Dear Editor,

We highly appreciate the editor's careful handing of the manuscript and two anonymous referees' valuable comments. We have addressed the comments point by point (referees' comments in blue and our response in black). Corresponding revisions have been made in the manuscript. A marked-up manuscript version has been uploaded after our response to referees comments.

Authors Reply to Anonymous Referee #1

General comments

1. abstract, line 11: "rotational subsidence" and "descend slowly along a circle" are not well defined or discussed in the text. Rotational subsidence implies that rotation is causing subsidence, which is not the case. And the slow descent is certainly not along a circle, but is rather more helical in shape. Finally, the 2-D projection of the trajectory is more elliptical than circular.
The authors rewrote this sentence "secondly via rotational subsidence, during which air parcels descend slowly along a circle following the anticyclone flow within a timescale of one week." to "secondly via transport following the clockwise wind flow of the ASM."

2. p. 2 line 21: "...large-scale slow upward circulation within the ASM anticyclone..." would appear to contradict the subsidence noted above and seen in the trajectory calculations. Can this be clarified?
We cited "...the large-scale slow upward circulation within the ASM anticyclone..." in the introduction part. The slow upwelling usually occurred over the eastern part of the ASM anticyclone, and the down welling usually occurred over the western and northern part of the ASM anticyclone. These results are based on many years of model data. We try to describe the second cyclone case using backward trajectories. From the case 2, we noticed that the large subsidence occurred in altitude levels over the northern edge of the ASM anticyclone.

3. p. 3 line 20: grammar "will be displayed".
The authors removed these three words.

4. p. 4 section 2.2: There are surely uncertainties in the ERA-I wind fields as well as with the use of a 1×1 regular grid for the winds, which must be interpolated to the precise location of the balloon's flight path. These uncertainties likely increase with time going backwards. Some attempt should be made to quantify these uncertainties on the back trajectories and how this may affect the conclusions that sampled air masses originated from the boundary layer beneath tropical cyclones.
We totally agree with the referee's comment about the uncertainties of interpolation to the location of the balloon's flight path using the ERA-Interim data. The probability distribution function of the error of the lagrangian model is estimated by (Liu, 2009). Fig. 1 shows that there are minor gradients in the potential temperature distribution within 10−day. Following the referee's advice, we run a bundle of trajectories around the location of the balloon measurement (Fig. 2). After analysing the backward trajectories of (i-Δi,j), (i,j-Δj), (i+Δi ,j), and (i,j+Δj) around the location of balloon (i,j) (Δi=Δj=1 degree), we find the backward trajectories of target air parcels are nearly consistent with the trajectories around the balloon sites (Fig. 3). Considering above description, we all thought that the error caused by interpolation would have a mirror impact on our conclusion.

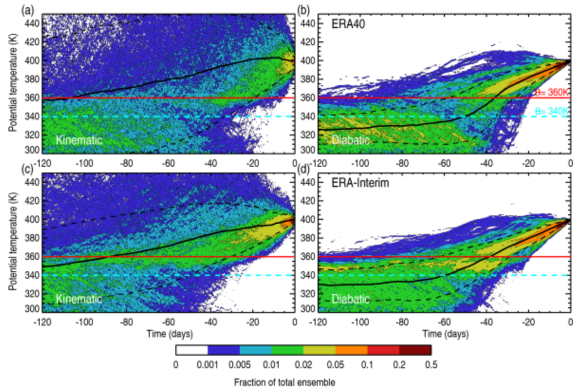

Figure 1. From Liu (2009); Probability distribution function of trajectory potential temperature as a function of time since initialisation for winter 2000−2001. Trajectories are initialised on 400 K between (30° S, 30° N) and integrated backwards in time. Only trajectories which remained within (45° S, 45° N) are included in the plot. Overlay is the ensemble mean potential temperature (black solid line) with one standard deviation (black dashed lines). The 360 K potential temperature level (approximate level of zero clear sky radiative heating) and 340 K potential temperature levels are plotted.The panels illustrate results for ERA-40 kinematic trajectories (top left), ERA−40 diabatic trajectories (top right), ERA−Interim kinematic trajectories (bottom left) and ERA-Interim diabatic trajectories (bottom right).

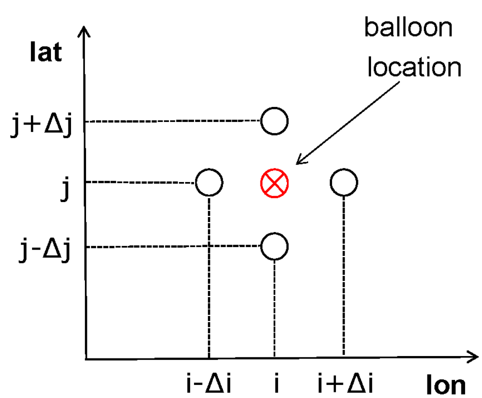

Figure 2. Parcels around the location of balloon measurement (i,j) in 1×1 degree distance.

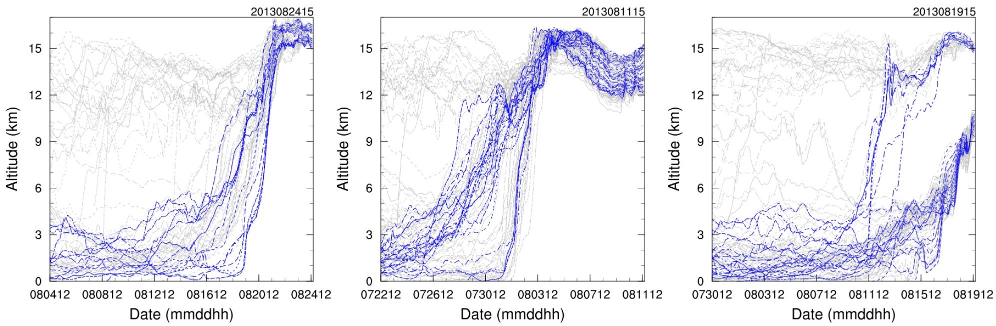

Figure 3. 20-day backward trajectories of target air parcels initialised on 24 August, 11 August, and 19 August 2013. Blue/grey lines mark the backward trajectories of (i,j)/(i-Δi,j), (i,j-Δj), (i+Δi ,j), and (i,j+Δj).

5. p. 5 line 6: This definition of the top of the lower troposphere as zeta <190 K is unfamiliar to me and possibly to other ACP readers. It would be useful to clarify this.

In the lower troposphere, the 190 K is not the isentropic coordinate. It is the hybrid vertical coordinate of the CLaMS model. We added the vertical coordinate information “A hybrid vertical coordinate ζ is employed in the CLaMS model. The isentropic coordinate θ is used when the pressure is less than 300 hPa, and a pressure-based orography-following coordinate is used when the pressure is higher than 300 hPa” in the model part. Pommrich et al. (2014) gave the vertical coordinate ζ definition as follows:

$$\zeta(p) = f(\sigma)\cdot\theta(p,T(p)), \sigma = \frac{p}{p_s}, p_s = surface\ pressure \tag{1}$$

with

$$f(\sigma) = \begin{cases} \sin(\frac{\pi}{2}\frac{1-\sigma}{1-\sigma_\tau}) & \sigma > \sigma_\tau \\ 1 & \sigma \le \sigma_\tau, \sigma_\tau = 0.3 \end{cases} \tag{2}$$

Before we set the top of the lower troposphere, we plotted the altitude and zeta relationship of the 20th backward trajectories of all balloons in August 2013 (see the Fig. 4). As the Fig. 4 shows, the 190 K level in the model is about 3 km. We selected 190 K as the top of the lower troposphere in our manuscript.

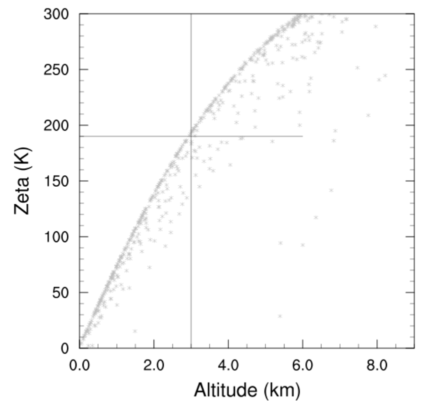

Figure 4. The altitude and zeta relationship.

6. p. 5 line 15: The definition of a cyclones intensity in terms of a pressure is not a standard practice (also used in Table 1). Intensity is usually defined by maximum surface wind, or the Saffir-Simpson scale or equivalent. The pressures quoted must be something like the minimum surface pressure or the average over some time interval of the central eye pressure. I suggest this be reworded and that the caption for Table include details on what this pressure represents.

We replaced the “intensity” with the “lowest pressure” in Table 2. The pressure data from the Japanese website is the minimum surface pressure of a cyclone. The wind speed used here is the ten minutes mean wind speed.

7. p. 6 lines 28−29: There are some potential problems with tropospheric PV that should be noted. While PV is an appropriate tracer for the stratosphere, it is not as well conserved in the troposphere and there are likely large uncertainties in tropospheric PV in ERA-I that might be important for this analysis. Furthermore, it is not clear that showing PV in Fig 4 actually adds new information to the case studies.

We agree with the referee’s comment about the tropospheric PV. Before we submitted this manuscript, we discussed about Fig. 4. However, Fig.4c is very important to explain the high ozone concentration of the tracers in upper troposphere and lower stratosphere. The high ozone concentration of air parcels is consistent with the high PV. We also plotted the PV contour in Fig. 7b to show that a stratospheric intrusion process occurred during typhoon Utor. For those reasons, we thought that the mixing process maybe occur during typhoon Utor period. We thought it was necessary to add the Fig. 4.

8. p. 7 line 2: “...observed right over the typhoon’s track over the Western Pacific...” This statement implies that Fu et al (2013) observed typhoon Trami, when in fact they observed a different typhoon, Hai-Tang in 2005. This should be clarified.

We revised this sentence to “Our findings are in accordance with study by Fu et al.(2013) showing that low ozone mixing ratios in altitudes higher than 200 hPa were also observed right above the track of typhoon Haitang in 2005 over the Western Pacific using Aura’s Ozone Monitoring Instrument data. ” to avoid confusion.

9. p. 7 line 32: “...the average of vapors...” Total ozone is usually used to refer to the vertical column abundance, but I do not think that is what is meant here.

The referee is correct. We replaced “ total ozone profile” with “mean ozone profile” in this sentence.

10. p. 8 lines 9−10: Last sentence of this paragraph is incomplete and unclear.

The authors rewrote the sentence “Explaining air parcels maintain high ozone concentration at an altitude of 15 km (Fig. 3h)” to “Both high PV values and high concentrations of ozone of the air parcels at an altitude of 15 km (see Fig. 3h) indicate a stratospheric origin of theses air parcels (more details see Fig. 7b and discussion in section 4.2)”.

11. p. 9 line 1 "...the potential temperature of parcels increases slowly." In convective uplift, theta usually changes on a relatively rapid timescale (hours, not days). Please clarify.

We tried to explain the rapid uplift process associated with convection in case 1 and case 3, but we found it was too difficult to state this process in case 2. Only three parcels show strong uplift process in case 2. The potential temperature of most parcels in case 2 increases slowly. We thought that the parcels with strong uplift process could not arrive at our observation site. In contrast, the CLaMS model catch the updraft associated with the slow uplift process. As a result, the potential temperature of parcels increases slowly.

12. Figure 8 caption: The white contours are probably vertical velocities from ERA-I, but this should be noted in the caption along with the units and contour intervals.

The white contours are vertical velocities from ERA-Interim reanalysis data. We removed this figure and added a new figure about the OLR.

Authors Reply to Anonymous Referee #2

Specific comments

1. P1/L11: What is rotational subsidence?

The authors rewrote this sentence “secondly via rotational subsidence, during which air parcels descend slowly along a circle following the anticyclone flow within a timescale of one week.” to “secondly via transport following the clockwise wind flow of the ASM.”

2. P2/L24: “Strong tropical cyclones in the...”. This sentence is not necessary in the manuscript.

The authors removed this sentence.

3. P5/L1-3: “The trajectories...” The sentence is not clear. Please rephrase it.

We deleted the sentence “The trajectories were calculated with vertical velocities from the diabatic heating rate” and added this sentence “A hybrid vertical coordinate ζ is employed in the CLaMS model.”.

4. P7/L32: remove “total ozone profile” with “mean ozone profile” and following the same throughout the manuscript.

We replaced “total ozone profile” with “mean ozone profile”.

5. P8/L35-P9/L1: “During uplift process...” Provide a reference.

We deleted this sentence “During the uplift process, the potential temperature of parcels increases slowly.”. And we rewrote the former sentence from “Parcels from the marine boundary layer are lifted by updraft associated with tropical storm Jebi and are then entrained into the southeast edge of the ASM anticyclone (Figs. 6a and b).” to “ Parcels from the marine boundary layer are lifted by updraft associated with tropical storm Jebi with potential temperature increasing slowly and are then entrained into the southeast edge of the ASM anticyclone (Figs. 6a and b).”

6. Fig.8: What indicate the white contours? It should be mentioned in the text as well as in the figure caption. I am hard to find any discussion about it. White contours can be removed if necessary discussion is not included in the manuscript.

The white contours are vertical velocities from ERA-Interim reanalysis data. We removed this figure and added a new figure about the OLR.

7. Fig.8: Authors are discussing about the deep convective clouds, then lidar backscattering from CALIPSO cannot be used. CALIPSO is good for thin/cirrus cloud. CloudSat data could be helpful for estimation of penetration height of convective clouds. Fig.8 can be omitted. Alternative, brightness temperature or OLR can be useful to estimate (indirect) the penetration height of convective cloud.

We also thought that it was good to use the CloudSat data to highlight the deep convection. After checking the CloudSat orbit, we found that the track of the CloudSat was not on the right above the tropical cyclones Trami and Jebi (Fig. 5). In contrast, the CALIPSO orbit track was right above the tropical cyclones, and caught the cirrus cloud on top of cloud band. In our new manuscript, we added the AIRS OLR data to estimate the convective activities (Fig. 6).

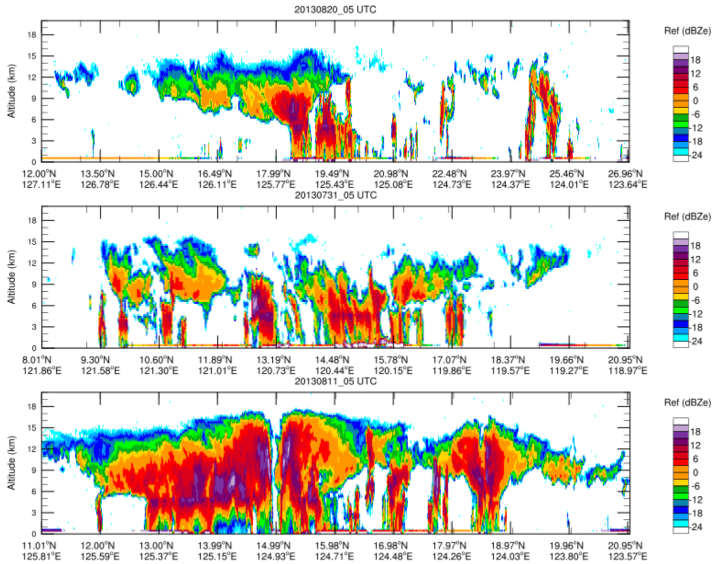

Figure 5. The back scattering reflectivity along the CloudSat orbit tracks.

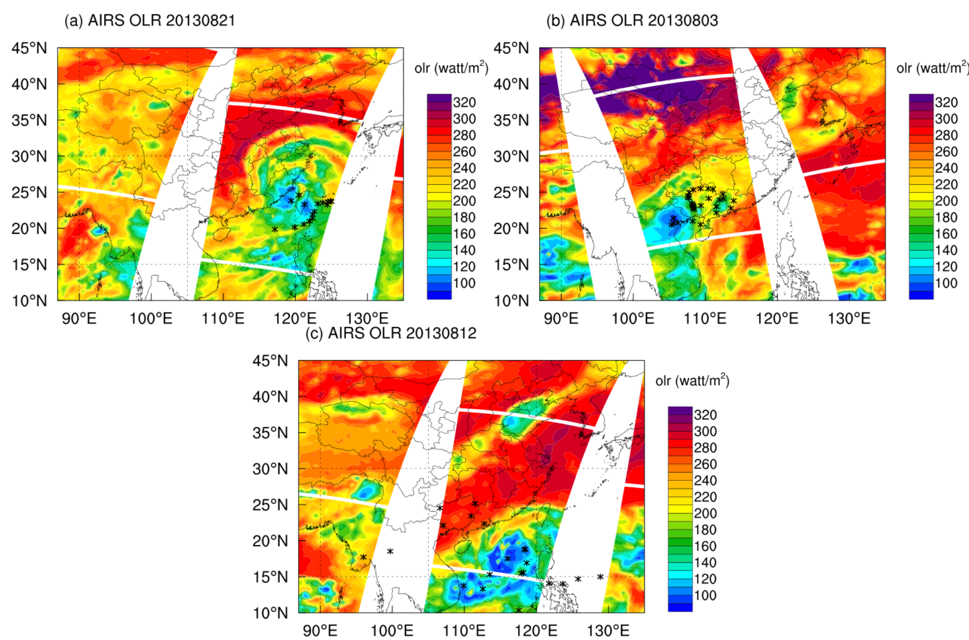

Figure 6. Outgoing longwave radiation from AIRS satellite on 21 August, 03 August, and 12 August. The asterisks mark the locations of parcels with corresponding time.

8. Fig.9: Figure caption meaning is not clear. In this fig. legends:“Ozone−obs, ozone−typhoons, ozone−no typhoons” need to be explain properly in the text.

The authors added the detailed explanation about the “Ozone−obs,ozone−typhoons,ozone−no typhoons” in caption. See

“The relative frequency distribution of all ozone profiles observed in August 2013 (ozone−obs, in black) and ozone with (ozone−typhoons, in red) or without (ozone−no typhoons, in blue) the influence of tropical cyclones with respect to potential temperature layer of (a) 370−380 K, (b) 360−370 K, (c) 350−360 K, and (d) 340−350 K.”

9. Fig.10: (Figure caption) : “The relative humidity...”. Is it “2013” or “August 2013”?
The relative frequency is in August 2013. We added the “August” to the caption.

10.Table 1: Burst altitude of balloon can be omitted in the table.
We removed the column of burst altitude in Table 1.

11. Additional analysis of vertical velocity (altitude-time cross-section over Lhasa) using reanalysis data will be helpful to identify the updrafts and down drafts over the campaign site.
The main up-draft of air parcels occurred over the Western Pacific then subsequently these air parcels are horizontally transported to Lhasa site. We also tried to plot Fig. 7 according to the referee’s comment.

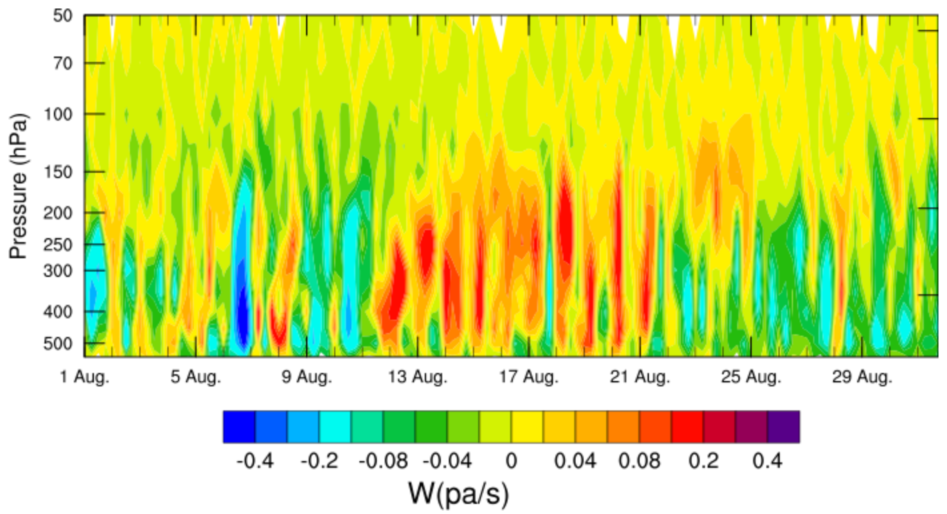

Figure 7. The time series of vertical velocity from Era-Interim over Lhasa in August 2013.

References

Liu, Y. S.: Lagrangian studies of troposphere-to-stratosphere transport, Ph.D. thesis, University of Cambridge, 2009.

Pommrich, R., Müller, R., Grooß, J.-U., Konopka, P., Ploeger, F., Vogel, B., Tao, M., Hoppe, C. M., Günther, G., Spelten, N., Hoffmann, L., Pumphrey, H.-C., Viciani, S., D'Amato, F., Volk, C. M., Hoor, P., Schlager, H., and Riese, M.: Tropical troposphere to stratosphere transport of carbon monoxide and long-lived trace species in the Chemical Lagrangian Model of the Stratosphere (CLaMS), Geoscientific Model Development, 7, 2895–2916, doi:10.5194/gmd-7-2895-2014, http://www.geosci-model-dev.net/7/2895/2014/, 2014.