

Answer to general comments RC1

(...) The manuscript is generally well structured and the results represent a useful contribution to the interpretation of EMeRGe observations and to the broader understanding of pollution transport over the Po Plain. However, the manuscript is some times highly descriptive and would benefit from some streamlining, with a clearer synthesis of its main scientific advances. I therefore recommend publication after minor revision.

Reply: The manuscript was revised to better highlight the links between the observations described and the paper objectives listed in the introduction. The main scientific findings (NO_x-limited formation of O₃ except in the CPP BL, return vs export of pollution plumes, lack of complete mixing of the boundary layer) described in detail in the discussion were clearly synthesised in the abstract and conclusions.

Specific comments

Line 108: *The manuscript states that one of its objectives is the identification of trans-regional and trans-boundary transport to and from the Po Plain. The presented observations and trajectory analyses clearly document regional recirculation and export towards the Adriatic and Alpine regions. However, direct evidence for cross-national impacts remains limited. It would be helpful if the authors could define more explicitly what is meant by trans-boundary transport in this study and distinguish clearly between regional recirculation and transport with potential cross-border implications.*

Reply: In the revised manuscript a couple of cases of pollution plume transport to neighbouring countries at the ground level have been mentioned and several examples of air pollution recirculation have been given which were evidenced by forward trajectories.

Line 194: *This multi-tool approach is appropriate and commonly used in aircraft campaign analyses. However, the consistency between these tools is discussed mainly qualitatively. In addition, the models are driven by different meteorological datasets and resolutions. A short discussion of uncertainties would strengthen the manuscript, particularly under the weak-wind conditions typical of the Po Plain.*

Reply: The manuscript has been revised to include more information about instrumental uncertainties and comparability. The instrument airborne details provided in a list with quoted literature in Andrés Hernández et al., 2022, are referred to in the revised paper in Line 120.

In addition, the sensitivity study in Fig. S12 of the COSMO-LAGRANTO wind field by changing the starting positions to show the limitation of the COSMO back-trajectories has been enhanced with the comparison of COSMO-LAGRANTO and FLEXTRA trajectories under the weak wind conditions occurring in the Po Plain during the E-EU-06 HALO flight. Following text has been included in Sect. 3.3: “The comparison between 12-hour backward trajectories calculated with LAGRANTO and FLEXTRA, initialized from the aircraft coordinates over the CPP (Fig. S12.1 in the supplement), shows reasonable agreement. However, the agreement deteriorates in the BL between 11-13°E, particularly with respect to the geographical extent of the trajectories. The observed differences may be related to slight discrepancies in the parameterisation of

the vertical wind velocity considered by the models and the specific aircraft altitude. Indeed, in the regions where the discrepancies between the models are largest, the aircraft was flying close to the upper limit of the ABL, where the strongest vertical gradients in horizontal wind velocity are expected (low wind velocities at lower altitudes, high wind velocities higher up). A sensitivity study of the COSMO wind field was carried out by perturbing the starting point of the COSMO-LAGRANTO backward trajectories on 20 July 2017 horizontally within a radius of 2000 m and vertically by ± 200 m around the actual location and altitude of HALO, respectively (see Fig. S12.2 in the supplement)."

Section 3 contains a detailed and comprehensive description of observed plumes and vertical structures. While this level of detail is valuable, the main conceptual conclusions sometimes remain implicit. In several places the text describes individual plume segments without clearly summarising the broader implications.

Reply: The revised manuscript systematically makes the link between direct observations and implications regarding the origin, characteristics, and fate of the pollution plumes.

Line 306-314, 339-349, 408-414: The manuscript uses a combination of trace-gas ratios, aerosol composition indicators, trajectory analysis, and HYSPLIT age estimates to distinguish fresh and aged pollution plumes. It would be helpful to provide a short summary explaining how these indicators were used to identify fresh versus aged air masses for each shuttle.

Reply: In the revised manuscript it has been put special emphasis to discuss for each leg the pieces of information used to understand the nature of the air masses intercepted during the flights. In addition, the lines 235-243 in Section 2.3 have been extended to clarify which indicators were used and how.

Section 3.1: The multi-platform observational approach is a major strength of the study. The manuscript would benefit from a slightly stronger integration of the different datasets in the interpretation, with clearer statements on how airborne, ground-based, and lidar observations jointly support the conclusions.

Reply: Ground level and airborne trace gas measurements were jointly considered to demonstrate the low level of mixing of the boundary layer (Section 3.6). Other conclusions are primarily based on the combination of airborne measurements and model data (simulated CO enhancement, meteorological variable fields, back- and forward- trajectories). Aerosol lidar data could not quantitatively be used beyond the determination of the BL height due i.a. to the long-life time of particles compared to that of reactive trace gases. However, they provided important information about the different layers of aerosols from the BL to the FT, which helped understand the relationships between particle concentrations and trace gas mixing ratios measured from the aircraft (Section 4.4).

Section 3.3: *This section provides important interpretation. Consider adding one or two sentences quantifying or summarising inferred mixing or recirculation timescales where possible. It may be a proper place to clarify how these conclusions relate to previous Po Valley studies.*

Reply: Trace gas measurements as well as meteorological data profiles suggest that the BL was not well mixed on 20 July 2017 before 12:00 UTC, making the estimate of mixing times difficult. In contrast, recirculation and transport time scales are indicated in this section (currently 3.6) at lines 645, 647, 659, 688, and 694.

In Sect. S3 in the supplement a summary of previous studies in the Po Valley is included. The corresponding publications rarely discuss mixing and recirculation timescales. Within TEAMx, microscale flows resulting from boundary layer dynamics in the Alp valleys were investigated and the results shown to be very site dependent. Concerning RI urbans, among the large number of city targets studied, the investigation in Milan focused specifically on the effect of the emissions of the airport Milano Linate in the urban area (Magnani et al., 2024).

Section 3.4 *discusses the presence of an elevated dust layer and its possible influence on atmospheric stability. This section is useful for providing context for the observed layering, but the connection to the main transport and mixing interpretation could be stated more explicitly. A brief clarification of how the dust layer may have influenced boundary-layer mixing, vertical exchange, or plume persistence during the analysed flights would help integrate this section more closely with the main conclusions of the paper.*

Reply: We thank the referee for raising this point that allowed us to clarify this aspect in the revised section 3.4 (now 3.7). In fact, on July 20 the atmospheric pressure field over Italy revealed that the desert dust transport was associated with typical anticyclonic conditions, quite frequent during warmer months (as already mentioned in the Introduction, Lines 50-53). As the aerosol optical depth of the dust layer indicates, the desert dust itself was not sufficient to induce stability, but it was associated with the high-pressure system, which provided stable, subsiding air.

This issue has been clarified in this section 3.7 (formerly 3.4) by rephrasing the text:

“In fact, on July 20, 2017, vertical mixing was not favoured by the high pressure field extending from North Africa to northern Italy. Model-based data indeed show the gradient of the w temperature to be neutral below 1000-1500 m during the HALO overpass above the CPP, whereas it was positive above 1500 m (Figure S10.5 in the supplement. Thus, the atmospheric layering was very stable in the FT, likely inducing minor or no mixing of dust within the boundary layer.”

And removing the statement:

“The slow vertical mixing and low photochemical activity observed over the CPP on 20 July 2017 may be related to the presence of Saharan dust.”

Answer to general comments RC2

(...) Lines 230-266 (incl Table 1, Figures 4 and 5): deserve their own results section, possibly titled "3.1 Boundary layer meteorology during flights E-EU-03 and E-EU-06" Arguably, Lines 268-282 are still describing methodology rather than presenting results, so should go in a new Section 2.3 possibly titled "2.3 Analysis approach for pollutant distribution and gradients within the BL during flight E-EU-06"

Reply: Section 3.1 and 2.3 have been included in the manuscript as suggested by the referee.

The current sections 3.1.1, 3.1.2 and 3.1.3 provide the bulk of the observational data set analysis (11 pages), whereas subsequent sections 3.2, 3.3 and 3.4 are much shorter (7 pages in total). For better clarity and better structure of paragraphs, I suggest renaming sections as follows:

3.1 Boundary layer meteorology during flights E-EU-03 and E-EU-06 [lines 230-266 incl Table 1, Figures 4 and 5 as suggested above]

3.2 Vertical distribution and advection of pollutants in the Gulf of Venice (GV) [line 290]

3.2.1 Shuttle BC along Croatian coast [line 302]

3.2.2 Shuttle FG in Gulf of Venice [line 320]

3.2. MBL transect BF across Adriatic Sea [line 404]

3.3 Distribution of pollutants in the BL across the central Po Plain (CPP) [line 430]

3.4 Vertical distribution and advection of pollutants in the Gulf of Genoa (GG) [line 508]

3.5 Comparison of ground and airborne observations over the Po Plain [line 530]

3.6 General features in the processing of the air masses over the Po Plain [Line 574]

3.7 Desert dust transport and secondary organic aerosol formation above the central Po Plain [Line 636]

Reply: The sections of the manuscript have been renamed and renumbered as suggested by the referee above. A new section has been included: "3.2.3 Comparison of the two shuttles across the GV" and the following sections were renumbered accordingly:

(..) Some of the referencing and stated results do not match observational or computational evidence, or figures referencing is inconsistent, which I have highlighted below and will require correction or explanations. Main manuscript:

Line 93: *"...of the local meteorology and orography" [instead of "dynamic"; orography as study of mountains' topography effects].*

Reply: This has been changed as suggested by the referee.

Line 190 *(caption of Fig3) AQM... stations are identified by their station number (tabulated in Supplementary Table S6.1).*

Reply: This has been added in the figure caption as suggested by the referee.

Line 171 *(section 2.1.3 Met soundings) In Fig 3, San Pietro Capofiume is listed as a radiosonde station, but is not listed in the main 3 text or in Table 1 of Section Results and discussion.*

Reply: San Pietro Capofiume is a radiosonde station but there were no data available for the period of study and therefore could not be considered in the discussion of results.

Line 203 b) COSMO: high resolution wind fields [strengthens the need for this model]

"The COSMO-Model (...) offers a factor ten higher spatial resolution than ERA5, so is employed in this study..."

Reply: The term "high resolution" has been included in the title of b) as suggested by the referee and the following sentences in the manuscript have been rephrased to emphasise the added value of the COSMO simulations.

Line 218 Marenco et al and Birmili et al merely use HYSPLIT. Please cite Draxler, R. and Hess, G.: Description of the HYSPLIT4 modeling system, NOAA Technical Memorandum, ERL, ARL-224, revised April 2020, and "NOAA's HYSPLIT Atmospheric Transport and Dispersion Modeling System", Bulletin of the American Meteorological Society 96, 12 (2015): 2059-2077, accessed Oct 7, 2021, <https://doi.org/10.1175/BAMSD-14-00110.1>.

Reply: The suggested references have been included.

Line 226 correct URL for EDGAR HTAP V2 is https://edgar.jrc.ec.europa.eu/dataset_htap_v2.

Reply: The URL has been corrected

Line 228 If possible, could you provide a doi or GitHub reference for the DLR/IPA infrastructure?

Reply: We agree that a Doi or GitHub reference would be helpful. Unfortunately, the colleague who did this work has retired and is no longer available.

Line 237 "JNO2 was used as a qualitative proxy metric for cloudiness..."

Reply: The sentence has been modified as suggested by the referee.

Line 249 "...Gulf of Genoa, which contrasted with speeds ranging -2 to +4 m s⁻¹..." [I assume that this statement was meant to contrast low variability and speed over the sea versus the more turbulent winds over the CPP].

Reply: This assumption is right and the corresponding sentence (in Line 271 in the revised version) has been extended as suggested by the referee.

Line 254 "In Fig. 5, the wind fields calculated by COSMO for..."

Reply: The sentence has been modified as suggested by the referee.

Line 256 "Inside the BL (<1000 m altitude), sea breeze regimes are..."

Reply: The sentence has been modified as suggested by the referee.

Lines 257-258 "... with altitude over the Gulf of Genoa coastline (43.5-45° N; 8-10° E) and sea to land circulation up to approximately 600 m altitude over the Gulf of Venice coastline (44-46° N; 12-14° E)." [consistent geographical references as used in Fig 5 caption].

Reply: The sentence has been corrected as suggested by the referee.

Line 260 "... following the dominant pattern previously observed over the ...".

Reply: The sentence has been modified as suggested by the referee

Line 291 *"Figure 6 shows the spatial variation of ..." [although the observations are presented as timeseries, their variability is spatial].*

Reply: The sentence has been corrected as suggested by the referee.

Line 300 *"3)" refers to new section 3.2.3. Could FT leg 6-6a averages be added in Supplementary Table S8 to support the HCHO/NO₂ ratios discussion on lines 312-314?*

Reply: The reference has been updated and the Table S8 has been extended with the 6-6a averages as suggested by the referee.

Line 307 *"...of FT air masses (> 2000 m) at 3220 m..." The 12 UTC radiosonde sounding at Rivotto suggests a BLH of 1000 +/- 70 m according to Table 1, which may suggest that the 6b-6c leg at 1630 m altitude may be FT-influenced, possibly by BL/FT entrainment zone. The proximity of the Croatian coast to the "B-C shuttle" may have contributed to this entrainment, as hinted by the easterly veering of winds at 1000 and 1600 m shown in Figure 5.*

Reply: Measured and reanalysed meteorological data profiles (Fig. S10.3 and S10.4) indicate that at 1630 m asl the aircraft flew in the FT indeed, and at the top of the entrainment zone. There is no evidence from transport of pollutants to the aircraft altitude from aloft. In contrast, backward trajectories suggest that the air masses intercepted at 1630 m asl were lifted up from the surface of the CPP during the afternoon the day before.

Lines 315-318 *for a better reference for emissions from north industrial area of Porto Marghera in Venetto Lagoon, especially SO₂ monitored at Station Enel Fusina, see https://apice-project.eu/img_web/pagine/files/Results/Apice_WP3%20Report.pdf*

Reply: This report is no longer available but a similar adequate reference is the paper Toscano, D. The Impact of Shipping on Air Quality in the Port Cities of the Mediterranean Area: A Review. Atmosphere 2023, 14, 1180. <https://doi.org/10.3390/atmos14071180>, which has been included in the text and reference list.

Line 318 *"... from the sea surface to 520 m, 10-16 hours before the measurements, as shown on the left-hand panel in Fig. 9."*

Reply: The sentence has been extended as suggested by the referee.

Line 321 *"Figure 6: Spatial variation of NO, ..."*

Reply: The figure caption has been extended as "temporal and spatial variation of..." since some of the places are overflowed twice, as indicated by the shaded areas.

Lines 329-328 *comments on Fig 6 and its caption: The colour shading scheme complicates Fig 6 significantly and I would advise against it. Certainly the darker/lighter shading is unnecessary given that altitude is charted in the top plot of Fig 6. If shading must be kept to better highlight legs on individual time charts, I'd recommend a light neutral grey shading. I also wonder whether the top chart would benefit from geolocations B, C, F, G and X to be added alongside waypoints 6-6I, so the reader can better identify the B-C, X-F and GX legs discussed in the main text*

Reply: The shading helps to emphasise the part of the leg directly flown on the reverse, i.e., the temporal closeness of the vertical profile over the same area, what is not always obvious. They are actually indicating the geolocations B, C, F, and G. There are areas flown at the same altitude, which do not belong to the spatial area covered

by the shuttle. This visualisation is not possible if the shading is all in grey or if the darker/lighter shading is not included. The plot has been slightly modified to improve clarity. The geolocations have been additionally included on the top as suggested by the referee.

Line 326 "(see Figure 13)". Not sure this reference is correct; presumably should be Fig 3?

Reply: Yes, this is indeed Fig.3 and it has been corrected.

Lines 333-334 see above comment about shaded areas in Fig 6

Reply: Please see the answer above for the comment in Lines 329-328.

Line 317-318 "Backward trajectories indicate that air masses were lifted up from the sea surface to 520 m, 10-16 hours before the measurements, as showed in Figure 9 left hand panel."

Reply: The sentence has been extended as suggested by the referee in the comment for Line 318.

Line 348 "... it is less water-soluble and its rate of removal by reaction with OH is much slower than that of SO₂".

Reply: The sentence has been corrected as suggested by the referee.

Line 372-373 HCHO/NO₂ values are not provided in Figure 8 nor in Table S8c. NO₂ data are not provided in Fig 8 either. "... being on average a factor of ~3 to 4 higher than that in the BL (see Table S8).

Reply: The HCHO/NO₂ and SO₂/CO values have been included in the table and the sentence has been corrected according to the table.

Line 383-386 No RH or H₂O mole fractions are presented in any of the observed meteorology data shown so far, such as in the new Section 3.1 which focuses mainly on winds. Do radiosonde soundings support this statement?

Reply: RH and water vapor profiles from the radiosonde launched on 20 July 2017 at 12 UTC from Rivolto, as well as RH vertical profiles along both the B-C and F-G tracks above GV generated the EMEP-MSC-W model for 20 July 2017 at 9 UTC are provided in the Sect. S10 in the supplement. Both the measurement and model data show an increase in RH and water vapor mixing ratio around 1600 m above the GV surface.

Line 392 change "BB" to "biomass burning". A more definitive BB marker such as acetonitrile CH₃CN was measured by HALO's PTR-MS (Förster et al 2023, <https://doi.org/10.5194/acp-23-1893-2023>) to substantially indicate fresh wildfires in the Mediterranean area. BC particles microphysics were also reported by Andrés Hernández et al 2022 to illustrate the complex mixing of BB and anthropogenic emission sources.

Reply: We have revised the PTR-MS measurements of acetonitrile during the HALO flight and the observations do not support the statement above. The text has been modified to clarify this point:

"Interestingly, the HYSPLIT simulations do not capture the increase in CO observed at 1630 m between G and X, which may have a fresh anthropogenic origin given the increase in benzene and HCHO mixing ratios as measured by PTR-MS on board (Förster et al, 2023). In contrast, the biomass burning marker acetonitrile CH₃CN did

not show simultaneous enhancements despite the number of active wildfires at the time of the measurements.

Line 396 *Millán et al 2004 reference is missing. Similar land-sea breeze coastal shallow circulations were reported in southern West Africa (Flamant et al 2018, <https://doi.org/10.5194/acp-18-12363-2018>).*

Reply: The references and the sentence have been included as suggested by the referee.

Line 412 "(see Section 3.3)" ensure correct section ref.

Reply: The Section is now 3.5 and has been correctly re-numbered.

Lines 410-413 *The FLEXTRA backward trajectories on the RH panel in Figure 9 (10:12-10:27 UTC, 520 m, 6e-6f and part of 6f-6g) do not substantiate the statement "...of air masses circulating below 500 m agl over the CPP between approximately 45.2°N – 45.8°N, and 10.4°E – 11.6°E, an area including Brescia and Verona urban areas". Air masses appear to be advected south of 45°N, between 10°E – 12°E, encompassing Bologna-Modena-Parma from the Apennine Mountains, rather than as stated from NW Veneto. Brescia and Verona, what I do not think is NW Veneto*

Reply: The description of these back-trajectories was indeed erroneous and was corrected in the revised manuscript as "air masses originating from the surface in the CPP between approximately 44.0°N-44.6°N, and 10.0°N-12.5°E, a densely populated area (Figure S1) including Modena, Bologna, and one of the busiest highways in Italy".

Lines 421-422 *"...A significant contribution to air pollution from shipping and industrial activities around the ports of Venice and Marghera is consistent with..." [Porto Marghera should be referred to as it is by far the biggest industrial emitter in the Venetian Lagoon].*

Reply: The sentence has been extended as suggested by the referee.

Line 426 *Figure 9 caption would benefit quoting back traj origin, eg 6d-6e (B-C) transect at 520 m (LHS), and 6e-6f (B-F) transect at 520 m (RHS)*

Reply: The Figure 9 has been changed to emphasise the back-trajectories origin in the shuttles.

Line 443 *Figure 10 caption. These are twenty fours FLEXTRA backward trajectories assuming the dots mark 6h time steps (same as Fig. 9).*

Reply: These are indeed 24h FLEXTRA trajectories and the figure caption has been corrected.

Line 448 *"... could not be safely qualified as NO_x- nor VOC-limited."*

Reply: The sentence has been modified as suggested by the referee.

Line 461-464 *"(44.5°N-45.5°N, 10°E-11°E)" ["10°N" is a typo].*

Reply: The typo has been corrected.

Figure 10 does not evidence air mass advection below 500 m during previous 24 hours. Between 10°-11°E, the cyan coloured trajectories indicate heights > 1000 m. However since the Milano Linate derived BLH is 1600+/-400 m (Table 1), one can argue that air masses remained in the BL during their slow advection.

Reply: Mass advection below 500 m is evidenced at least for the first 2 backward trajectories (11:30-11:31h) corresponding to the period P1 described in this part of the text (Line 499 of the revised manuscript). The sentence has been modified for clarification: "Backward trajectories suggest that the air mass sampled during P1 originated from the south and traveled within the BL below 500 m in the prior 12 to 18 hours (Fig. 10)."

Line 486 "Figure 11: Spatial variation of NO_x,...". Since CO is such an important tracer in this study, could the observed CO blue trace be overlaid on top of the HYSPLIT simulated deltaCO on the bottom time chart? The same comment applies to Figure 6.

Reply: The Figures 6 and 11 have been modified as suggested by the referee.

Line 520-521 "This means that on 20 July 2017 the CPP area did not appear to be a source of pollution in the FT above 2500 m asl."

Reply: The sentence has been included as suggested by the referee.

Line 529 Caption of Figure 12. "The delimiting times are marked in Fig. 11 as red lines in the bottom panel" [Fig. 10 incorrectly referred]

Reply: The figure caption has been corrected.

Line 525: Arguably this extra LR transported CO could be from forest fires in southeastern France which was experiencing an exceptionally hot and dry summer in 2017, which were later detected during HALO E-EU-07 and E-EU-09 flights downwind of Marseille, Nice and Saint Tropez on 24 and 28 July 2017 (Andrés Hernández et al 2022).

Reply: We thank the referee for the suggestion. The text has been accordingly extended.

Line 531 PP is undefined in this main manuscript. Keep to previous used CPP acronym? [see suppl comment].

Reply: This acronym has been replaced by "Po Plain" for clarity in the manuscript and supplement.

Line 530 Section 3.2 Comparison of...I feel that the comparison presented in this section is overly simplified and does not illustrate the Eulerian and Lagrangian nature of respectively ground-based and aircraft observations. How were hourly concentrations at ground sites time selected, given that some of these sites are located up to 18 to 24 hours advection time from the HALO flight track according to the FLEXTRA backward trajectory simulations? These advection times are short enough for long-lived CO (~1 month), but much closer to the NO_x and O₃ lifetimes (hours-day). Some ground sites considered in the comparison do fall outside the backward trajectories lat/long domain when inspecting Figures 10 and 13, for instance sites 8, 9, 10, 11, 17, 18, 19, 23.

For NO_x and O₃, you quote 11:30-12:00 UTC and 11:00-12:00 UTC for the 1h-mean mixing ratios at ground stations (lines 565 and 568), which suggest you did not consider advection times; I admit that day-to-day variability during the study period maybe be low, however this should be stated somehow. O₃ mixing ratios measured by HALO over the CPP at 800 m being

8% higher than at ground level is explained by the possible result of O₃ photochemical production in convected air masses or poor mixing of NO_x emissions on the ground (lines 571-573). Higher O₃ deposition velocities near the ground and associated O₃ losses would also affect net O₃ production efficiencies at ground level, and could also explain the observed discrepancy.

Reply: The HALO flight E-EU-06 was not intended to follow pollution plumes in a Lagrangian approach. The section comparing “ground and airborne observations over the Po Plain” was revised to better highlight that in the conditions of neutral thermal stability and low wind speed that are common in the Po Plain, and which were well documented for the 20 July 2017 (Section S10 in the supplement), the BL is not well mixed and strong negative gradients for primary pollutants emitted at the surface can occur. In particular, we modified the sentence in lines 611-615 as “The significantly lower NO and NO₂ mixing ratios measured on board HALO than at the ground around 11:30 – 12:00 UTC on 20 July 2017 (Figure 13) suggest a decoupling between the ground and the aircraft altitude (~800 m agl).”.

Line 549-551 *"This might be explained by the large contribution of long-range transported and long-lived CO to the BL mixing ratios at 800 m above the ground. According to HYSPLIT simulations, emissions from the Po Valley actually accounted at the maximum for 15 % (~20 ppb) to the BL CO mixing ratios measured on-board the aircraft (~130 ppb). " [quoting max delta CO and mean BL CO values] Your CO comparison conclusion sounds reasonable.*

Reply: The sentence has been extended for clarification according to the comments made by the referee.

Line 552 *30-60 ppbv NO_x are not "extreme concentrations", they are fairly common for cities curbside monitoring stations...*

Reply: “extreme” has been replaced by “high”.

Lines 560-564 *"Based on the mean vertical... (e.g. Maclean et al., 2017; Helbig et al, 2021)". I'm afraid I do not understand the statement "the decrease in NO_x mixing ratio between the ground and the aircraft altitude could be estimated to a factor of ~3 only." The two references provided ("Maclean et al., 2017; Helbig et al, 2021") do not support the statement "upper limit of the range for a convective BL between 20-30 min to a few hours".*

Reply: The factor ~3 estimate was coming from a simple 1-D steady state transport calculation: $800/0.07/3600 = 3.2$ hr, $[\text{NO}_x]_{\text{HALO}}/[\text{NO}_x]_{\text{ground}} = \exp(-3.2/3) = 0.35$, and $1/0.35=2.9$. The estimate 4-6 hr came from $H/0.07$, with H ranging from 1000 to 1500 m.

Maclean et al. (2017) quoted (Wallace and Hobbs, 2006) when stating that vertical mixing of air masses in the PBL is rapid, on the order of 30 min. Seibert et al., 2000 refers that it can take hours to get a full equilibration of the BL.

However, since this estimation was unclear to the referee, and not essential, it was substituted by “Indeed, a 1-D model simulating correctly the ground and aircraft level NO_x mixing ratios, as well as the HNO₃ mixing ratio at the aircraft altitude, leads to a BL mixing time of ~30 hrs, i.e. inconsistent with the order of magnitude (30 min) stated by Wallace and Hobbs (2006). Back-trajectories also suggest that the air probed at 830 asl over the CPP left the ground at least ~24 hrs before (Fig. 9).”

Line 570 "...NO_x and VOC have mixed upwind with biogenic VOC..." [rather than "upstream"]

Reply: Upstream has been replaced by upwind as suggested by the referee

Line 584 "...with respect to the GV." [full stop missing]

Reply: The full stop has been added.

Line 604 "..., but also by an effective oxidation of NO_x-rich emissions..." [rather than "processing"]

Reply: Although oxidation is likely dominant in the processing of the airmass, getting nitrate ions needs something more than oxidation only of NH₄⁺, or deliquescent particles. Therefore, the sentence has been modified as "... also by oxidation and subsequent processing of NO_x rich emissions (...)"

Line 610 "...measurements points at the western tip of the CPP..." [rather than "west"]

Reply: The sentence has been modified as suggested by the referee.

Line 615 "... and to a lesser extent O₃."

Reply: The sentence has been modified as suggested by the referee.

Line 624-625 "However measured and simulated radiosondes vertical soundings in this study indicate that turbulence in the BL atmosphere over the CPP was much less than in the simulations of Wang et al. (2022)." [Wang's study in the vicinity of Hong Kong Island was subject to strong turbulent flow which was not the case for the CPP study]

Reply: The sentence has been modified for clarification as suggested by the referee.

Line 626 Hůnová et al. (2023) reference missing.

Reply: The reference has been included.

Line 660 "In particular, elevated aerosol layers up to 5000 m altitude are visible in the ALC and lidar traces (Fig. 14-15)". [delete "over"]

Reply: The sentence has been modified as suggested by the referee.

Line 643 "... if they are entrained back inside the BL."

Reply: The sentence has been modified as suggested by the referee.

Supplement S3 : you introduce the acronym PP without defining it (Po Plain), and refer to PP in the supplementary info. However in the main manuscript you refer to CPP (central Po Plain) in section 3.1.2, and reference S9. For consistency, could you use a single acronym?

Reply: The acronym has been deleted and replaced by "Po Plain" all over the supplement