Response to Reviewers

May 28, 2025

We thank the editor and referees for pointing out that some additional information should be included for the wider audience. Reviewer comments are in **bold**, while our responses are in normal text.

Response to Reviewer 3

Model Validation: The response to Comment 1 is not adequately incorporated into the revised manuscript. Aside from a brief addition in Section 3.1, no substantial changes are evident. Furthermore, the limitations of the modeling approach – explicitly raised in the original review – are still not clearly discussed in either the Introduction or the Conclusions.

We added in the manuscript "Furthermore, a sensitivity study was conducted on the supplement to show the necessity for higher vertical and horizontal resolution. The occurrence of high TKE values in EH-84 is more frequent than in EX60 (Figure S9), hence the mixing (Figure 5 and S11). CM10 (Figure 5) also shows more efficient mixing than CM40 (Figure S12) despite the similar TKE frequency."
We also added in the manuscript "the model turbulence kinetic energy (TKE) is compared with a calculated turbulence index using the grid-scale wind data from COSMO, i.e., the turbulence diagnostic TI1 from Ellrod and Knapp (1992), which includes a vertical wind shear term and a deformation (stretching and shearing) term **to examine whether the highly parametrized subgrid scale turbulence scheme is consistent with the grid-scale wind.**"
We also added "It is also important to note that the comparison between the TI1 and TKE is not sufficient enough considering both of them are calculated from the COSMO wind field; comparison with observation should be conducted when there is available data."
We also added in the conclusion"However, further comparison with observation is needed considering both of them are calculated from the COSMO wind field."

Sponge Layer: The revised manuscript should include a clear comment on the validation of the sponge layer depth, as provided in the authors' reply. Currently, this is missing.

We added into the manuscript "Our enhanced setting has 84 levels and reaches also up to 33 km, with an identical 5 km damping layer starting at 27 km **considering Eckstein et al. (2015) shows that the differences between a 5 km and 11 km damping layer is marginally small, and the analysis carried out in this study are far below the damping layer with more than 20 model levels, the potential reflection from the model top should be negligible."**

Simulation Setup: Figures 1 and 2 presented in the reply contain important information but are not discussed or included in the revised manuscript. The paper

should be rewritten to incorporate and discuss these figures.

Section 3.3.1 is added to describe the large-scale meteorology. We also added in the manuscript "The high TKE and Ellrod index shown in Figure 3 and Figure 4 is caused by the jet stream as shown in Figure S18. The increased shear could be attributed to the higher vertical and horizontal resolution, as shown in figure S19. CM10 shows more fine structure and hence more shear." In order not to make the manuscript too lengthy, we have added figures 1 and 2 of the horizontal wind from the first reply into the supplement as figures S18 and S19.

Missing Simulation Details: It remains unclear why the details provided in the authors' reply were not integrated into the revision. In addition, instead of the 2D plot shown in Fig. 3, a vertical profile will be sufficient. Key information is still lacking – regarding the width of the transition layer.

We added in the manuscript "For the same pair of tracers, the only difference is whether the physical process of vertical diffusion (vdiff) is turned on or off. **The vertical diffusion was switched off at the very beginning."** We also added in the manuscript "The tracers change linearly at the transition layer near the tropopause between approximately 300 hPa to 150 hPa. The initial condition of the tracers is shown in figure S20 as a vertical profile." Again, we added the vertical profile into the supplement as figure S20 to ensure the main manuscript isn't overly lengthy.

Analysis of Mixing and Exchange: In their reply, the authors state: "The fluxes would be difficult to compare with the tracer–tracer correlation since it shows an accumulation of tracer due to advection of mixed air from other events." If this is indeed the case, it raises fundamental question:
- **Why are individual events analyzed at all, if they cannot be clearly separated?**
- **Why is this critical limitation not explained in the manuscript?**

In the original review, the reviewer mentioned "Tracer-tracer correlations are used to discuss the direction of troposphere-stratosphere exchange. While this is a valuable diagnostic in observational studies, given the model data, is it not possible to diagnose the diffusive fluxes directly (using the equation on page 4) and confirm the findings from the tracer-tracer correlations?" Tracer-tracer correlations are not used to discuss the direction of troposphere-stratosphere exchange, instead, the delta tracer-tracer correlation is used. The tracer-tracer correlation cannot derive the mixing direction or the mixing from a single event, it shows a background accumulation of the mixing from all kinds of sources over time, therefore, it is not possible to directly compare with the diffusive fluxes from the equation on page 4. Therefore, we introduced the delta tracer-tracer correlation, which can show in the model the direction of the mixing. Since we can switch off the vertical diffusion in the model, we show in cross-section like figure 5 the mixing by the turbulence vertical diffusion only which can be directly related to the delta tracer-tracer correlation. Considering the tracer field of COSMO is initialised and boundary driven from EMAC, which is without an efficient turbulence scheme in the free troposphere, the differences in COSMO originate from turbulent mixing after the tracer enters the domain showing the accumulation of the mixing in the domain before the airmass propagates out of the domain. Consequently, the mixing in the cross-section must be the accumulation of the mixing within several hours, most likely from the same events or at least affected by the same meteorological condition, otherwise it is out of the domain already. So in principle, delta tracer-tracer correlation is trying to show a similar concept as the tracer-tracer correlation, but not an accumulation of all, only the accumulation of a single process. We also added in the manuscript "It is a similar concept as the tracer-tracer correlation, but instead of showing the mixing as an

accumulation of all related processes, it shows the impact of a single process."

Response to Reviewer 2

I wonder whether it would be worthwhile to include somewhat more information, e.g., in the vertical cross sections of horizontal wind. For example, one could show there: the location of the tropopause, some TI1 contour lines, some contour lines from Figure 5c,d of the ozone tracers,... In short, now the wind cross-sections stand quite 'isolated', also the discussion in the text does not go beyond stating whether the cases are or are not near the jet. A few more sentences would be nice that relate more directly the ozone mixing to the jets.

We add the O3-like tracer contour line in the vertical cross sections of the horizontal wind. We have already added in the section describing the synoptic that "Case 1 and 2 (figure 8a and 8b) are also associated with the jet stream, while in case 3 (figure 8c) the jet stream was shifted outside of the CM10 model domain."