Response to referee #1

Response to general comments

Thank you for your insightful comments and suggestions that, we believe, contributed to improve the paper.

We understand your concerns about the choices made for this study. We believe that some of the arguments pointed out in the introduction may give erroneously the impression that our aim is to analyze the properties of turbulence in the UTLS generated by some large-scale forcing resulting from specific atmospheric processes or meteorological events.

In fact, the objective of the study is different: ultimately, we are interested in assessing the atmospheric impact of aircraft emissions, in particular in the form of contrails and contrail-cirrus. The first step of this project consists in reproducing the dispersion of emissions in the free atmosphere. This can be done if we are able to provide some "realistic" fields of atmospheric turbulence using an atmospheric model formulated in the physical space that contains all the necessary microphysics and radiative transfer modules. In addition, we want to be able to control the level of the generated turbulence. This is a crucial point to understand what is the impact and the relative role of turbulent dispersion and radiative transfer in controlling the diffusion of contrails and their life time. If we want to answer this question, we need to perform simulations with different levels of ambient turbulence that we can control over all the simulation time.

For these reasons, the study proposed here aims at evaluating the accuracy of Méso-Nh to generate background atmospheric turbulent in the UTLS, especially in terms of the computational requirements for LES and accuracy of the subgrid-scale model (as you correctly noted). To the best of our knowledge, observations at the scale considered here are extremely rare or inexistent and it is not possible to carry out a thorough validation of the numerical simulations proposed here. Therefore, we refer essentially to results of spectral DNS and theory.

To summarize, to avoid possible confusion in the reader we modified title and abstract of the paper and we reorganized the introduction trying to better clarify the objective of the study.

Response to Specific points

1) It would be nice to see some additional justification for the choice of forcing, especially for its relevance to the UTLS. Have you checked sensitivity of the results to forcing? Are wave and vortical modes both forced? (It is stated that vertical velocity is not forced, but what about horizontal divergence?) Why is the time scale set at 33.6 sec?

The forcing scheme is defined by the size of the spectral shell (in 2D or 3D) of the forced wave numbers and by the variance σ_f^2 and timescale τ_f of the UB process used to build the stochastic acceleration. In the case of isotropic turbulence, it was shown (Eswaran and Pope, 1988, Paoli and Shariff, 2009) that for given turbulence statistics (such as turbulence Reynolds number and dissipation rate) the turbulence spectra and the details of the high-wave number turbulence are independent of the choice of forcing parameters. On the other hand, such turbulence statistics depend on combinations of these parameters rather than the parameters taken separately. For example, the predicted dissipation rate scales as the product of $\sigma_f^2 \tau_f$ and this property has been verified also in the anisotropic case for the present study (see the Fig. A1). Hence, we decided to analyze the sensitivity of turbulence to the forcing scheme by varying σ_f^2 with τ_f kept fixed, which allows to explore different turbulence

intensities (denoted weak, moderate and strong in the paper). The value $\tau_f = 33,6\,s$ was scaled from previous non-dimensional DNS to match our dimensional set-up but the exact value doesn't really matter (once $\sigma_f^2 \tau_f$ has been fixed) except for the numerical integration of UB processes: τ_f has to be sufficiently larger than the time step Δt to avoid excessive random noise, and sufficiently smaller than the larger characteristic time of the flow, estimated as $t_{large} = U^2/\epsilon$ to avoid large-scale drift of the flow. For the present runs, Δt varies between 0,3 and 1s while t_{large} is of the order of $10^4 s$ so that the two conditions are satisfied. We added these details at the end of Sec. 2.1 and the beginning of Sec. 3.3.
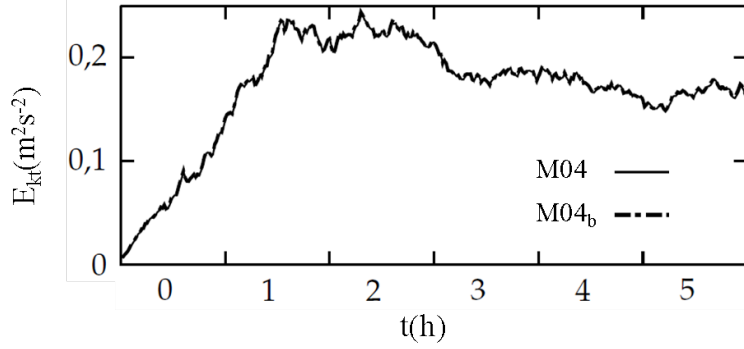

Figure A1: Evolution of resolved kinetic energy for the moderate forcing and 4m resolution. Run M04: original case with $\sigma_f = 1,2.10^{-4} ms^{-2}$, $\tau_f = 33,6\,s$. Run M04$_b$: $\sigma_f = 2,4.10^{-4} ms^{-2}$, $\tau_f = 8,4\,s$ (the two runs have the same $\sigma_f^2 \tau_f = 4,8.10^{-7} m^2 s^{-3}$).

In the present implementation of the scheme both vortical and wave modes are excited. We have mentioned this in the text, at the end of sec. 2.1. The incompressibility condition is guaranteed through a modification of pressure that accounts for the presence of $\boldsymbol{f}$ when solving Poisson equation. Meanwhile, we have been working on a modification of scheme that consists in: (i) having the horizontal divergence of the forcing exactly zero, and (ii) allowing to chose the origin of the forced shell of horizontal mode numbers, for example $n_h = 2$ or 3 instead of 0 as it is the default case now. The preliminary results showed, as expected, that the rms of vertical velocity is a little less wavy and that the spectral energy in the first modes is lower with these modifications (see Fig. A2). However neither of these modifications change the conclusions drawn in the study.
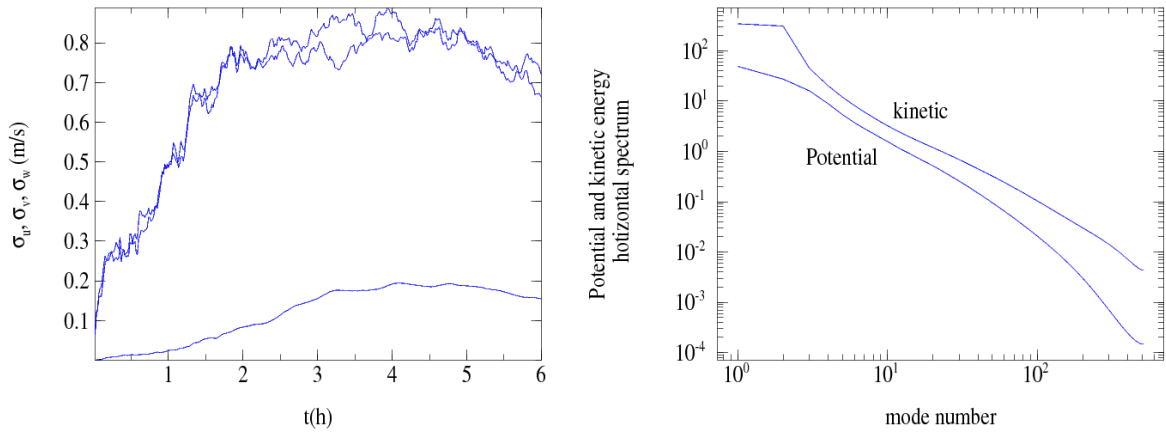

Figure A2: Modified turbulence scheme. Left panel: evolution of rms of horizontal (upper curves) and vertical (lower curve) velocity. Right panel: kinetic and potential energy spectra.

2) *What is the point of including water vapour in these simulations, given that the reference altitude is 11 km and no moist physics is included? Surely the effect of vapour on buoyancy will be negligible.*

We agree that water vapor has negligible effect on turbulence. However, as mentioned above, one of the objectives of the study is to have a tool for generating turbulent flow-fields in the UTLS for the analysis of contrail dispersion. To that end, it is necessary to initialize the simulations with a field of ambient vapor mixing ratio that is consistent with the other dynamic and thermodynamic variables. So, in view of this future work, we prefer to keep the parts concerning water vapor in Sec. 2 and 3.1, but if you find more appropriate to remove the sentence on the initialization of water vapor, we can do it.

3) *In the discussion of $(\bar{u})_h$ in and around equation (20), reference should be made to the vertically sheared horizontal flow (VSHF) modes of Smith Waleffe (2002). Do you find that the energy in these modes grows as in Smith Waleffe?*

We added the reference to Smith and Waleffe, 2002 after the introduction of $(\bar{u})_h$, page 5, as you suggested. We plotted in the new Fig. 12, page 12, the evolution of the energy in the shear modes and observed it grows as it was found in Lindborg, 2006.

4) *On page 31905, it is said to be "interesting" that the resolved KE is independent of resolution. Shouldn't this be expected, since the forcing is at large scales? The following statement that this gives "a posteriori verification that the turbulence model has no or limited impact on mean resolved quantities" seems like an overstatement. All it means is that the energy in the energy containing scales (ie the forcing scales) is independent of the turbulence model, which is to be expected*

We agree so we rephrased the sentence in sec. 3.3, page 6.

5) *It would be helpful to report the Froude number of these simulations for better comparison with previous work*

Done, in Tab. 1 in the revised paper.

6) *The horizontal kinetic energy spectra with strong and moderate forcing are actually consistently shallower than -5/3. This finding should be mentioned and investigated. It would also be helpful to compute the spectral slopes objectively with a least squares fit. Interestingly, it seems to be the weak forcing case that is closest to a -5/3 spectrum. Previous studies (Waite 2011, Augier et al 2013) found a spectral bump at horizontal scales close to the buoyancy scale, which could account for the shallower-than-5/3 spectra reported here - this possibility should be explored. Similarly, the vertical spectra seem consistently shallower than -3. This should be discussed*

Two regimes can be identified in the horizontal spectra of kinetic energy for the high-resolution cases: an inertial range with -5/3 slope at relatively small scales (but above the scales affected by dissipation) and a shallower range above these scales.

The inertial range, denoted by $[k_{hk1},k_{hk2}]$ in the revised paper, was determined using a linear regression over the range of wave numbers with slope equal to -5/3 with a tolerance of plus/minus 0.03. Because the inertial range is established by determining the modes where the -5/3 slope is encountered within this tolerance, we decided to report the values of wave numbers delimiting such range in the new Tab. 2 (together with the computed scaling constants C_{hk}). We clarified this point in Sec 3.5, after Eq. 5.1 in the revised paper. We applied the same method to the spectra of potential energy (Sec. 3.6, page 13).

For wave numbers above the inertial range, we computed the slope using again a linear regression method and obtained -1.45. We added this value on page 12 in the revised paper. We also added the references to the work of Waite (2011) and Augier et al (2012) on the spectral bump on page 12. The origin and explanation of this bump are not clear. As discussed by those authors it can reflect physical mechanisms such as the injection of kinetic energy from nonlinear interactions or be the consequence of the dissipation used in the model (hyperviscosity or sgs kinetic energy here). Hence, given the objectives of the paper, we did not attempt to analyze this point further in the present study. Similar arguments can be applied to the vertical spectra although the range of scales were the -3 slope is supposed to hold (between the buoyancy and Ozmidov scales) is much narrower than the inertial range.

7) For the comparison with observations, I don't think it makes sense to compare the spectra in these simulations with observed spectra at much larger scales. Indeed, why would it be justified to assume "that the spectrum can be extrapolated down to the sub-km scale" as stated on page 31913?. The simulated spectra should be compared with observed spectra at the appropriate scale.

Unfortunately, we have not found observations at the appropriate scale in the literature. For this reason we have principally validated our results with spectral models and theory. Nevertheless, we have opted for a comparison with observations at (more or less) larger scales. The comparison should not be considered as a real validation based on controlled experiences, rather as a verification that the range of values seems acceptable. We added a sentence at the beginning of Sec. 3.7 to avoid confusion on the sense of these comparisons.

Responses to Technical corrections

8) In equation (32): shouldn't you include perturbation buoyancy (not just N^2) in the definition of Ri?

Yes, you are right, we computed the Richardson number using the full potential temperature field, so we corrected eq. (38) in the revised paper.

9) Above equation (37) "Fouriers" should be "Fourier"

Done

Response to referee #2

There is one thing I really miss, and which I think the authors should include in the paper: temperature spectra, or more appropriately temperature spectra converted into available potential energy spectra (APE). The ratio of APE to KE in the $k^{-5/3}$ range should also be given. The results may be compered to the results from other simulations.

Thank you for your comments and suggestion to include the analysis on potential energy, which we believe contributed to improve the paper. We added Sec. 3.2.2 for the definition of new flow statistics and reorganized Sec. 3.3, in particular Fig. 2 that now contains the evolution of the grid-averaged potential energy in addition to kinetic energy. We also wrote a new Sec. 3.6 to present the analysis of potential energy spectra.

Another thing that the authors may consider, is to make a rotational/divergent decompositions of the KE-spectra and compare with previous findings, for example the calculation of LIndborg (JAS, 2007) based on measurements of structure functions.

All the spectra shown in the paper were computed at run time because of the huge memory and CPU resources required to post-process the full 3D dataset generated by Méso-Nh for computational grids of one billion grid points and larger. As a result, we only have stored a few instantaneous solutions for restart. In order to compute the rotational-divergent decomposition we would need to rerun the simulations since we do not have enough samples of the full 3D data to calculate the average of instantaneous spectra. Unfortunately at the present time we do not have computational resources available for these simulations. We took however your suggestion and implemented the spectral decomposition in the code so that it will be available for future LES analysis.