Anonymous Referee #1 Comments on the manuscript

(…) the manuscript needs moderate revision before publications. My specific comments are following under :

(1) The authors have shown a significant increase in ozone concentration in the proxy of dry air and low CO concentration which occurs in the middle and upper troposphere. I do agree with the authors that the enhanced ozone is primarily associated with the passage of typhoon. But my main concern, even the authors themselves have stated that lightning associated with a typhoon may cause enhancement of ozone. Then how sure we are that the observed enhanced ozone in the middle and upper troposphere is of stratospheric origin. <u>I suggest doing a back-trajectory analysis</u>.

> Retrotrajectory analyses have been added for typhoons Nepartak, Nida and Megi. FLEXPART Lagrangian dispersion model(Stohl *et al.* 2005) has been used in order to determine the origin of ozone-rich layers identified with IAGOS pseudo-vertical soundings. As discussed in the revised version, the backward plumes of particles initiated within these layers reveal a probable stratospheric origin 2.5 to 4 days before IAGOS observations. Associated Figs. 8 (for Nepartak), 11 (for Nida) and 14 (for Megi) have been added in the revised version.

(2) Potential vorticity (PV): PV is a dynamical tracer for stratosphere-troposphere exchange (STE) processes but in the absence of diabatic heating and frictional forces. During a typhoon, the convective tower will have a high value of PV, which is generated due to latent heating associated with it. A detailed study on the evolution of PV structures associated with a typhoon has been carried out by Grad et al. (2011). Thus one needs to take caution during a typhoon while linking high PV value as the air of stratospheric origin. However, Leclair De Bellevue et al. (2007) have mentioned that latent heat can be negligible outside the convection core in the upper troposphere. In this aspect, a detailed discussion is required in the manuscript.

> A sentence has been added at the end of 2.3. « The use of potential vorticity » : In the upper troposphere outflow layer, at large distances (≥500 km) from the cyclone core, where diabatic heating and friction are small, PV must be nearly conserved following air parcels and may be a good indicator of large-scale horizontal and vertical motions (e.g. Molinari et al., 1998 ; Leclair de Bellevue et al., 2007).

(3) There may be a thermal sensor in the aircraft. Thus it will be good to show the thermal structure (maybe temperature inversion).

> We have compared the vertical profiles of relative humidity, ozone and carbon monoxide contents (previous Figs. 5, 8 and 11), with the vertical profiles of pressure, temperature T and potential temperature θ (see the attached Profiles_RHO3CO+PTTH_Nep+Nid+Meg.pdf). No systematic association could objectively be found between the base and the top of the dry and ozone-rich layers (orange dashed lines), and the vertical gradients of T or θ. Nevertheless, small changes in dT/dz and dθ/dz can be seen for profiles NEP-1 at 2.2 km altitude, NEP-2 at 3 km, NEP-6 at 5.4 and 7.2 km, NID-5 at 3.2 km, MEG-2 at 9 km, MEG-4 at 6.4 and 8,4 km, MEG-5 at 4.5 km. Considering this meager evidence, this discussion was not included in the revised version.

(4) Near Taipei, there is an ozonesonde launching station at Banqiao (25 degree N, 121.3 degree E) or elsewhere. It will be quite supportive of the IAGOS observations if one can show the ozonesonde profiles during any of the typhoon cases (till 3-4 days after typhoon landfall). This will also validate IAGOS data in the convective situation.

Unfortunately, the Central Weather Bureau did not conduct ozonesonde measurements in June-October 2016.

(5) The tropopause structure has a key role in STE and also during a typhoon. Thus, I suggest authors take nearby radiosonde data (twice a day) to show the thermal structure and also the wind information.

Indeed, these radiosounding data reveal that the « cold point tropopause » decreased substantially when the dry zone arrived over Taiwan (Typhoon Nepartak : from 18.2 km on 5 July 00 UTC to 16.2 km on 6 July 00 UTC, Typhoon Megi : from 17.3 km on 25 September 00 UTC to 16.1 kmon 26 September 00 UTC, no significant change was found for Tropical Storm Nida). These remarks have been included in the revised version.

(6) Ratnam et al. (2016) have shown a significant increase in upper tropospheric ozone associated with north Indian Ocean tropical cyclones. It is shown that a particular sector of the cyclone has high ozone and low humidity. I also suggest the authors look into this possibility in the existing data set and discuss the results.

A reference to Venkam Ratman *et al.* (2016) has been added in the revised version.

(7) Figs.4, 7 and 10 can be combined.

Figs. 4, 7 and 10 have been combined into Fig. 5 in the revised version.

We thank Referee #1 for the constructive comments which helped to clarify the submitted version.

Referee #2 (Laura Pan) comments on the manuscript

(…) I recommend the manuscript to be accepted for publication largely as it is, with some minor corrections. I have some suggestions, optional for the authors, which may enhance the take home message of the work.

<u>*Minor changes and corrections*</u>:

1. Page 4: line 142: "where F is.." => No F in the equation

Equation (2) has been corrected :
$$\frac{\partial\, PV}{\partial t} + (\boldsymbol{v}.\boldsymbol{\nabla})\, PV = \frac{1}{\rho}(\boldsymbol{\nabla}\times\boldsymbol{F}).\boldsymbol{\nabla}\theta + \boldsymbol{\nabla}\dot{\theta}$$

2. Page 7, Line 267: this sentence needs to be revised. The word "determine" is too strong. ". . .two more cases to highlight the common features"?

« *determine* » has been changed in « *investigate* » in the revised version.

3. Page 8, you made a number of references of "low CO" to values of ~100 ppbv, which is somewhat problematic. Overall, the CO signatures are weak.

« *low CO values* » have been replaced by « *CO values less than 100 ppbv* » in the revised version.

4. Figures:
* Figure 1 labels are too small to read on a print page.
Figure 1 has been changed (see below) in the revised version.
* Fig 4: "Yellow box"
The change has been made in the revised version.

<u>*Additional suggestions*</u>:

1) Figure 1 serves a purpose but could have much more information content. As shown in later profiles, the individual profiles could be very structured. These mean profiles,however, are kind of uninteresting. Suggest you to try "box-and-whisker" plots with distribution in layers instead of mean and standard deviation.

In the revised version, Fig. 1 shows boxplots of RH, O3 and CO for each 500-m deep layer between the surface and 12 km altitude. Comments have been changed accordingly.

2) Figure 2&3 could use some color adjustment to highlight the features you want readers to see. Panels C in particular. Fig. 2C could be clearer if color changes for CO above/below 100 ppb and 50 ppbv. Fig. 3C should show color change at 0 to highlight direction change
The color codes for Figs. 2c (IAGOS CO), 3b (ERA-5 PV) and 3c (ERA-5 W) has been changed in the revised version in order to highlight contrasts.

3) After presenting the details of three cases, it would be more satisfying to have a summary figure quantifying the layer influenced by the stratospheric air, its vertical extent and the amount of ozone enhancement. It is possible to do this using all 18 profiles but present the data in the tracer-tracer space. For an example, see Fig. 3 of Randel et al., 2016. If you have a large anti-correlation between O3 and RH, it would be a strong support for transport. The part of the tracer space with positive ozone anomaly and unclear anti-correlation with RH may indicate other mechanisms, including lightening NOx facilitated ozone production.

In the revised version, a figure (Fig. 4) has been added to show the two-dimensional distribution of O3 and RH from 4 to 12 km altitudes for 56 vertical profiles derived from IAGOS measurements during take-off from and landing to TPE at less than 1200 km from the centers of 7 storms that passed relatively close to TPE during July to October 2016. As discussed in the revised version, two populations can be identified : ozone-rich (50 to 100 ppbv) and dry (RH < 50%) air represents 35.8 % of the measurements, ozone-poor (<50 ppbv) air with various RH values (20 to 100 %) accounts for the remaining 64.2 %. The absence of measurements with positive ozone anomaly and high relative humidity indicates that lightning-induced nitrogen oxides are probably not the main cause of ozone production here. These results bear some resemblance with those Randel et al. (2016) derived from vertical tropospheric profiles collected during the "Convective Transport and Active Specied in the Tropics" experiment from Guam (14°N, 145°E) in January-February 2014. They also observed dry air (RH < 20%) with enhanced ozone (40-80 ppbv) contents within layers between 3 and 9 km altitudes, which they linked to quasi-isentropic transport from the extratropical UTLS. These data were obtained in the descending branch of the Hadley cell, with only occasional deep convection, which contrasts with the moister and stormier conditions that prevailed during July to October 2016 near Taiwan.

We thank Laura Pan for the constructive comments which helped to clarify the submitted version.