with 8000 × 1200 × 334 grid points, your UTLS simulation (2048 **3 grid points) is impressive, but the present contrail LES with 519 x 619 x 100 is still only moderately resolving. Also Lewellen et al (2014) have used higher resolution. You may also point out that a simulation, which resolves the contrail dynamics from engine exit (with core and bypass engine jets), or even from the engine combustion chamber to early vortex phase, including interaction with the various counter-rotating vortex systems that form behind real aircraft with wing and tail, is still missing. It would require far higher resolution, but may be necessary to understand the details of ice nucleation in young contrails.

L 21: are you sure that the Ozmidov scale L_O (as defined, e.g., in Riley and Lindborg (JAS, 2008, DOI: 10.1175/2007JAS2455.1)), is one order of magnitude larger than your grid scale? Please give numbers. I compute very small values from L_O= (epsilon/N**3)**(1/2): L_O= 3 m for your values, epsilon=1.6E-5 mˆ2/sˆ3, N= 0.012 sˆ-1. This value is small, in particular for your low-turbulence case (epsilon a factor of about 3 smaller). I am not at all convinced that your grid-scale turbulence can be isotropic. Please show me data to convince me otherwise. Measured turbulence spectra show large departures from local isotropy at these scales (e.g., Schumann et al., JGR, 1995). As a consequence, I expect that you may find a strong sensitivity of your results to numerical resolution.

P 29506: L 3, how does the selective dissipation influence your results? How large are your values of subgrid scale diffusivities? Can you give an effective Reynolds number for your contrail simulations based on contrail scales and SGS diffusivities?

L 10: The SI rules exclude usage of units like kg-fuel. Delete "-fuel".

Is the value 10**5/kg correct? I think it should be 10**15/kg.

P 29512, Results: I agree that the eigenvalue lambda_2 is negative for the most intense vorticity. Hence, smaller lambda implies stronger turbulence. However, the text would be easier to follow if you use negative eigenvalues and then say turbulence is the stronger the larger your (negative) eigenvalue.

L 17: I miss an explanation for the selection of the scaling factors 10 and 400000.

P 29513, L 1: "Various numerical simulations" – here you have to add the references to the papers you refer to. My impression is that you have not yet studied those vortex turbulence papers carefully. The discussion is not sufficiently precise and to the point. When you have done so, you will delete the word "vast" in line 11. You may also put another "by" before "Misaka in line 14: Only Misaka did discuss passive scalars. In fact, Misaka did not only discuss passive scalars but mainly the 3-d dynamics of wake vortex bursting. They presented fine-scale LES and comparisons to observations.

P 29514, L 2: add "range" after "axis"

L. 25: replace "after" by "until"

At the end of section 4.1, it was not clear to me what you presented in this section as basically new insight.

P. 29515, L 11: what is "well-distributed"??

L. 17, add "the" before "secondary"

Last line: replace "per meter" by "per unit flight distance".

P 29516, L 1, replace "elementary volumes" by "volume of the grid cells"

Your method of assuming a grid cell as being part of a contrail for just one crystal gives a bias to overestimate the volume. This will have consequences for your results, as discussed below.

L 7: the sentence is unclear. Do you mean the importance of the intensity?

L 18: This is a misconception: the potential mass of ice results from both the emitted water mass and the water mass above saturation in the plume entrained from ambient air. Hence it is larger than M_v,0. See also Eq (12) of Jeßberger et al. (2013).

P. 29517, L 5: At the end of the sentence before Figure 11, a reference to previous studies on the global climate impact of contrails, which account for particle sizes, is needed.

Lines 5 to 12 address the number of ice particles. Lines before line 5 and after line 12 address the mass of ice. I suggest some reordering to bring the mass aspects together.

Eq. 16: I note that M_v,a depends on the definition of V_p. Hence, an overestimate of V_p has the consequence that the corresponding mass M_v,a is overestimated. And this has consequences for the fraction 30% given later. I guess, this value could be smaller if you would use higher numerical grid resolution or other V_p definitions.

P. 29518: L 1-4. I have the impression that this discussion is wrong. Turbulence does not impact the ice deposition but the mixing of air between the contrail plume and ambient air.

L 14: This discussion misses to discuss the impact to particle sizes. See d_p in Eq. (7).

L 18: to make the sentence meaningful, you have to add "by ???%", with the proper value instead of ???, after "reduced"

L 22: the notation for S_xy is cumbersome. I suggest omitting the formal definition. The text is clear enough without.

Eq. (25): When the reader comes to this equation, he cannot know whether the last bracket is a multiplier to Q or the argument of a function Q(rho). Perhaps you find better ways to formulate this.

P. 29519, L13: Here you could compare your results with Eq. (12) of Jeßberger et al. (2013).

L 16: I am not convinced that the fluctuations are mainly due to turbulence. They could be as well due to the variability of Q(rho), your eq. 18, which is an oscillating function

of the argument rho for small values of rho.

L. 20: what is 0...3

P 29520, Eq 19). Schröder et al. (2000) present plots of dn/dlogD. I am not sure that your definition is consistent with the log-part.

L 12: the word "Besides" is misleading and can be omitted.

L 12: Please define what you call a "sublimation tail".

L 26: please note that Jeßberger et al (2013) were not able to explain the observed number of large ice particles with their models. See their Fig. 6 and related discussion. I am not sure whether your model explains the number of large particles (up to 20 micrometer) as measured. Your maximum diameter seems to stay well below 10 micrometer. The large particle may arise becaue of quickly growing ice particles nucleated at the outer edge of contrails, in humid and cold air, in the jet regime,

P 29521. L 24 etc.: The discussion of the comparison between your results and those by Holzäpfel, Hennemann, Misaka, Lewellen et al. is not satisfactory.

For example, Hennemann and Holzäpfel (20119 pointed out the importance of the integral length scale of turbulence to the instability of the wake vortex lines. From your paper, I miss data about the variance of turbulence intensities (in 3 directions) at the scales of the simulated contrail. What would be the integral length scales in your simulation relevant for wake vortex dynamics?

P 29522, L 1: do you mean contrails instead of vortices?

L 9: "at time scales" instead of "on time scales" ?

L 14: the 30% result: How sensitive is this result to the definition of V_p. Please note, the number of grid cells containing a few ice particles increases as the plume radius (or surface) increases.

Actually, I do not understand why 30% of water vapor above ice saturation should remain in the gas phase for a contrail with a high density of ice particles of the given size. I expect that the time scale for return to saturation is very small in your simulations.

For this purpose, you may consider the time scales for return to ice saturation given by Korolev and Mazin (2003). This is also discussed in Kaufmann et al. (GRL, 2014, doi: 10.1002/2013GL058276). In fact, the quantification of such time scales will add further insight into your results. (Reference: Korolev, A. V. and I. P. Mazin (2003). Supersaturation of Water Vapor in Clouds. J. Atmos. Sci. 60: 2957–2974 DOI: 10.1175/1520-0469(2003)060<2957:SOWVIC>2.0.CO;2)

Your paper misses a discussion of numerical approximation errors and their impact on the results. I feel this is quite relevant because the number of grid cells per contrail diameter is still not very large.