Answer to Reviewer 1:

The authors wrote a case-study based paper about how the dynamics and chemical composition of the boundary-layer changes after frontal passages. They evaluate a warm front and cold front using observations (e.g. surface-based and drone) and numerical weather prediction models. The paper itself was clearly written, and the topic is relevant as I am not aware of many studies that look to the ABL during periods of precipitation. I appreciate how the authors incorporated both observation a land model data. That being said, I have a number of suggestions and comments that I think should be addressed before publication. Most importantly, I think the evidence that supports important conclusions from this manuscript are not clearly evident to me. It is likely a combination of the figures selected and the order of the text.

Dear Reviewer 1, thank you very much for the review and feedback on our manuscript. To clarify the argumentation and structure of the manuscript, the overview figures Fig. 4 and 6 and the related text are now introduced in the beginning of the respective case study section, which enhances the understanding of the context and the more detailed results in this study.

Major Comments:

1. There is too much material in the supplemental material. In my opinion, some of figures that are in the supplemental material are essential for supporting the conclusions that are drawn from the paper. The manuscript relies heavily on them. Those figures either should be included in the paper, or the results section should be rewritten so that the supplemental figures are not relied on as heavily as figures in the paper.

   Thank you for noting this. We have decided to include Fig. S6 in the main text to support argumentation of Sect. 3.1, same fore Fig. S9 and S10 and Sect. 4.1. As argumentation in Sect. 4.5 relies heavily on Fig. S14, it was included in the main manuscript as well.

2. ABL Terminology and Methodology.
   a. Lines 130-135: how do you calculation ABL height from the observations and the model? Are they the same method? Explain this more, and discuss (somewhere) the effect of the definition of ABL height on your results. For example, ABL height very much depends on the method (e.g., Seidel et al, 2010).
      i. There are some citations (or justification) missing here. Where do these thresholds come from? Why would Brunt-Vaisala frequency be used for mixing layer height?
   b. b. How do you compute it from observations?
      i. From reading the manuscript, I get the impression that the terms NBL, MHL and PBL are used somewhat interchangeably. They are all different concepts, and in the case of MHL and NBL, they cannot co-exist. A mixing layer implies a convective ABL and a nocturnal ABL typically wouldn't have a mixed layer to my knowledge. Please revise the terminology accordingly.

Thank you for pointing possible misunderstanding out. We have revised the usage of the different descriptions of ABLs. The abbreviation ABL and MHL were not used, but MLH (mixing layer height), which is a metric describing the upper boundary of an atmospheric layer (which is assumed to be mixed within itself). The MLH is not supposed to describe a mixing state of the respective layer like CBL (convective boundary layer) and is made clear in the beginning of Sect. 4.4:

"Until this point of the study the MLH is used as a synonym for the height of the planetary boundary layer (PBLH), which is considered the most relevant measure separating the free troposphere and the ground-influenced layer with different dynamical characteristics and potential impacts on the chemical composition."

The calculation of the MLH from the model has been adapted from (Wang and Wang, 2014). In addition to the direct calculation using this approach, also the profiles for relative and absolute humidity and θ, as well as the Brunt-Vaisälä frequency (as a measure of the change of the stability of the vertical profile at the upper edge of the PBL (e.g., Hyun et al., 2005)) are analyzed and checked for consistency. The critical threshold values have been determined from a statistical analysis of all simulated profiles at the measurement site by a subjective measure which also fit well with observations from the ceilometer data.

The last statement was included in the end of Sect. 2.3 after explaining MLH criteria:

"The MLH is selected as the lowest altitude where all the following criteria in the ICON-D2 data are fulfilled: Brunt–Väisälä frequency > 5 × 10-5 s-1, vertical gradient of potential virtual temperature > 0.3 K km-1, and vertical gradient of absolute humidity must be curved (Wang and Wang, 2014) ***with threshold values empirically determined by the analysis of the simulation profiles in conjunction with the local observations and Hyun et al. (2005)."***

In the original version of the manuscript, "NBL" was used widely for the lowermost layer identified in the morning, which is a remnant of the NBL (although its already daytime, see answer to your comment 3b). The term "NBL" was changed to "NBL remnant" to avoid confusion and highlight, that this is an atmospheric layer residual from nighttime.

3. In the results, the evidence for some of the claims/conclusions is not clear. In particular, with regards to the presence of a cold pool in Case II and the presence of an NBL.
    a. Where do you see evidence of a cold pool? If it is from ICON, I suggest including a figure that shows it. Right now, it reads as a cold pool might be there because of surface evaporation (which wasn't measured I think?) would increase after a rain event. If all the evidence is from the ceilometer (Fig. S10) that should be moved into the main body.

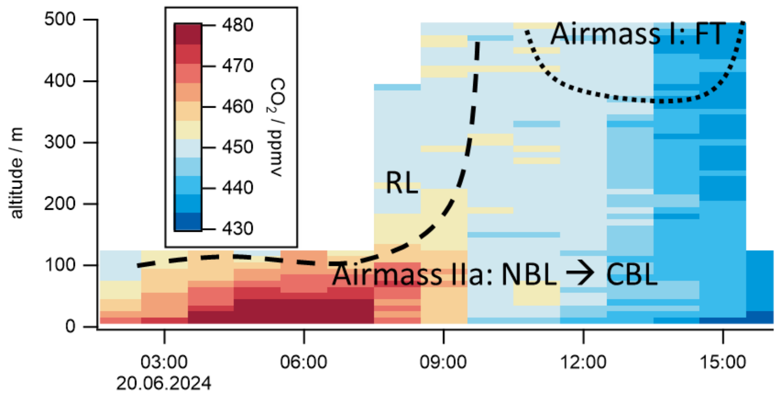
The evidence for a cold pool originates from the sum of indicators from vertical profiles measured with the UAV and ground-based observations:

i. In Fig. 7c, for dθ/dz, the lowermost bins at 50 m to 150 m a.g.l. between 14:00 and 16:00 show a strongly stabilized shallow layer, while conditions aloft are more unstable.
ii. In Fig. 7d, the gradient of the absolute humidity is strongly negative for the lowest two height bins, and decreases between 16:00 and 19:00. This indicates evaporation.
iii. Regarding the probing height in 7a and e, the O_3 mixing ratio stays low at the 50 m bin indicating a clear separation from the air masses included in the 150 m bin (100 m to 200 m a.g.l.), while the Richardson number highlights a clear separation of different mixing layers between the 50 m and 150 m mark. The gradient dissipates for well mixed conditions from 17:30 on.
iv. "The increase of equivalent potential temperature after the last rain at ground level by 6 K and by 2.1 K for the mean vertical profile between 0 and 100 m, indicates a cold pool (Tompkins, 2001)".

The argumentation in Sect. 4.2 was rephrased to include the four indicators:
"In our case, following the rain events at 13:30 and 14:45, we observe a cold pool from ground and FLab measurements, identified based on four indicators: (i) stratification between the lowermost 100 m (cold pool) and aloft is visible for the (thermo-)dynamical markers dθ/dz and Ri (Fig. 5a, c) as well as for the O_3 mixing ratio (ii), which is ~10 pptv (20%) lower in the lowermost layer (Fig. 5e, presumably due to depletion in the wet cold pool); (iii) the increased negative gradient and variance of the absolute humidity (Fig. 5d) and (iv) an increase of the equivalent potential temperature by ~6 K at ground and by 2.1 K for the whole 500 m range (Fig. 5b) show the evaporation and conditional enhanced latent heat flux typical for cold pools (Tompkins, 2001; Engerer et al., 2008)."

b. From your results, it is not clear to me why you're considering the ABL before the rain an NBL. For example, in Case I, you have positive sensible heat flux, relatively strong TKE (Fig. 1) and negative Ri and negative slope in $d\theta eqdz$ (Fig. 2). Same with Case II, it looks like a typical morning ABL growth (e.g., 50 m is unstable before the rain).

Thank you for pointing out that the term "NBL" for airmass IIa in Case I is used wrongly. The airmass IIa in Case I should best be described as a suppressed convectively boundary layer (CBL) originating from NBL dissipation. At this time of the day, the NBL has already dissipated into a CBL as the distribution of CO_2 (an inert marker for the NBL) indicates in the following figure, where the dotted lines highlight different MLHs:



For Case I, consequentially, we have changed the description of a "NBL" to "(suppressed) CBL" to highlight the internal convective mixing of the layer IIa. The strong *TKE* at ground, a negative *Ri* and a negative slope in $d\theta eq/dz$ in the vertical profiles indeed indicate turbulent motion in the CBL. However, this turbulent motion is apparently not strong enough to allow mixing lowermost layer IIa with the free-tropospheric airmass I aloft as demonstrated by the clear chemical separation (free tropospheric air (O_3-rich & particle-poor) that is layered aloft NBL-originating-air (O_3-poor & particle-rich)).

For Case II we disagree and would stick to the term "NBL". It was assigned to the respective airmass IIa, which dissipates in the early morning from 08:00 on and does not exceed the 250 m mark until 10:00.

c. I like Figs. 4 and 6 for making the concept easy to understand, but I don't think the data that underlies them is clear in the manuscript.

We now introduce Figs. 4 and 6 earlier at the beginning of the respective section, when the individual case is presented the first time. The additional information and interpretation of the NBL/CBL/FT- separation and the cold pool identification were added in the following subsections. Conclusions are hopefully more reasonable with this new structure.

4. It is quite a board paper. Topic-wise, you cover ABL dynamics and chemical composition and you use many different methods. In the abstract, introduction, and summary, clarify the intent and significance of the paper. For example, what can we apply to other sites? Is this a validation of ICON?

The observations are brought into context with the larger scale by using the ICON data. The consistency between the observations and the simulation results depicts the validity of the observations not to be only local phenomena of the measurement site and allows for process-oriented analysis, e.g., how the BLH is changing in the event of the warm front, whereas in situations with strong rainfall local small-scale cold pool development can deteriorate the PBL structure. Consequently, intense cold fronts or intense precipitation from convergence lines lead to a stronger modification of the BL characteristics including the height estimates. The physicochemical processes inside the boundary layer are assumed to be representative for similar topography (rural hilly regions and plain fields).

Now, we imply the intent and significance of the paper in the second last paragraph or the introduction and in the beginning of the abstract, explain the significance at the end of the abstract and in the second last paragraph of the summary.

Technical points:

1. Line 45 – It is a bit confusing why MOST is brought up here in the context of observational limitations.

   The introduction of MOST was removed from the introduction to reduce the load of material, which is not crucial to understand the manuscript.

2. It would help if you included the equations for equivalent potential temperature and vertical potential temperature as they are derived variables.

   Both equations are now included in Sect. 2.2.

3. Line 87 – I am not familiar with "aspiration" in this context, and I don't understand the meaning here.

   The term "aspiration" was changed to "air mass inflow" for better clarity.

4. The order of the supplemental material is out of order compared to how it is referenced in the paper.
   The order and references of supplemental material was corrected.

5. The order of referencing figures in the paper is also out of order (e.g. Fig. 4 is referenced in text before Fig. 3).
   We revised the order of referenced figures completely as Fig. 4 and Fig 6 were shifted closer to the beginning. Now, the order and figure references are all correct.

6. Some citations are the wrong type (e.g. lines 118, 119, 120, 124 and other places). If using latex, change \citep to \cite for the proper citation type.

   Using Microsoft Word for writing the draft, we do not see an inconsistency how the citations were included. We are confident that any changes in type will be removed during the typesetting process.

7. Line 132 – you haven't defined the MLH yet.

   The definition of a MLH was added in the beginning of Sect. 2.3 where it appears the first time.

8. Fig. 1. Check the units of Q_H are identified correctly. Is it truly < 2 W m-2 all day? And what is the meaning of QH * 10 in the legend.

The units of Q_H are correct. The high values of up to "1.5 W m^{-2}" are due to the artificial factor of 10 on Q_H to display TKE and Q_H on the same axis. This is now clarified in the figure caption.

9. Line 214 – rephrase the "like, e.g." combination.

",e.g." was removed to reduce the duplicity of this adverb.

10. Line 271 – I'm not familiar with "lability" in this context.

We think that the term "lability" can be used as a synonym for low tropospheric stability, e.g., $d\theta/dz < 7.5$ K km^{-1}, as used in typical synoptical analysis, i.e., that the air mass is unstable against moist adiabatic vertical movement.

11. Line 426 – don't use "significant" if you didn't do a significance test.

The term "significant" was removed.

References

Hyun, Y.-K., Kim, K.-E., and Ha, K.-J.: A comparison of methods to estimate the height of stable boundary layer over a temperate grassland, Agricultural and Forest Meteorology, 132, 132-142, https://doi.org/10.1016/j.agrformet.2005.03.010, 2005.

Wang, X. Y. and Wang, K. C.: Estimation of atmospheric mixing layer height from radiosonde data, Atmospheric Measurement Techniques, 7, 1701-1709, https://doi.org/10.5194/amt-7-1701-2014, 2014.