

Answer to Reviewer 2:

This study aims to understand chemical and meteorological processes occurring in the boundary layer during summer rain events, by using a combination of drone-based measurements, ground-based observations, and ICON forecast model data. The aim of the work is novel and interesting, yet the structure and the methodology suffer from a number of drawbacks which limit the impact and the full potential of exploitation for the work. Below I report my major comments based on my revision of the paper.

Dear Reviewer 2, thank you for your comprehensive feedback and critics. We revised the manuscript and have changed several sections to lower concerns and improve the understanding of the concept and the results.

General comments

The structure of the Introduction section is chaotic and messy. It starts from understanding atmospheric processes during different weather events, then begins to talk about fronts, then of models and measurements, then again of atmospheric dynamics, Monin-Obukhov theory (MOST), drone-based observations, fronts, ... Therefore, I suggest improving the logic of the Introduction removing multiple jumps from one topic to the other, and more clearly demonstrating the gaps present in the literature and the key research questions addressed in the current work.

Thank you for letting us know that the introduction appears to be not as clear as desired. An improved version of the introduction does not contain MOST and first focuses on boundary layer dynamics and the difficulties to characterize them before moving towards measurement approaches.

Additionally, we added the major gaps in the literature to motivate the research goal of investigating the influence of rain events on the chemical composition of the PBL:

“While model data lack the high spatial resolution in the planetary boundary layer (PBL, Lochbihler et al., 2021), in-situ measurements are usually limited to specific meteorological variables, while chemical markers and frontal influence on the air mass’ chemical composition is neglected. Large data sets of local data cover underlying dynamics of different rain fronts, however, they often do not cover a sufficient vertical range (Bopape et al., 2021; Helbig et al., 2021; Wang et al., 2021) or focus on different ecosystems such as tropical rainforest (Machado et al., 2024b; Machado et al., 2024a). The influence of entrainment at the PBL top on the chemical composition has been examined (Pohorsky et al., 2025; Platis et al., 2016), but, to our knowledge, detailed analyses that couple PBL dynamics with precipitating frontal passages under midlatitude continental-plain conditions are scarce.”

Although not directly described in the Abstract, the methodology seems to be based not only on the ICON model, but also on HySplit back-trajectories driven by 0.25° GFS analysis data for calculation of boundary layer height. The use of back-trajectories in such complex terrain and driven by such coarse resolution meteorology is substantially problematic, as it is extremely difficult for such a meteorological model to successfully catch the micrometeorological and mesoscale processes occurring in the study site. In addition, it is not clear why you need this estimation when you have ceilometer data and higher resolution models. Last but not least, it is not clear why you restrict the focus to 24 hours only trajectories.

In addition to the HySplit trajectories we also calculated trajectories from ICON-D2 high resolution meteorological fields. However, given the vicinity of the measurement site to the borders of the model domain of ICON-D2, those back trajectories often did not provide information beyond a few hours before arrival at the measurement site. Nevertheless, a high degree of consistency between the ICON-D2 and the HySplit trajectories have been found for the overlapping domain.

The following statement was added:

*“Besides the HySplit trajectories driven by 0.25°-GFS analysis data, ICON-D2 analysis and hourly forecast data for the gaps between the analysis time events (3 hourly) were utilised to calculate corresponding MLHs, backward trajectories within the German domain **and to validate the coarser HySplit trajectories** (Figs. S3 and S4).”*

I tend to agree with the other reviewer highlighting the relevant number of figures in the Supplementary, which also seem to be rather fundamental as per their discussion in the manuscript (in addition, with an odd and not consequential numbering). Figures in the Supplementary Material can support the findings, but should not contain relevant information that is fundamental to the discussion of the results.

Thank you for noting this, too. We have reconsidered the number and selection of figures in the manuscript and supplement to include the figures which are most crucial into the manuscript, whereas the supplementary material mainly contains figures that rather serve as a reference or do not contribute that much.

We merged the previous figures 1 and S6, S9 and S10, and included S14 in the main text as several sections in the manuscript refer to them.

Section 5 only contains a summary of the findings, while an inclusion of the conclusions, limitations and next steps or outlook could be rather beneficial for improving the impact of the work over the long-time horizon.

The major limitation was the low number of two analyzed cases, the absence of in-rain flights and the resolution of hourly profiles that could be improved in future studies. This is now mentioned in the manuscript in the second last paragraph of the summary:

*“**Despite being limited to two cases with hourly resolution and no in-rain flights**, the study shows that combined model- and ground-based analysis can conceptually identify complex boundary layer phenomena like pollutant sinks and local stratification.”*

We included more detailed suggestions for future studies and implications in the final paragraph of the study:

“Findings of this study are only based on two in-detail analyzed case studies, depicting different types of fronts. Further field studies with similar vertical resolution at different locations with varying surfaces like in flatland, alpine mountains or cities could help to better characterize frontal dynamics. Reanalysis of MLHs with ICON for comparable cold pool scenarios as in Sect. 4 could allow finding a parameterization to identify and predict cold pools solely on a model basis, which is important as the occurrence of cold pools is expected to increase with climate warming in the mid-latitudes (Borquez et al., 2020). This could also help to estimate the MLH more precisely under strongly unstable conditions.

Recorded BVOC data from field campaigns should be reanalyzed regarding O₃ depletion after rain above grassland, where vertical O₃ measurements were available.”

Specific comments

Lines 9-11: I doubt there is such a clear division between research in polluted area and at continental rural mid-latitude sites. In addition, this is for sure out of scope for the beginning of the abstract.

We wanted to emphasize the setting of previous studies regarding its geographical region and as it is expected to influence boundary layer dynamics differently. Lines 9-11 were rephrased to motivate the research gap differently in a short sentence.

“Understanding atmospheric processes is essential for improving weather forecasts and models, but in continental regions the effects of dynamical mixing and frontal events on the chemical composition of the planetary boundary layer remain poorly characterized.”

Lines 20-22: What does this mean more in general for boundary layer dynamics?

Local small scale cold pool formation is an uncertainty for the model simulations even though the general feature can be reasonably represented by the ICON-D2 model. However, the extent and intensity of these cold pools, especially after heavy rainfalls, and their effects provide opportunities for improved PBL parameterization schemes to simulate the MLH accurately.

In the last paragraph of the abstract the following general implication of the analysis of such unstable conditions was included:

“Analysis of these events help to reduce uncertainty of model simulations and enhance the PBL parameterization scheme.”

Lines 90-139: Given the high number of data sources, perhaps having a Table with all data sources and key information extracted, as well as resolution, ... could be helpful for the reader.

Thank you for pointing this out. The information for experimental and model data was included into Table A1 and A2 in the Appendix A.

Lines 150-152: this note is not in the appropriate location.

This note was shifted to the beginning of section 3.

Lines 158-160: Not clear what is the assumption if this is something you are observing in your results. Maybe it is a speculative result?

You are right, the message was not as clear as desired. We rewrote the sentence to clearly distinguish between observation, expectation and result:

“A strong upper free tropospheric layer (I) that suppresses a trajectory uplift is shown by the local scale observation of an immediate but short-lived dynamical stabilization above the lowermost layer and its hardly changed conditions below. Despite the missing updraft transport, the rain events can indeed be described as a warm front (see Sections 3.2 and 3.3.1).”

Lines 163-167: The caption contains material that is rather for the main text.

This figure was extended with additional graphs from the Supporting Information. The interpretation

of the data given in the figure caption of the previous version was now moved to the main text and the figure caption was shortened to avoid redundancy.

Lines 173-175: Rather, the high correlation indicates the presence of the same temporal pattern, which is quite obvious and expected, but does not demonstrate the existence of a cause-effect relationship.

You are right, that the high correlation between irradiance and the sensible heat flux is expected, however, to us the irradiance-influenced sensible heat flux is plausible. We have therefore opted for a more restrained wording:

*“The strong correlation between irradiance and sensible heat flux throughout the day (Pearson correlation coefficient $r = 0.87$) **suggests** that sensible heat flux is generated mainly by direct irradiance and **probably** not driven by stored energy from the ground (Fig 1).”*

Line 222: Change “Contrary” to “Conversely”

Done.

Lines 229-232 and lines 309-311: Suggest rephrasing this sentence to highlight the findings and the eventual agreement with previous studies. The current structure does not reflect what is original in the observations, while it highlights more the previous studies.

For 229-232: *Lambe et al. (2011)* was included to demonstrate that an increasing O/C-ratio in the aerosol composition can essentially originate from an O₃ increase. The finding that delayed BL break-up alters photochemical aging is given right afterwards as consequence in context with the study:

“In addition to the photochemical production of O₃, mixing with free tropospheric air also causes a rapid increase of O₃ at ground level (Neuman et al., 2012), potentially causing photochemical aging of the aerosol (Lambe et al., 2011), as suggested by the strong correlation ($r = 0.94$) that we found for the time series of O₃ levels and the O/C ratio of organic particulate aerosol measured with the ground-based MoLa (Fig. S8). This strong correlation of O₃ with aerosol aging and the observed stratification pattern show that a delayed breakup of the NBL can also lead to a delay in the onset of diurnal chemistry. An inflow of already-aged aerosols as a reason for enhanced O/C ratios is unlikely due to the stability of the PM₁ concentration measured at ground.”

For 309-311: Two further studies, which suggest cold pool dynamics (*Lochbihler et al., 2021; Haerter and Schlemmer, 2018*), were cited and the fact that vertical profiles prove their assumptions and compliment the (Tompkins, 2001) study is emphasized:

“Simulations of cold-pool dynamics indicate that, under continental conditions in the mid latitude, strong latent heat flux can increase convective mass flux (Lochbihler et al., 2021). Consistent with this, (Tompkins, 2001) reported increases in equivalent potential temperature along recovered cold pools from ground measurements. In our case (Figs. 5b, S14), following the rain events at 13:30 and 14:45, we observe increases in equivalent potential temperature of ~6 K within an hour at the surface for each event, and aloft by 2.1 K and >3 K, respectively, at the sampled altitude levels up to 500 m. These high-vertical-resolution observations between 13:30 and 14:45 support the model-based hypothesis by Lochbihler et al. (2021) and Haerter and Schlemmer (2018) that strong latent heat flux

and associated moist updrafts can promote the formation of new convective cells like the upcoming rain front for our case study.”

Lines 262-264: This sentence needs to be rephrased as it is not clear.

Thank you for this hint. The sentence was rephrased for clarification:

“The influence of thunderstorms on the pollutant distribution and air mass dynamics in the lower troposphere was investigated by vertical profiling. Throughout a day when a thunderstorm occurred at the measurement site eleven FLab-based measurements were conducted hourly, as long as weather conditions allowed for safe operation.”

Line 275: What do you mean by “aspiration of air masses”? (this term is also used at line 232 and 87, but it is not very technical).

The term “aspiration” was changed to “inflow” or “air mass inflow” for better clarity.

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