Response to Referee 1

Common response to the referees: Thank you for your thorough review and suggestions to improve the paper. We ran two addition LES with Kelvin effect activated, we substantially modified the text, added new figures and an appendix to provide further details of the numerical simulations.

Overview

Q. Most of the analysis focuses on wake vortices, not so much on the contrails. The discussion in Section 4.1 is self-contained and the selection of most of the included figures appears sensible. Nevertheless, wouldn't it be more illustrative to show vertical profiles of contrail mass/number rather than some 2-based properties as done in Figure 8?
A. Yes, we agree that vertical profiles of contrails mass/number are more representative for contrails than the λ_2 profiles. We added two new figures and the corresponding discussions: Fig. 10 and Fig. 13 (line 666).

Q. Your results suggest a weak impact of turbulence, which could not be known in advance and thus does not invalidate your efforts and results. Nevertheless, I would appreciate when further simulations are carried out to explore the sensitivity of more significant parameters. This should not be postponed to future studies. Including the Kelvin effect would also allow to compare your simulations better with other recent studies.
A. We understand your point. As you mentioned, our goal is not to explore a large parameters space as done by the cited authors but (i) to focus on the atmospheric turbulence effects in the vortex phase and (ii) prepare a the initial conditions for a follow-up study of contrail-to-cirrus transition in the diffusion phase. Because of the chosen methodology, changing the atmospheric conditions such as stability or temperature would imply recalculating the entire background turbulence field over a 1024^3 computational domain, until statistically steady conditions are achieved. We do not currently have the computational resources to run these simulations (those used for the present study were granted by a dedicated UE project which took about 20 million CPU hours). However we took your suggestion of checking the influence of Kelvin effect and ran two additional simulations of the vortex phase for RHi=110% and 130%. We found that, consistently with the recent work, the number of surviving crystals decreases when Kelvin effect is activated. We added the new Section 4.3 separated from the Discussion/Conclusions to account for these results.

Major comments

Q. Lewellen et al. (2014, p.4404) and Lewellen (2014, p.4436) state that contrail properties depend not only on statistical properties of the turbulent fields, but also on the specific realization of the turbulence field. Are your reported differences between cases 1-3 significant, especially as they are sometimes small? Figure 2 nicely shows the boxes from where you extracted your specific flow field. I recommend to shift the boxes to other positions (one or two extra simulations). In the present selection, the values in the right panel are mostly negative (bluish). What would happen if you selected a box from a reddish part of the domain? Anyway, how do you manage to have periodic boundary conditions in the end?
A. Figure 2 (right) shows one horizontal slice of the atmospheric turbulent filed at flight level, the aim being to illustrate the topology of atmospheric turbulent fluctuations in the large atmospheric domain. However, the data for the vortex phase simulations were extracted in a 3D box that include lower and higher levels where perturbations are different and eventually opposed to those shown in the Figure (bluish instead of reddish and vice versa), producing a variability in the background field. We do not expect shifting the box would produce variation in the contrail mean quantities. Figure A1 in Lewellen (JAS, 2014) also shows a slight impact of turbulence realization only for axial grid spacing $\Delta x > 50m$ (which is an order of magnitude larger than Δx=4m used here) and for wake ages longer than a few hours (but even at 10 hours the differences between realizations remain small). We added a comment in Sec. to mention this point though.
Periodic boundary conditions in a given direction, say x, are enforced by replacing $f(L_x) = f(O)$ for any variable f. Although this operation slightly modifies the background turbulent fields, the latter equilibrate to this

constraint in a few time steps (furthermore, the amplitude of ambient fluctuations are small compared to the Lamb-Oseen vortex flow-field).

Q. Stratification strongly affects the wake vortex decay. What about testing a second value of N?
A. As mentioned above, we do not have the resources to run additional turbulence simulations in particular for the turbulence simulation in the large domain.

Q. Your value of EIs is at the lower end of the range investigated by Lewellen (2014, see their Fig. 2). The lower EIs is, the fewer ice crystals get lost. In addition to the omission of the Kelvin effect, this could explain your high survival rates compared to other studies. The present study would benefit from a Eis variation.
Q. p29509: Unlike all other recent simulations studies you did not consider the Kelvin effect, although it was shown to affect contrail microphysics. I strongly recommend to include the Kelvin correction term in the deposition equation. See, e.g., Eq. 14 in Naiman et al. (2011). This should not be postponed to a follow-up study as proposed in Sect. 5. Moreover, the inclusion of the Kelvin effect would make comparisons between the various modeling studies more conclusive and improve your Sect. 5.
A. Kelvin effect is the main cause for this difference (although we agree that the low $\mathrm{EI_s}$ may also contribute). Then we decided to put the all the effort on running two additional LES with Kelvin effect activated and added the new Sec. 4.3 accordingly.

Minor comments

Q. Your flow field analyzes suggest vortex break up after two minutes. For case 4, most of the ice crystals get lost after that time. I always thought that the vortex sinking is the main driver for crystal loss. What is the reason for the continued ice crystal loss? As mentioned above, vertical profiles of, e.g., contrail ice mass may reveal vortex sinking for a longer time.
A. Yes, the adiabatic compression is the main driver of crystals loss. For case 4 this starts before 2 minutes. The latter is the time when vortices first collide and subsequently break up but the descent continues in the dissipation regime. To clarify this point we compared (line 666) the mass profiles at t=*4.5* min case 2 (s_0=*1.3*), which shows two peaks in the primary and secondary wakes and for case 4 (s_0=*1.1*), which shows only one peak in the secondary wake due to ice sublimation in the primary wake.

Q. Figure 1: I think it is not necessary to include Figure 1. In my opinion it would be enough to add one sentence in the text and simply report the dimensions of the domain that has the highest resolution, i.e. 1m x 1m x 4m.
A. Because the mesh is stretched in the cross-sectional plane, we think this is useful information, which helps the reader to judge on the computational details of the simulation (similarly, for example to Fig. 2 in Naiman et al (JGR, 2011)). In our opinion, providing only the finest resolution (minimum grid spacing) without showing the domain itself (as done for example Table 1 in Lewellen et al, 2014 where the extension of the regular portion of the grid or the details of the stretching laws are omitted) does not allow for a critical evaluation of the numerical grid.

Q. Figure 9: The three selected cases look fairly similar. In my opinion, it is enough to show just one case or replace two of them by cases 4 or 5.
A. We prefer to leave cases 1-3 that correspond to three levels of turbulence with everything else left unchanged (the goal here is to show the effects f atmospheric turbulence on Crow instability).

Q. Quantity Lx (length of vortex axis): I understand that Lx helps to identify the time of vortex collision. However, the description of how Lx evolves seems longer than necessary to understand the contrail evolution.
A. We guess you mean x_v. We see you point but we prefer to leave it as a diagnostics of the vortex induced turbulence.

Q. Is it necessary to define the Hact, as done in Eq. 5? In p29511, l.12, you state that the particles are activated anyway. Your simulations start at a wake age where nucleation has long been finished. So would it not be better

to not speak of nucleation sites? And instead just say "ice crystals". Or do I mix up anything?
A. You're right that all particles are activated at the beginning of the simulation. However, ice crystals can sublimate and potentially reactivate later on (we did not observe the timescales of the present study though). In this case the test function H_p^{act} is needed to identify the thermodynamic conditions for nucleation (water saturation in our simple model) and we kept it in the general formulation of Sect. 2.

Q. LPT method:
-Are 2 million particles enough? Naiman et al. (2011) speculate that 8 millions particles might be not enough? Unterstrasser (2014) states that the number of simulation particles is not a limiting factor, however they use more particles than you do.
- The relevant turbulence, does it happen on the resolved scales, or on the subgrid scale? Is subgrid scale motion considered for ice particle transport? Is it important?
We did not analyze the sensitivity to the number of particle clusters for this specific study but we did it in previous LES of two-phase flows including contrails and observed that the critical quantities such as the mean variables and the momentum, energy and mass transfer rates between gaseous and particulate phases are well captured with a few millions of clusters.
We did not investigate the effects of subgrid-scale fluctuations "seen" by particles. In LES all variables are already filtered implicitly and the interpolation procedure using grid-node values acts as an additional filter. This problem has been studied in engineering applications of two-phase flows (particularly for the velocity field and its feedback on the gaseous carrier phase) but generally in view of characterizing high-order turbulence statistics. We deserve this to further study although we are quite confident that most of kinetic energy is resolved with a grid of 4m resolution for atmospheric applications (80% according to Pope's criterion). We proved this in the LES of atmospheric turbulence that serve as background field for this study (Fig. 3 in Paoli et al (ACP, 2014)).

Q. The comparison with observations is neither very conclusive nor convincing. The environmental and aircraft parameters in your simulations and the observations are not similar or unspecified. What do you want to demonstrate with these comparisons? Are your interpretations and drawn conclusions robust? Naiman et al. (2011, Section 5) shows a profound attempt to compare simulations results with observations. However, I am not sure, whether such an exercise has to be reproduced here.
A. We did not intend to reproduce the same exercise as in Naimann et al, 2011 nor to make a validation against a precise case study, the goal was just to show how some of the key wake and contrail parameters (ex. descent velocity, size distributions) are in the range of observed values.