# 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 $\lambda_2$ 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 $\Delta$x=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 $EI_s$ 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 $H_p^{\text{act}}$ 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.

# Response to Referee 2

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.

*Q. 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".*
A. Done

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

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

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

*Q. 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.*
A. We agree, indeed we didn't say vortices descend *because of* mutual induction but simply mentioned the dynamical mechanism explaining the descent. We slightly changed the sentence as: "The vortices also interact each other via a kinematic process of mutual instability.." (line 63).

*Q. L 20: add comma after "humidity"*
A. Done

*P 29503, L 1: add "axial" before "range"*
Q. Done

*Q. L 5: (here and at later places) replace "global" by "mean"*
A. Done

*Q. L 14, insert comma after "time"*
A. Done

*Q. Introduction: the introduction misses to introduce previous 3d LES of non-contrail wake dynamics (e.g. Misaka et al.).*
A. The literature of wake vortex dynamics is vast so we cited some classical reviews that contain the relevant references for the reader interested in the subject. We also cited the works by Gerz and Holzapfel (AIAA J, 1999) and Misaka et al (PoF 2012) as they analyzed at different levels the scalar dispersion in addition to the vortex dynamics (line 106).

*Q. 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.*
A. Lane and Sharman analyzed a different problem and focuses on scales much larger than ours so we decided not to cite this work.
In the central portion of our stretched mesh, the cross-sectional grid spacing is uniform and equal to $\Delta x = \Delta z = 1m$. This amounts to 8 grid points in the vortex core, which is a decent resolution for a Lamb-Oseen vortex. In the axial direction, the grid spacing is $4m$, enough to barely capture the short-wave elliptical instability that scales with the vortex core radius (in addition of course to Crow instability that scales with the wingspan). We disagree with your comment on Lewellen et al (JAS, 2014) work. Their Table 1 reports the maximum resolution (minimum grid spacing) and the overall dimensions of the computational domain used for different meshes at different wake ages. Unfortunately, the table does not report the extension of the domain where the grid size is uniform (if the mesh has a uniform portion) or the stretching law used for the mesh, or a figure showing the mesh itself. This missing information is crucial because the minimum grid spacing alone is not enough to critically evaluate the LES resolution: for example, the Lamb-Oseen vortex has to be resolved over a few wingspans in the cross-section to avoid the effects of grid dissipation and distortion of the flow-field on the instability patterns and on the vortex interaction with the ambient turbulence.

*Q. 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.*
A. Done (line 190).

*Q. 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 m^2/s^3, N= 0.012 s^-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.*
A. We first would like to point out that the concept of Ozmidov scale like that of buoyancy scale or Kolmogorov scale are all based on dimensional arguments rather than on definitions derived from first principles. We made quite a big effort to evaluate the influence of grid resolution on the spectra and the structure of stratified turbulence in the work by Paoli et al (ACP, 2014). In that work and in the present paper we used the definition of Ozmidov scale given by Waite (PoF, 2011, DOI: 10.1063/1.3599699, Eq. 2) and used in DNS and LES of stratified turbulence thereafter. This definition includes the factor $2\pi$ to account for wavenumber ($k$) to wavelength ($\lambda$) transformation, $\lambda = 2\pi/k$ as it is the wavenumber that emerges in spectral theory of homogeneous turbulence. With this definition we have $L_O = \lambda_O = 2\pi(\varepsilon/N^3)^{1/2} = 19.4m$ for the moderate turbulence (corresponding to run M04, $1024^3$ in Tab. 1 of Paoli et al (ACP, 2014)) and $11m$ for the weak turbulence. These values are larger than the grid spacing, 4m, used both in Paoli et al (ACP, 2014) and in the present study in the axial direction (in the cross-sectional direction we further reduced it to $1m$ to resolve the vortex core). In order to further assess the resolution of our LES and for the sake of quantitative analysis we reported the ratio between the resolved and total (resolved+subgrid) energy and the turbulent kinetic energy spectra. Figure R1 below shows that in statistical steady conditions, the subgrid-scale energy is at most 1% for the cases studied here, far below 20%, the criterion proposed by Pope for *a posteriori* evaluation of LES (NJP, 2004, DOI: 10.1088/1367-2630/6/1/035).

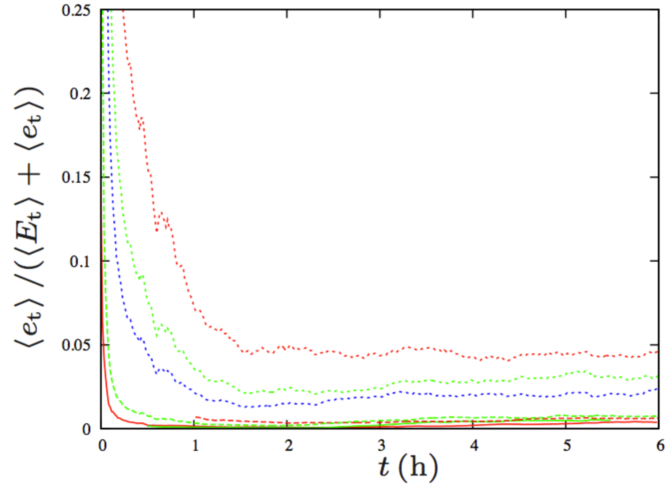


Figure R1. Evolution of the ratio between subgrid and total turbulent kinetic energy for various LES of turbulent stratified flows (from Fig. 3 in Paoli et al (ACP, 2014)). The relevant case for the present study is the green dashed line (run M04, $1024^3$ with Δ=4m resolution). In steady conditions the ratio amounts to about 1% (it reaches 5% only for the three cases with resolution Δ=10m corresponding to the three upper lines)

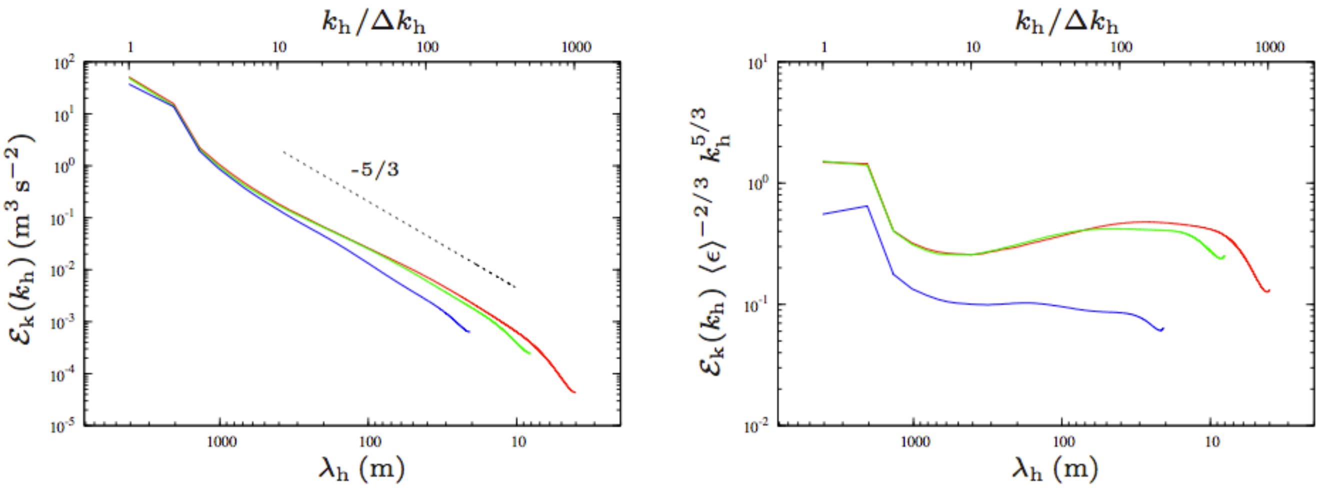


Figure R2. Horizontal turbulent kinetic energy spectra (raw and compensated) for various LES of turbulent stratified flows (from Fig. 11 in Paoli et al (ACP, 2014)). Red lines: Δ=2m; green lines: Δ= 4m (run M04, relevant to the present study); blue lines: Δ= 10m. Except for the low-resolution case Δ=10m), the Ozmidov scale following Waite (PoF, 2011), $\lambda_O = 19.4m$ is fairly in the well-resolved inertial range.

Furthermore, the horizontal turbulent kinetic energy spectra for run M04 in Figure 2 indicate that the wavelength $\lambda_h = \lambda_O$ =19.4 m is fairly within the well-resolved -5/3 inertial range.

Unfortunately we cannot quantitatively compare with Schumann et al (JGR, 1995). These data were obtained for specific atmospheric conditions with an exceptionally weak turbulence with dissipation rates $\varepsilon = O(10^{-8})\ m^2s^{-3}$ (their Tab. 7). These values are two to three orders of magnitude smaller than the mean dissipation rate reported in the literature at the tropopause level, which is rather $\varepsilon = O(10^{-5})\ m^2s^{-3}$. This value was calculated by Lindborg (JFM, 2006, DOI: 10.1017/S0022112005008128, page 213) and by Cho and Lindborg (JGR, 2001, DOI: 10.1029/2000JD900814) and Lindborg and Cho (JGR, 2001, DOI: 10.1029/2000JD900815) by analyzing an ensemble of Mozaic data.

Of course we did observe anisotropic structures in our LES of atmospheric turbulence (see for example the pancake structures in Fig. 6 of Paoli et al (ACP, 2014) but not at scales smaller than the calculated Ozmidov scale. The anisotropy was also quantified in Tab. 1 of Paoli et al (ACP, 2014), which shows rms $\sigma_x \approx \sigma_y = 0.4\ ms^{-1}$ and $\sigma_z \approx 0.1\ ms^{-1}$ for the background turbulence. To our knowledge, direct measurements of velocity fluctuations in the three directions are not available at scales of meters or a few tens of meters. Tables 6 and 8 in Schumann et al (JGR, 1995) do show anisotropy but they pertain to larger scales (order of $10km$ *or* more), which is perfectly compatible with our data and spectra (assuming that the inertial range shown in Nastrom and Gage (JAS, 1985) can be prolonged down to subkilometer scale as suggested by Lindborg (JFM, 2006).

*Q. 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?*
A. All centered finite difference schemes are subject to numerical instabilities when spurious solutions from the discrete problem are excited by the approximations caused by the under-resolved flow gradients, grid stretching or boundary conditions. This type of high wavenumber instability is classically overcome by either adding an artificial dissipation through upwinding, by adding explicit damping terms, or by filtering the solution. We have chosen to follow the approach of Tam and Webb (JCP, 1993), Tam et al (JCA, 1993) and later refined by Barone (PhD thesis, Stanford 2003) by adding a selective dissipation term to the equations. This is a high (=6th) order compact dissipation that is derived in spectral space with the dissipation operator given by: $D_x \sim \Delta x^5 \frac{\partial^6}{\partial x^6}$. The damping is strongly restricted to the high wavenumber of the spectrum due to this functional form as demonstrated in the aforementioned papers (see for example Fig. 3.12 in Barone's thesis).
The subgrid scale model in this study is based on filtered structure function by Lesieur that provides at each grid node the turbulent viscosity reconstructed locally using the nine closest grid points surrounding the grid node. The figure below shows the evolution of the min, max and mean values of turbulent viscosity normalized by the molecular viscosity $\nu_{lam}$. We can see for the mean turbulent viscosity reaches a decent factor of 10 times the molecular viscosity at the moment of break-up (where the gradients are the largest). Peak values of this ratio are of course larger although they are local in nature as they pertain to specific points in the flow-field. We computed the effective Reynolds number defined as $\mathrm{Re}_{\mathrm{eff}} = \frac{\Gamma}{\nu_{\mathrm{tot}}}$ with $\nu_{\mathrm{tot}} = \nu_{\mathrm{mol}} + \nu_{\mathrm{turb}}$.

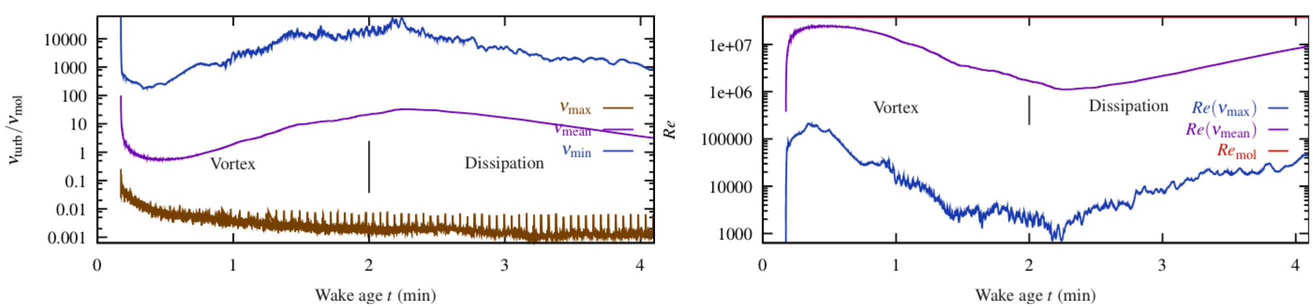


Figure 3. Left panel: Evolution of the ratio of turbulent-to-laminar viscosity (minimum, maximum and mean values in the flow-field). Right panel: Evolution of the effective Reynolds number abased on circulation and the total viscosity: $\mathrm{Re}_{\mathrm{eff}} = \frac{\Gamma}{\nu_{tot}}$. The Reynolds number based on the molecular viscosity is also plotted for reference.

*Q. 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.*
A. Yes, it is $10^{15}$ kg$^{-1}$, thanks!

*Q. 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.*

*Q. L 17: I miss an explanation for the selection of the scaling factors 10 and 400000.*
A. We observed that taking a tenth of the largest value of a lambda-2 structure provides a good visualization of this structure. Thus we chose $\lambda_2 = \lambda_2/10$ to visualize wake vortices; this value is sufficiently high to filter out secondary vortices and atmospheric eddies. As $\lambda_{2,atm}/\lambda_{2,0} \approx 1/40000$, we chose $\lambda_{2,0} = \lambda_{2,0}/40000$ to visualize the atmospheric eddies.

*Q. 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.*
A. We reformulated this part and added proper references (starting at line 493).

*Q. P 29514, L 2: add "range" after "axis" L. 25: replace "after" by "until"*
A. Done.

*Q. At the end of section 4.1, it was not clear to me what you presented in this section as basically new insight.*
A. Indeed, the main objective of this section was to show that our model is able to capture the main dynamical features wake vortices before discussing the microphysical aspects of contrails.

*Q. P. 29515, L 11: what is "well-distributed"?? L. 17, add "the" before "secondary" Last line: replace "per meter" by "per unit flight distance".*
A. We removed "well" and corrected the other points.

*Q. 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.*
A. Done.

*Q. L 7: the sentence is unclear. Do you mean the importance of the intensity?*
A. Yes, we corrected.

*Q.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).*
A. You're correct even though at a wake age of 10 second, much of the formed ice mass is due to the deposition of exhaust water, the contribution of entrainment from ambient vapor is initially small but becomes predominant later on. The timescales controlling the competition between entrainment of ambient air and deposition depend on ambient temperature and $RH_i$ and were also discussed in the paper by Paoli et al (PoF, 2013).

*Q. 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.*
Q. Done

*Q. 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.*
A. Done.

*Q. 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.*
A. The idea behind $M_{v,a}$ in Eq. 16 is motivated by modeling/numerical arguments rather than physical arguments. We wanted to evaluate the error that would be made by an "equilibrium microphysical model" that assumes an instantaneous return to equilibrium (100% $RH_i$ conditions). In this case, the mass of available vapor $M_{v,a}$ is evaluated by assuming a uniform saturation field in each grid cell just as an equilibrium microphysical model would do. Assuming that ice crystals absorb all available vapor in the corresponding grid cells leads to an

overestimation of the actual ice mass, and you're correct that reducing $V_p$ will reduce $M_{v,a}$ as well. With that said, we agree that $M_{v,a}$ is not a proper diagnostics from a physical standpoint. A better diagnostics to analyze the thermodynamic non-equilibrium in the contrail is the instantaneous mean (ensemble average) relative humidity around particles, which has been added to the discussion as explained later.

*Q. 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.*
A. This is what we meant although we tied to make the discussion clearer (line 712). Strong turbulence leads to higher entrainment rate of supersaturated ambient air into the plume. This in turn (or indirectly if you want) increases the ice deposition rate, which scales with the local saturation ratio $s(\boldsymbol{x}_p)$ or the vapor mass fraction $Y_v(\boldsymbol{x}_p)$.

*Q. L 14: This discussion misses to discuss the impact to particle sizes. See d_p in Eq. (7).*
A. In this context, for the estimation of $Q_d$, the mean particle radius $\langle r_p \rangle$ can be considered constant between 1 and $2 min$ like the ice mas (that scales as $\langle r_p^3 \rangle$) shown in Fig. 12. We mentioned it in the text (line 734).

*Q. L 18: to make the sentence meaningful, you have to add "by ???%", with the proper value instead of ???, after "reduced"*
A. We corrected: "reduced by 15%" (from $s_0 = 1.3$ to 1.10).

*Q. L 22: the notation for S_xy is cumbersome. I suggest omitting the formal definition. The text is clear enough without.*
A. We prefer to leave this notation as it eases the definition of optical thickness in Eq. 18.

*Q. 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.*
A. Done, we switched Eq. 18 with the definition of $Q(\rho)$ before Eq. 17 and adapted the text (from line 747).

*P. 29519, L13: Here you could compare your results with Eq. (12) of Jeßberger et al. (2013).*
A. Jeßberger et al. (2013) established Eq. (12) using an effective contrail of ellipsoidal cross section. Consequently the equation needs a contrail effective height, effective width and effective area. We did not succeed to compute this values with enough accuracy and confidence to present a sound comparison with their results. However we had reported in Fig. 14 the most probable value of the optical thickness (peak pdf) measured by Voigt et al (GRL, 2011) as reported by Jeßberger et al (ACP, 2013).

*Q. 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.*
A. Yes, we reformulated the sentence (line 775).

*Q. L. 20: what is 0...3*
A. It is 0.3, we corrected.

*Q. 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.*
A. For comparison with Schröder et al. (2000) we did compute the log-plots of the size distribution, however we agree that Eq. 19 is useless and confusing so we eliminated it (the log-plot is standard for particle size distributions).

*Q. L 12: the word "Besides" is misleading and can be omitted.*
A. Done.

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

A. We defined it as the portion of the size distribution that spreads to very small values (diameter less than 0.1μm). It identifies the ice crystals that are shrinking and are likely to sublimate.

*Q. 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 because of quickly growing ice particles nucleated at the outer edge of contrails, in humid and cold air, in the jet regime.*
A. Yes, this might be a plausible explanation. In order to verify it, one should simulate the initial development (roll-up) of the wake of the airliner and its interaction with the exhaust jets, something that was beyond the scope of the present study.

*Q. 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?*
A. The dissipation rate calculated in Hennemann and Holzapfel (JAE, 0211), Misaka et al (PoF, 2012) etc and measured by Schumann et al (JGR, 1995) correspond to exceptionally calm meteoroidal condition. However, Mozaic data indicate that the mean dissipation rate at the tropopause is $\varepsilon \approx 10^{-5}\ \mathrm{m^2s^{-3}}$ (rather than $10^{-7}$ or $10^{-8}\ \mathrm{m^2s^{-3}}$) as investigated by Lindborg and co-authors mentioned above. In the present paper ,we did not want to reproduce a specific experiment but have turbulence parameters as much as possible representative of cruise conditions --indeed were surprised (or we missed something) to see in Fig. 1 in Misaka et al that the largest scale represented in the simulation was $6.28\ \mathrm{m} = (2\pi/10^0)$)

*Q. P 29522, L 1: do you mean contrails instead of vortices?*
A. We mean vortices but we slightly reformulated the sentence.

*Q. L 9: "at time scales" instead of "on time scales" ?*
A. Done.

*Q. 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.*
A. The contrail volume $V_p$ represents a ``bulk" volume of the plume that is a collection of elementary volumes of size $h^3$ (that do not necessarily coincide with the grid-cell volumes) containing a particle. We tried different $h$ as shown in the Figure below and we finally chose $h = 10$ m to plot $V_p$ in Fig. 10. We admit this is rather arbitrary, hence we decided to plot Fig.10 using a more objective definition $V_p$ based on the same grid-cell volumes of the LES mesh, which is also consistent with the definition of $M_{v,a}$ (see next below). We apologize for the confusion for using the same symbol $V_p$ for two different quantities. There is no more ambiguity now.

*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 )*
The definition of $M_{v,a}$ in Eq. 16 was based on $V_p$ calculated by summing up the grid-cell volumes of the LES mesh. As explained above, the idea behind $M_{v,a}$ was to estimate the error that would be introduced by a model forcing a instantaneous return to a cell-based equilibrium at every time step. For the current resolution, these models overestimate ice mass by 30%. As you correctly pointed out, this diagnostics depends on the definition of $V_p$, may not be suitable to evaluate the actual equilibrium in the contrail. To that end, we reconstructed the

mean saturation ratio by an ensemble average over all ice particles and its evolution is shown in the figure below. In the middle of the vortex phase between 1 and 2 minutes, thermodynamic conditions are very close to equilibrium (relative humidity is slightly less than 100%) because of the sublimation due to adiabatic heating balancing the deposition due to the entrainment of fresh ambient vapor (similar levels of relative humidity were observed for example in the LES of Naiman at al (JGR, 2011, their Fig. 8)). In the dissipation phase humidity is greater than 100% as the ice crystals originally trapped in the vortices are fully exposed to ambient vapor although relative humidity does not exceed 104% for the conditions of this study. As a side comment, the obtained mean superstations are comparable with those from the modeling study by Korolev and Mazin (2003) even if they focused different clouds and atmospheric situations.

*Q. 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.*

A. The code has been extensively used for DNS and LES of turbulent flows including contrail simulations for the last two decades in our group (of course with upgrades and addition of new physical modules). In this work, we did our best to design the numerical grid that allows for a fair resolution of contrail dynamics. We provided additional information on the energy spectrum and the subgrid-to-resolved energy for the atmospheric turbulence used as background flow, and the turbulent viscosity and effective Reynolds number for the contrail simulation. Concerning the resolution of our LES, as shown in Fig. 1, our grid spacing is uniform and equal to $\Delta x = \Delta z = 1m$ in the cross-sectional central portion of the mesh: $L_{x,\mathrm{reg}} = 400m$ in the transverse direction and $L_{z,\mathrm{reg}} = 450m$ in the vertical direction. This amounts to about 10 vortex spacing $b$, which is enough to contain the contrail during the vortex phase. Along the vortex axis, the resolution is $\Delta y = 4m$, which, we admit it can only barely resolve he elliptical instability (that scales with the core radius) but this was not the main scope of the paper (we are interested in Crow instability as the primary source of instability for the vortex system). We feel these numerical data are comparable with the latest 3D LES of contrails in the vortex phase that used the finest resolution. For example, Unterstrasser (JGR, 2014) employed a uniform grid spacing of $1m$ in the cross-sectional directions (in the axial direction he has $2m$). Naiman et al (JGR, 2011) used a meshing procedure similar to ours (according to their Fig. 1 and Table 5). In the central region, the grid spacing is $\Delta x = B/128$ (all data are normalized by the wingspan) for the first 30 seconds; then it is reduced to $B/64$ and finally to $B/32$ between 2 and 5 minutes wake age. In dimensional quantities, taking $B = 60m$ for a B747 as in our study, this amounts to $0.47m$, $0.93m$ and $1.86m$, respectively. Hence, a part for the first 30 seconds, our resolution is the same or higher than their resolution. Finally, as mentioned earlier, we do not have elements to evaluate the grid resolution in Lewellen et al (JAS, 2014) because their Tab.1 only reports the minimum grid spacing without mentioning the extension of the uniform portion of the mesh that presumably contains the contrail.