

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) a 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 this in 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.”