We would like to thank the referees for their time and investment to go through the paper and give valuable constructive and detailed comments. We created a revised version of the article for re-evaluation including all suggestions, improved unclear formulations and corrected Figure labels. We finally thank the reviewers in advance for their renewed efforts.

We have reanalysed the ozone data for the measurement results of Case 1 according to the new publication of Vömel et al. 2020. This confirmed the main message of the paper that tropospheric air masses with lower ozone and higher water vapor values were mixed into the stratopheric air by gravity wave breaking behind an overshooting top. In the updated Figures, the drop in ozone and peak in water vapor coincide almost perfectly. Further a unit conversion error in the analysis of ozone data along the trajectory from ERA5 data was corrected. The misleading formulation regarding the concept of overshooting was corrected.

In the following we address all the comments by the two referees.

1 RC 1

- L194-196: **To make this phrase easier to understand, I suggest to mention first the enhanced ozone and point to the subsequent section 3.3. Other wise, the description here appears to me difficult to understand. I see directly the high ozone content rather than the diluted ozone due to the mixing with tropospheric air. The question also rises here why the ozone content is high**

  → Corrected, Changed to: "A striking peak in the ozone profile is evident at a similar level as the peak in water vapor. With a lower edge at 162 hPa/359 K and an upper edge at 145 hPa/373 K the ozone peak starts at a lower level compared to the water vapor enhancement, but is limited by the same upper edge. This ozone peak is not associated with the overshooting event and the cause is discussed in Section 3.3."

- L201-202: **I'm wondering what is the cause of the zigzag form of the T profiles between 160 and 140 hPa. Is this due to the instrument artifacts or any other reasons?**

  → We think it is not an instrument artifact, as temperature measurements on radiosondes are quite accurate even at low temperatures (WMO Intercomparison of High Quality Radiosonde Systems, 2011). The drop in temperature is suggested to be caused by in-mixing of tropospheric air from the overshooting top which is much colder due to adiabatic ascent despite contributions from latent heat release.

- L202-203: **I have doubt about the use of overshooting top in this statement, The overshooting tops are usually of small dimension (several km in D). The ice water content is always very height inside and the temperature are usually much lower than the ambient lower stratospheric air (much larger than 2K). In addition, within the overshooting top it is usually very dry due to the low temperature. I guess what you talk about here is either the mixing area near the overshooting tops due to the breaking of gravity waves (jumping cirrus, ice plume and eventually plume with higher humidity, etc.). I do see in the simulation that these areas can be a little cooler that the ambient temperature due to the mixing with lower troposphere air and the sublimation of mixed ice just after the convection. Same for an other statement in L424**

  → We agree that the wording is misleading and changed the text to '[...]result of mixing with the strongly idiomatically cooled tropospheric air within the overshooting top and the warmer stratospheric air masses in the surrounding. In addition, evaporation/sublimation of cloud particles in this warmer and dryer mixing area around the overshooting top can also lead to further cooling.' and in L424: '[...] the in-mixing of tropospheric air masses caused by gravity wave breaking closely behind an overshooting top into the surrounding stratospheric air, which has a lower water vapor mixing ratio and a higher potential temperature. The hydro-meteors from the injection sublimate and are mixed into the stratospheric air mass on very small time-scales under these strongly sub-saturated conditions. It is therefore consistent that the additional COBALD measurement (not shown) did not detect any cloud particles in the measured profile. The sublimating hydro-meteors additionally cool the air mass.'

- Figure 34 **is there any ice observed in these two cases? If there is any in the lower stratosphere, maybe it is worth adding them into the plots with some discussions.**

→ For Case 1 the COBALD backscatter instrument does not show any indication for particles in the lower stratosphere and thus is a sign that all ice particles have evaporated by that time. For Case 2, unfortunately no COBALD measurements are available. As the COBALD data does not provide any important additional information it was not added to the paper for simplicity.

- L327: **I suggest emphasizing here that the thickness of the humid layer from the observation (vertical dimension in m or km?) is much thinner than the resolution of the MLS data in the lower stratosphere which is in the scale of kilometers**

  → The vertical spread of the water vapor peak is ≈800 m for Case 1 and ≈600 m for Case 2. We added the sentence: 'While the vertical extent of the water vapor peak is 800 m (600 m for Case 2), the vertical resolution of MLS H_2O is 1.5 km.'

- L485-486 **I suggest also to point out the need in the future satellite mission that in high resolution in the vertical dimension is crucial for understanding the water vapor distribution in the UTLS**

  → Changed last sentence to: 'Due to the low resolution of satellite data, in-situ measurements and future satellite missions with very high vertical resolution in the UTLS are therefore required to understand the impact of such small-scale events like overshooting convection.'

<u>Technical corrections:</u>

- L81 **is there any altitude dependency of the humidity sensor?**

  → Yes, we know that the accuracy of the RS41 radiosonde humidity sensor decreases with altitude due to the low temperatures. Here, we used the official values provided by the manufacturer in: "Survo Petteri, Raisa Lehtinen, Jari Kauranen: SI traceability of vaisala radiosonde RS41 sounding data - calibration and uncertainty analysis, pp. 6–9, 2008". It was shown that the RS41 has a higher uncertainty at lower temperature within 2-3% RH at -80C. Due to this issue we use, when possible, two humidity sensors i.e. the CFH as the second and more reliable humidity sensor. We added a sentence: " Petteri et al. 2008 shows a temperature dependency of the humidity sensor uncertainty, which does not exceed 3% RH at temperatures below -80C and RH below 30%."

- Figure 2 **I suggest to write H_2O instead of H2O in the x and y labels. Same for the other figures (and O3 as well).**

  → Corrected.

- L178-180 **Please verify the numbers used in this phrase** "Between pressure levels of 180 hPa and 162.5 hPa, which correspond to potential temperature levels of 345 K and 357.5 K, the water vapor mixing ratio fluctuates between 5 ppmv and 7.4 ppmv and between 6 ppmv and 14.5 ppmv as measured by the radiosonde and the CFH respectively, before it attains the stratospheric background value of ≈ 4-5 ppmv, which is reached within all Case 1 profiles below the 160 hPa/360 K level. A background value of ≈5 ppmv agrees well with results of previous studies (**?**)."

  → More exact numbers inserted.

- Figure 3 **In figure 3, I see the values are mostly around 4 ppmv instead of 5** → Values of around 4 ppmv are indeed reached shortly before the water vapor peak at 162.5 hPa. However, before that at pressure levels of 180-162.5 hPa (and the corresponding potential temperature levels), the water vapor mixing ratios vary between 5 and 7.4 ppmv. We corrected the sentence to 'Between pressure levels of 180 hPa and 162.5 hPa, which correspond to potential temperature levels of 345 K and 357.5 K, the water vapor mixing ratio fluctuates between 5 ppmv and 7.4 ppmv and between 6 ppmv and 14.5 ppmv as measured by the radiosonde and the CFH respectively, before it attains the stratospheric background value of ≈ 4-5 ppmv, which is reached within all Case 1 profiles below the 160 hPa/360 K level.'

- Figure 3: **repeating phrase in the caption: 'The gray region marks the level between 365K and 370K in which the water vapor enhancement is observed'**

  → Both phrases combined to : "The gray regions mark the level between 145 hPa and 165 hPa in a) and between 365 K and 370 K in b) in which the water vapor enhancement is observed."

2 RC 2

– **Regarding the relative levels of the water vapor and ozone peaks: Are sampling time constants for each instrument accounted for? If the response time for water/CFH is slower than that for the ECC cell, I can imagine a case where their peak locations might be offset?**

$\rightarrow$ Indeed, we didn't correct for a time lag of the ECC ozone instrument. Thus, the ozone data were reevaluated with the new data processing method described by Vömel et al., 2020. With this method one can directly see the drop inside of the ozone peak in both plots, using pressure and potential temperature as a vertical coordinate, which coincides with the water vapor peak. So both profiles fit even better to each other (see Figure 1). Figures 3, 5 and 9 in the manuscipt are updated, accordingly.

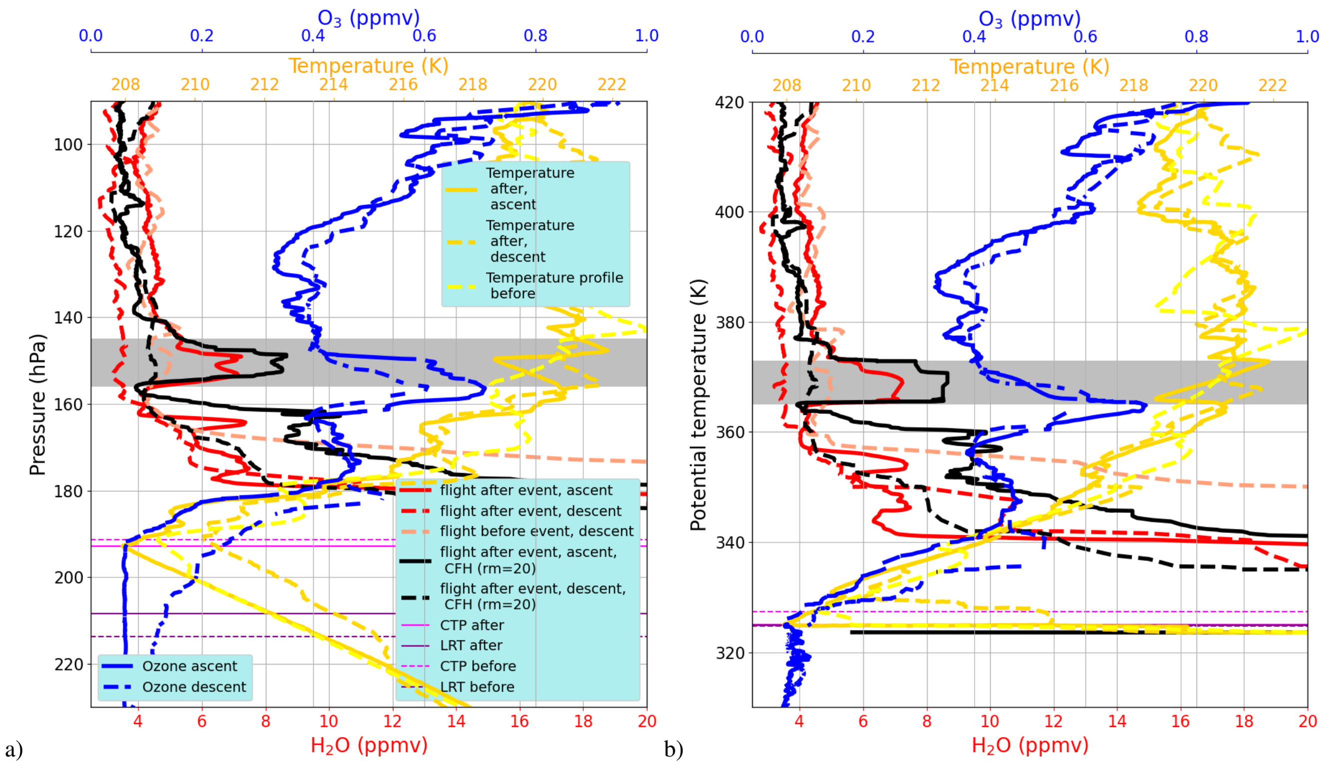

Figure 1. Same as Figure 3 in Ozone profiles corrected for time lag of the instrument.

– **The structure of the temperature profiles, and the mixing ratio profiles when converted to potential temperature are peculiar (square-shaped and peaked in Cases 1 2, respectively), is it possible there is a measurement issue - either vertical resolution, or something else?**

$\rightarrow$ The change of the peak structure due to conversion to potential temperature coordinates can be explained by the vertical potential temperature profile itself. In Figure 2 of this reply we added the potential temperature profile for each observation case. Here, it becomes evident that the structures of the potential temperature at the level of the water vapor peak are different leading to the different shapes in the potential temperature plots 3b and 4b of the paper. For Case 1, potential temperature (theta) is almost constant from the lower edge of the gray area to $\approx$ 148 hPa where theta abruptly increases its gradient to an almost horizontal line. When transferring this data into a plot with potential temperature as a vertical coordinate, the water vapor and ozone data where theta is almost constant become compressed and the structures

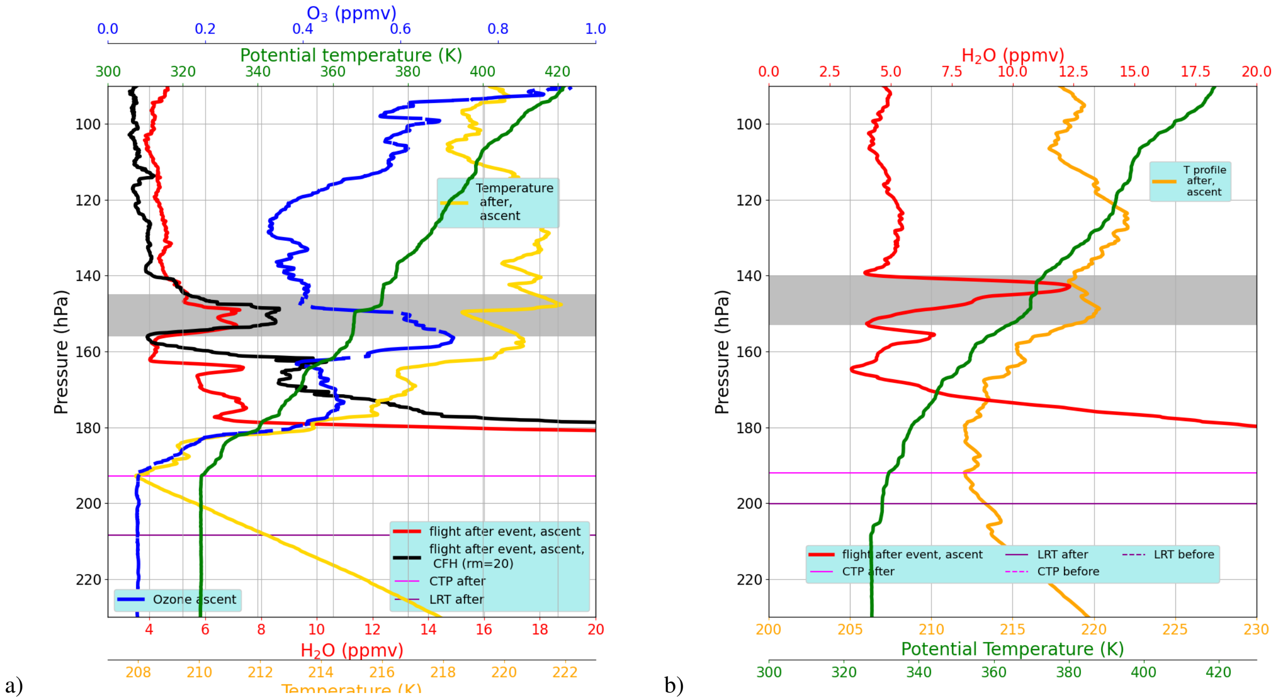

Figure 2. As Figures 3 a and 4 a but including potential temperature in the plot (green line).

become almost invisible. On the other hand, the data which coincide with the strong theta gradient are stretched. Hence, for Case 1, the lower part of the gray area in panel a "disappears", but a very thin part with the highest values (which appear at 148 hPa) is stretched forming a squared shape. This feature is even amplified by the fact that above the main peak theta again remains almost constant in the upper part of the gray panel, leading again to a compression of the data, which finally results in the square shape of the peak in theta coordinates.

Case 2 shows a different behavior of the location of the water vapor peak and theta gradient. When using pressure as a vertical coordinate, theta remains almost constant at the level where the water vapor peak is highest. Hence, this area becomes compressed while the areas below and above, where water vapor is increasing/decreasing, get stretched due the steep theta gradient. This explains the spike-like shape of the water vapor peak when using theta as a vertical coordinate.

In summary, we do not think that there is any measurement issue with respect to both profiles. We are using the same radiosonde type for measuring temperature and humidity in both cases. The radiosonde humidity profile in Case 1 shows the same square profile feature as the more accurate CFH instrument and the profiles are consistent to each other. Thus, we think that the difference between both the shapes of Case1 and Case2 is mainly due to the different temperature profile.

– **I am not convinced by the argument given in Lines 237+ regarding the profile of Case 1 being within the "usual range of observations." This may be true, but with only 8 profiles in total, it's difficult to define a "usual range." It might be possible to obtain better statistics on the distribution of ozone mixing ratios at this level using MLS?**

$\rightarrow$ Indeed 8 profiles might be not a good representation for a "usual range". However, this is our available data set. Using MLS data would be possible but might lead to a few complications. MLS ozone has a vertical resolutions of ≈ 1.5 km. The extend of the ozone peak is less then 1 km and as can be seen in the grey area (Figure 2), is quite variable. This strong averaging within the MLS profiles probably would narrow the true ozone variability in this region and hint that

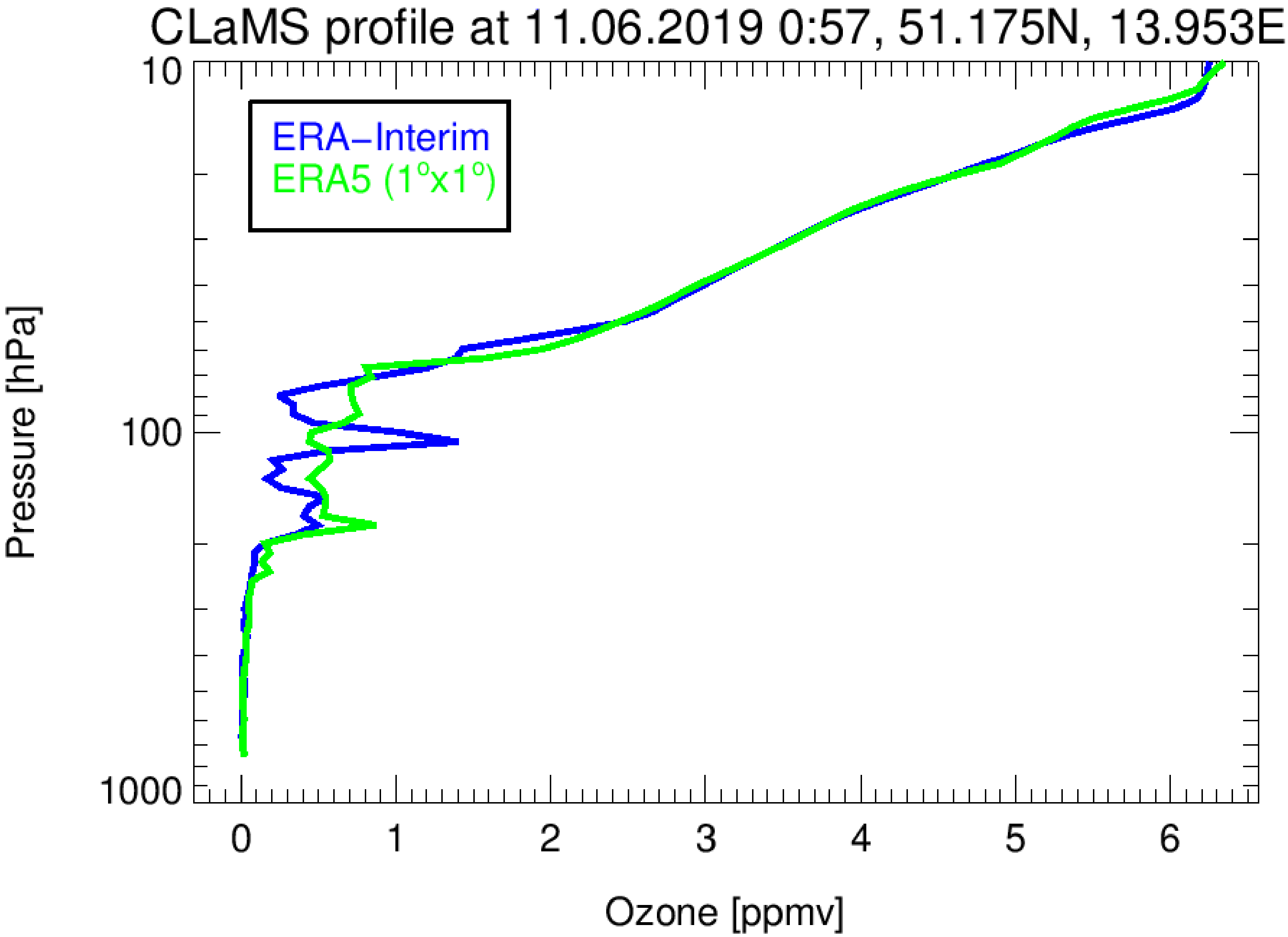

Figure 3. Ozone profile of CLaMS driven with ERA5 (green) and ERA-interim (blue).

this kind of peaks are not unusual.Further, also the horizontal resolution of MLS (several 100 km) is important here) The text was changed to :"[...]it is not out of our observed range analyzed here"

– **How narrow (vertically) are the modeled ozone enhancements in CLaMS and the ERA5 output? Is it consistent with the exceptionally narrow (0.6 km) enhancement in ozone observed on the ascent profile?**

→ The vertical extent (using pressure as a vertical coordinate) is about 10 hPa. The vertical extent of the peak seen in the CLaMS run driven with ERA5 has a similar extent as can be seen in Figure 3. In the CLaMS run driven with ERA-Interim the profile structure is different and has a larger vertical extent. This is most likely due to the coarser vertical resolution of ERA-interim in the UTLS.

In Figure 3 of this reply, "HN2/ERA-Interim" is the CLaMS run after Pommrich et al., 2014, (https://doi.org/10.5194/gmd-7-2895-2014) applied to 2019 using ERA-Interim data. As it is not the focus of the paper this plot was not added to the paper.

– **Lines 244-247: The brief mention of a model run with CLaMS “using different ECMWF data sets as input” is hard to evaluate and understand, particularly as the results are not shown. What is shown is a plot of output from**

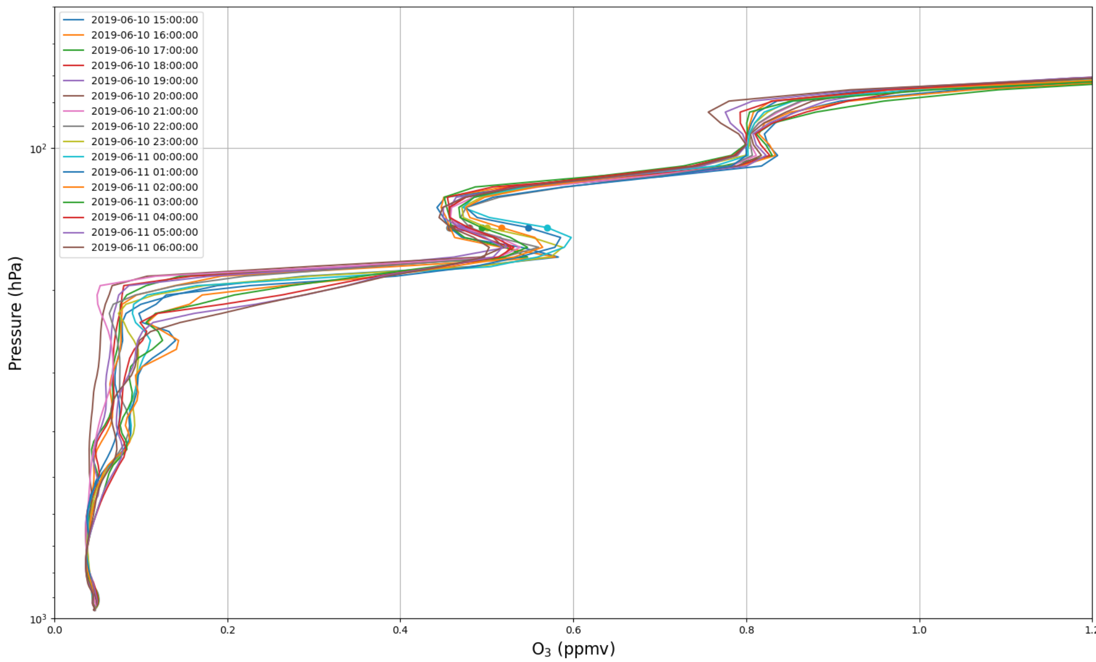

Figure 4. ERA5 vertical ozone profiles shortly before and after the convective event passed the measurement location at the closes grid point. The dots show the level of the water vapor peak.

ERA5, which, as the authors state, shows evidence for convective influence in multiple variables. More regarding the ERA5 ozone:

- Why does the map of ERA5 ozone (Fig. 6) with peak values at 145 hPa of ≈640 ppbv to 560 ppbv, differ so much from the ERA5 trajectory values in Fig. 9, which are >800 ppbv for the time period of interest?

→ Thank you very much for this note! Unfortunately, an unit conversion error from mass mixing ratio to volume mixing ratio, lead to this miss-match. The error was corrected and the Figures 9 and 10 were corrected and now show consistent ozone values. The new plots are shown in Figures 5 and 6.

- Is it reasonable to assume the assimilated ozone is impacted by convection?

→ ERA5 vertical ozone profiles do not show a dilution of the local ozone peak. We therefore assume, that ERA5 does not account for any decline in ozone for convective events. In Figure 4 a peak in ozone is visible, similar as in Figure 3, but no drop within the peak is visible like in the measured data. As satellite ozone measurements are typically assimilated into ERA5, we actually do not expect to find such a small scale feature in the ERA5 ozone profile. In contrast, ERA5 water vapor is a modelled quantity and thus can represent the impact on convection.

- Though ozone declines over the trajectory (Fig. 9), the declines do not appear to be closely associated with the locations/timing of convection. This is also noted by the authors in Lines 320-321.

→ This is true. Figure 4 further supports this fact that in ERA5 water vapor is affected by the convection, but ozone is not.

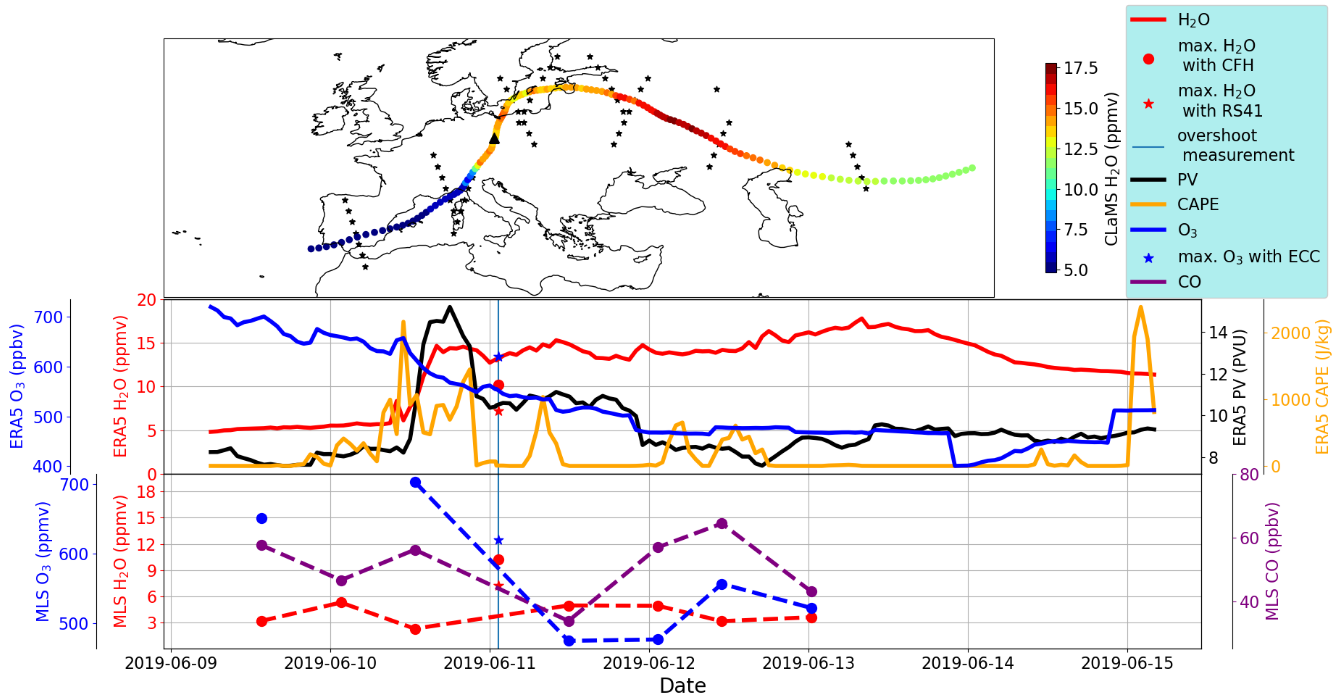

Figure 5. Figure 9 corrected

– **While I think this paper does a good job of utilizing and even showcasing the value of the new ERA5 product, it's somewhat concerning that the ERA5 ozone values are so elevated relative to measurements – they're consistently $\approx$80% higher!**

$\rightarrow$ The reviewer is correct here. As explained above, the mismatch of ozone values is the result of a unit conversion error which is now corrected. In the corrected version ERA5 ozone underestimates the measured data, but to a much smaller extent.

– **I find the practice of referring to reanalysis output as "data" misleading, as it is not a primary observation/measurement.**

$\rightarrow$ Corrected as suggested throughout the text.

– **Statements such as those in Lines 507-508 and in Lines 498-499 about instruments not requiring calibration are misleading. It is best to leave such statements out of a paper like this where there is not room to fully understand their meaning and context. A reference to papers describing the development, operation, calibration and validation of the ECC ozonesondes and CFH and other chilled mirror hygrometers is sufficient.**

Ozonesondes do not need a calibration with ozone standards before they are launched, however, they do need to go through careful preparation one to two weeks prior to launch. These steps include high ozone conditioning the pump/tubing, measuring the background cell current, accurately measuring the teflon pump flow rate, using the correct solution and understanding the pump's efficiency over the vertical profile, among many other things.

Similarly, while CFH does not need a calibration with water vapor standards prior to launch, it does require a thermistor calibration among other operational assessments to ensure measurement accuracy

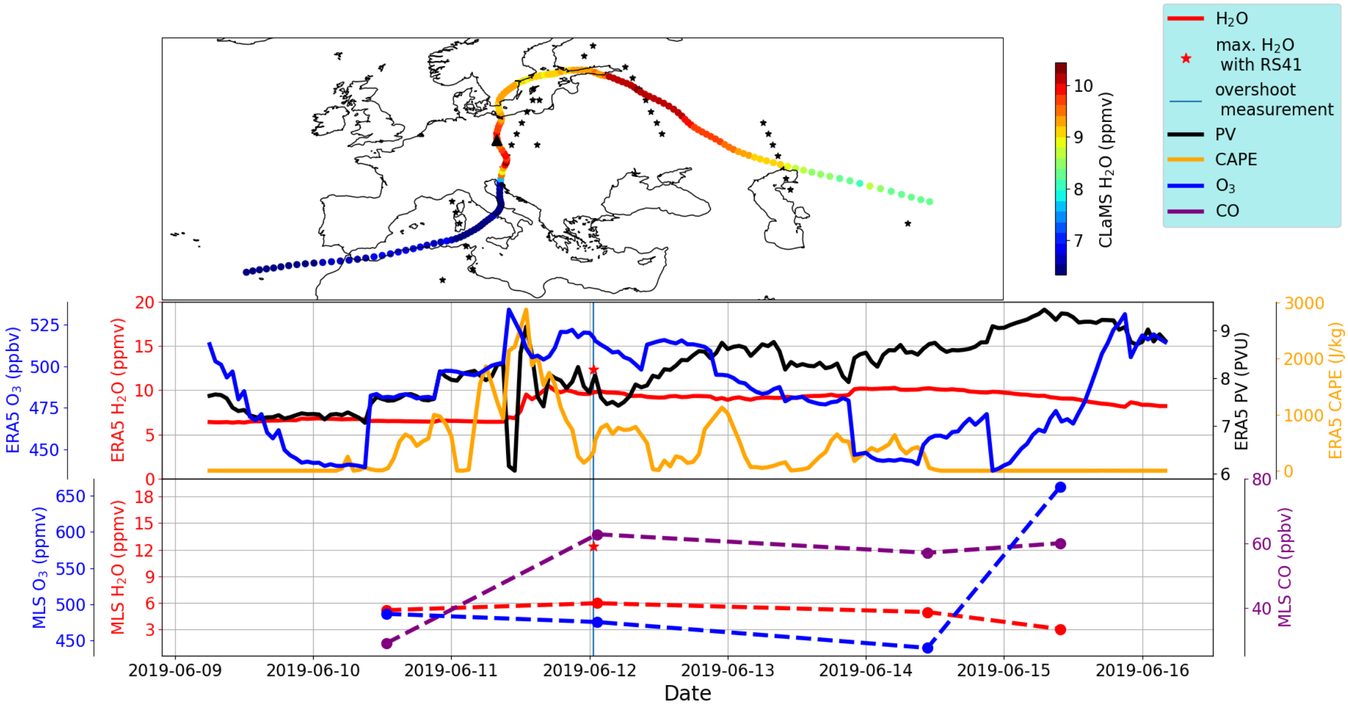

Figure 6. Figure 10 corrected

→ We agree with your option and removed both sentences from the text.

– **Similarly, I believe what is meant in Lines 508-509 is that there are few balloon-borne in-situ instruments capable of measuring stratospheric water vapor. There are balloonborne remote sensing instruments, as well as satellite instrumentations, e.g., MLS. Furthermore, there are multiple in-situ instruments employing different techniques for measuring stratospheric water vapor aboard aircraft.**

→ This sentence was a bit misleading and therefore was taken out of the manuscript.

<u>Additional comments</u>

<u>Abstract</u>

– **Line 14: . . . values measured by MLS in the LS are lower than the in situ observations. . . (MLS observations are measurements not estimations)** → Corrected as suggested.

– **Line 14: . . . ERA5 overestimates water vapor mixing ratios. . . (fine to say that reanalysis overestimates a value, given that it is not a primary measurement/observation)** → Corrected as suggested.

– **Line 16+: This is in good agreement with the reanalysis which shows a strong change in the structure of isotherms and a sudden and short-lived increase in potential vorticity at the altitude of the trajectory. Similarly, satellite data show low cloud top brightness temperatures during the overshooting event. (clearer delineation between the satellite observations and the reanalysis derived output)** → Corrected as suggested, but added "[...] at the altitude and location[...]"

<u>Introduction</u>

- **Line 27: . . . entry mixing ratio. . .** → Corrected as suggested.

- **Lines 30-34: It may be worth mentioning that there is some debate about the net magnitude of SWV feedback, with some studies suggesting that the impact on climate sensitivity may be significantly lower if changes in SWV are evaluated within a coupled system, e.g., Huang et al., 2020** → In line 35 we added: "However, the magnitude of the impact of stratospheric water vapor when a coupled global model is used are still under discussion. Huang et al. 2020 and Wang et al. 2020 show that the radiative effect of stratospheric water vapor is balanced by a decrease in high clouds and increase in the upper tropospheric temperature."

  Additionally a paragraph was added in lines 49-54: "Phoenix and Homeyer (2021) simulated two kinds of convection - one representing spring time convective events and one representing convective events typical for summer time. The study shows that simulation representing the spring time convective event lead to an increase of about 20% in the average water vapor mixing ratio in the UTLS while the summer time simulation lead to an 7-11% increase. Fischer et al. (2003) show data displaying troposphere to stratosphere transport of tropospheric tracers caused by convective storms over Italy and Hegglin et al. 2004 analyze a case study of the injection of tropospheric air into the LS by a large convective system over the Mediterranean area."

- **Lines 43-44: The observational analysis of Smith et al., 2017, focused on a significant localized effect. The model analyses of Dessler et al., are looking at global impacts. (By the way, Dessler et al., 2013a is the same as Dessler et al., 2013b.)** → We removed the references to Dessler et al. at this place and considered only Smith et al..

- **Line 56: . . . North American and Asian Monsoon regions** → Corrected as suggested.

- **Line 61: . . . the data is/are compared to output of the ECMWF ERA5 reanalysis, while. . .** → Corrected as suggested.

<u>Section 2 Data and Methods</u>

- **Line 76: (Consistency in tenses.) . . . before the convective cell reached the. . .** → Corrected as suggested.

- **Line 81 later, Lines 98-99: The actual uncertainty of the Vaisala humidity sensor is found to be far greater than 3% through comparison with CFH in the altitude range of interest. This is evident both in Figure 2, where differences can be 100% at altitudes <20 km (log/log plot), and in Figure 3, where differences in the quiescent stratosphere are on the order of 30-50%. That said, for the purposes of this analysis, the absolute accuracy of the Vaisala sensor is less important than its sensitivity to detecting abrupt changes with a magnitude far greater than its measurement uncertainty.**

  → Here we refer to the data give by the manufacturer. We changed the sentence to: "Survo et al. (2008) report a temperature dependency of the uncertainty of the humidity sensor, which does not exceed 3% RH at temperatures below -80C and RH below 30%. " and added the following text into this Section: "However, the data spread around the 1 to 1 line in Figure 2 reveals some times differences of up to around 100% in the altitude range of the UTLS (<20 km) mainly due to the slower time response under cold conditions. But for the purposes of this analysis, the absolute accuracy of the RS41 sensor is less important than its sensitivity to detecting abrupt changes with a magnitude far greater than its measurement uncertainty."

  A more accurate definition of the uncertainty of the Vaisala humidity sensor is beyond the scope of this work.

- Line 84: **. . . ozone mixing ratios with an uncertainty. . .** → Corrected as suggested.

- Line 87: **. . . uncertainty of the CFH instrument is given as. . .** → Corrected as suggested.

- Line 103: **. . . stratosphere reaching altitudes of up to. . .** → Corrected as suggested.

- Line 106: **. . . but all other sounding data are complete.** → Corrected as suggested.

- Line 108: **What does “a local instrument” mean? MLS is a remote sensing instrument.** → The word is local is removed as it makes no sense in this case.
- Line 111: **Temperature and pressure are retrieved…** → Corrected as suggested.

Section 3.2 Water Vapor Injection – Balloon

- Line 182: **(Awkward.) A background value of ≈5 ppmv agrees well with…** → Corrected as suggested.
- Line 185-186: **(Given the ±1 ppmv differences between the Vaisala and CFH in the quiescent stratosphere, i.e., above the hydration layer, I am not convinced the response time is the sole cause of the difference in the plume.) You might say: The lagging response time of the RS41 may explain most of the difference between the CFH and the radiosonde observations.** → Agreed and corrected as suggested.
- Line 191: **(I think you could start a new paragraph for the discussion of the ozonesonde and temperature profile results.) Consider: A striking peak in the ozone profile is evident at a similar level as the peak in water vapor.** → Corrected as suggested.
- Line 204: **(I think you could start a new paragraph for the discussion of Case 2.) Also:…different background atmosphere than Case 1.** → Corrected as suggested.
- Line 205: **…as is depicted in Figure 4a…** → Corrected as suggested.

Section 3.3 Source of Ozone Peak

- Line 227: **(Delete “that”) …shows a steep decrease…** → Corrected as suggested.
- Line 240: **(Delete “but”) …profile at this altitude, it remains…** → Corrected as suggested.
- Lines 241+: **(The term “measurement data” is vague.) Consider: Here, the data from Case 1 (red dots) with the high ozone and water vapor diverge...** → Corrected as suggested.

Section 3.4 Comparison with ER5

- Line 290: **Consider: …diabatic heating, which cause an increase…** → Corrected as suggested.
- Line 301: **Consider: Similar to rather than Similarly to** → Corrected as suggested.

Section 3.5 Origin Evolution Along CLaMS

- Lines 304+: **For simplicity, I think it would be best to refer to the trajectory output as ERA5 consistently rather than switching back and forth between ECMWF, ECMWF ERA5, etc.** → Changed to ERA5 everywhere.
- **I found this confusing and am wondering how to reconcile the “multiple MLS data points” from Line 311 with “Only 7 measurement points” from Line 323? I’m also unclear how the dots in Fig. 9 panel C correspond to the overpasses in panel A? It looks like there are 9 distinct overpasses that cross the ERA5 trajectory, but only 7 points in panel C?** → We applied two criteria for the MLS points to be considered in panel C. The MLS points shown in the upper panel are chosen as:

  "The upper panel also displays the water vapor mixing ratios along the trajectory and the data points measured by MLS within 5 degrees of latitude and longitude and an hour before or after the trajectory point (star symbols)" (Line 307-309)

  In panel C, the points are chosen from the points in the upper panel panel if (for each time step)

  - they are within 300 km
  - 60 min before or after each time step
  - if multiple points are full filling this criteria, the closest is displayed

This is mentioned in Line 321-323. This stricter borders can lead to the fact that none of the points of an MLS overpath visible in the upper panel fulfill the criteria and therefore only 7 points are displayed in the lower panel. It was not well sorted in the text and we have reorganized the sentences to make it clearer.

Section 3.6 Overshooting in Satellite Data

- Line 365: **(No comma needed after "Multiple areas")** → Removed.
- Line 367: **Consider: "...however, at a greater distance..."** → Corrected as suggested.

Section 4 Discussion

- Line 375: **Consider: "...on two consecutive days..."** → Corrected as suggested.
- Line 376: **Consider: "...overshooting that injected water vapor into the lower stratosphere several hours..."** → Changed to: "Both cases originate from gravity breaking behind the overshooting top leading to in-mixing of tropospheric air into the lower stratosphere several hours before the balloon launch. " due also to corrections from RC1.
- Line 386: **Consider: "The local injection of water vapor was detected within a larger scale peak in ozone for Case 1."** → Corrected as suggested.
- Line 388: **Consider: "A map of ERA5 ozone at 145 hPa in Figure 6 shows that the balloon measurement was at the edge of a front with higher ozone mixing ratios. This explains the lower ozone values at the same pressure/potential temperature level in the descending profile, which was located further north. This is also supported by the sparse data from MLS, which show higher ozone..."** → Corrected as suggested.
- Line 393:**"...ozone-rich air mass..."** → Corrected as suggested.
- Line 409: **Consider: "...further south-west, but otherwise both the reanalysis and the observational data show a convective storm moving northwards..."** → Corrected as suggested.
- Line 434: **"...before the suggested convective event..."** → Corrected as suggested.
- Lines 447+: **("it" is vague, I wasn't sure if "it" referred to ERA5 or the satellite data, and whether you meant to write satellite data at the end of the sentence?) Consider: "While ERA5 shows a likely overshooting event for Case 1 four hours before and slightly north-west of the event observed by satellites, the convective event in ERA5 for Case 2 is four hours later than the satellite observations."** → Corrected as suggested.
- Line 461: **Delete "for example" or consider: "...and is not caused by another mechanism, for example, the horizontal transport and..."** → Corrected as suggested.

Conclusions

- Line 468: **Consider: "...water vapor enhancements in excess of the background..."** → Corrected as suggested.
- Line 478: **("measured" is used twice.) Consider: "...measured water vapor enhancement shows...** → Corrected as suggested.

A2

- Line 518: **("uncertainty" is misspelled.)** → Corrected.

Figures

- Figure 1: **...after a deep convective event has passed the measurement site. On the right hand side the...** → Corrected as suggested.

– Figure 5: **Why do the lines corresponding to the ascent/descent profile exceed the gray area, which "marks the measured range of ozone mixing ratios?" Were these two profiles excluded from the definition of the mean and the range?** → Yes, both, the ascending and descending profile of this flight were taken out to be able to see whether it exceeds the range of the other flights.