Comments of the revised manuscript entitled, 'Strong downdrafts preceding rapid tropopause ascent and their potential to identify cross-tropopause stratospheric intrusions' by Chen et al.

The authors described 16 cases of stratospheric intrusion due to various synoptic cases over Beijing using MST radar. The results are supported by AIRS ozone observations along with ECMWF reanalysis and HYSPLIT back/forward trajectory model. This is certainly an interesting topic and the present scientific communities have an eye to understand the stratospheric intrusion and its impact on global ozone budget and earth's climate. The manuscript is potential but need substantial revision before publication.

Response: We really would like to thank the reviewer for giving us constructive and supportive suggestions which would help us to improve the quality of the paper. We also really would like to thank the reviewer for pointing out our deficiencies. We have followed the reviewer's suggestion and substantial revisions have been made in the revised manuscript. We have repeated the comments of the reviewer in italics before our response. The revised manuscript with tracked changes (highlighted in red font) is provided.

Specific points are following under:

(1) Title of the manuscript needs to change. How downdrafts can increase the tropopause level? It should be in other way, updrafts increase the tropopause level. Instead, as suitable tile can be chosen, something like : Rapid modulation of tropopause due to synoptic disturbances: : :: : :: : :: : :.

Response: Thank you very much for pointing out the comment. But, our attention have been focused on the key point that the strong downdrafts preceding the rapid tropopause (downdrafts occurred just before the rapid radar tropopause ascent) ascent have a strong potential for identifying possible intrusions. We didn't show and indicate that the downdrafts can increase the tropopause level. According to a detailed case and other 20 cases, observations have shown that the downdrafts followed by rapid radar tropopause ascent can be used to infer the occurrence of intrusions of stratospheric origin.

(2) L19 : How authors define a strong updrafts, is it above 0.8 m/s. During many MST radar experiments, we observed vertical velocity up to _12 m/s. Anything above _0.8 m/s is considered to be presence of convective system but certainly not strong updrafts (which could be above _2 m/s).

Response: We think the reviewer refers to the definition of strong downdrafts, which occurred just before the rapid RT ascent and is the key point. Three criteria have been proposed in the manuscript to define the case of strong downdrafts followed by rapid RT ascent. That is: 1) the amplitude of the RT ascent should exceed 0.6 km (four range

gates), 2) vertical velocities of the RT ascent excess 0.1 km/h, 3) the downdrafts occurred preceding the RT ascent should >0.5 m/s, and the height region of the downdrafts should pass through the RT layer. The background weather condition have been discussed from 500 hPa geopotential height, time series of surface hourly meteorological measurements, and maps of Outgoing Longwave Radiation. Besides, no matter where the downdrafts accurately originate, we indeed found that the strong downdrafts preceding rapid RT ascent can be used as a valuable predictor for possible stratospheric intrusions.

(3) L22 : 'destroyed' is not a correct word to use, instead 'stability of the tropopasue is weakened as observed by MST radar's SNR'

Response: Yes, you are right. Thank you very much for pointing out the deficiencies. We have followed the reviewer's suggestion and changed the associated sentence in the revised manuscript. It has changed to "Within the height region of the downdrafts, the stability of the radar tropopause seems to be weakened".

(4) L25-27 : "According to : : :: : :.intrusions". This sentence is not necessary in the abstract.

Response: We really would like to thank the reviewer for pointing out the deficiency. The corresponding sentence has been deleted.

(5) L27-31 : "Twenty: : :. Discussed". These sentences can be combined and shorten.

Response: Thank you for pointing out the comment. We consider that the two sentences need to be separated. The former one introduce the other 20 cases and their relationship with the occurrence of possible stratospheric intrusions. The latter one is about the statistics of the downdrafts.

(6) L31-33: "The observations: : :: : :.observations". Authors cannot conclude.

Response: We really thank the reviewer for the valuable comment. A major result of our study is the observations of strong downdrafts just preceding the rapid tropopause ascent, which serve as a valuable predictor for possible stratospheric intrusions. This potential value is verified from a detailed case and other 20 typical cases.

(7) L45 : How wind speeds plays a important role in STE? Is it shear generated turbulence ?

Response: Thank you for pointing out the comment. The sentence "Consequently, the natural stable tropopause layer, characterized by strong gradients of trace constituents and wind speeds, plays an important role in stratosphere-troposphere exchange (STE) processes" means that the stable tropopause layer plays an important role in STE

processes, rather than the wind speeds. The tropopause layer is a significant barrier for STE (Mahlman, 1997).

Mahlman, J. D.: Dynamics of transport processes in the upper troposphere. Science, 276(5315), 1079-1083, 1997.

(8) L48-50 : Sentence having repeating words. Few latest references are needed.

Response: Yes, you are right. Thank you very much for pointing out the deficiencies. The repeating word is "long-term", we have changed the sentence to "From a long-term point of view, the seasonal variation of the tropopause height determines the seasonal variation of the flux of stratospheric air into the free troposphere (Appenzeller et al., 1996)".

(9) L64-66 : Sentence is not clear. Needs rephrasing.

Response: Thank you for pointing out the comment. The corresponding sentence has been changed to "Although photochemical production within the troposphere is the main source of tropospheric ozone, the influence of the downward stratospheric intrusions on the tropospheric ozone content cannot be ignored (Oltmans and Levy II, 1992; Monks, 2000; Stevenson et al., 2006)".

(10) L69-70: More recent references need to be included. For example, increase in surface ozone is observed during (a) mesoscale (Grant et al., 2008), and synoptic (Das et al., 2016; Jiang et al., 2015;) scale convective systems.

Response: We have followed the reviewer's suggestion, and three more references have been added. Please see the corresponding sentence "sometimes even deep to the surface (e.g. Gerasopoulos et al., 2006; Grant et al., 2008; Lefohn et al., 2011; Jiang et al., 2015; Das et al., 2016;)" in the revised manuscript.

(11) L84-86: As the sentence is written, 'Ozonesonde' is a tracer to detect the stratospheric intrusion. Sentence should be rewritten for better clarity.

Response: Yes, you are right. The corresponding sentence has been changed from "Among them, balloon-borne ozonesonde sounding are without doubt one of the most appropriate tools, but is limited by coverage (He et al., 2011) and not possible to obtain continuous profiles with fine temporal resolution" to "Balloon-borne ozonesonde sounding is an effective tool to make measurements of ozone with high vertical resolution, but is limited by coverage (He et al., 2011) and temporal resolution."

(12) L93 : It is too old to say that "Small scale intrusion are still remain uncertain" by referring

Holten et al. (1995). There are many new research works and results are discovered in past 23 years. Authors must cite some latest references and what is the present scenario and lacuna in the existing recent literatures.

Response: Yes, you are right. To make it more appropriate, the corresponding sentence ha been changed to "By far, large-scale STE has been widely studied and is fairly well understood, but the details of small scale intrusions still need more researches (e.g. Holton et al., 1995)" in the revised manuscript.

(13) L94-96 : Unclear sentence. Needs to be rewritten.

Response: We have followed the reviewer's suggestion and changed the corresponding sentence to "Kumar and Uma (2009) reported that the shortage of direct measurements of vertical winds near the tropopause may be responsible for the lack of fine-scale observations of smaller scale intrusions".

(14) L98 : Tropopause is not directly measured from VHF radar. There is an algorithm from which tropopause is detected using backscattering signal. Thus, author must caution, while describing about VHF radar capability.

Response: Yes, you are right. The tropopause height is not measured directly from MST radar, it is retrieved from radar backscattered echo power in vertical incidence. The corresponding sentence has been changed to "Very-High-Frequency (VHF) radars, compared to the tools mentioned above, are capable of continuously monitoring the atmosphere under any weather conditions and detecting tropopause height from backscattered signal with both high temporal and spatial resolution". The details of the definition of radar tropopause is presented in section 2.1.

(15) L99-100 : "24 hours per day". This is not a scientific statement. Instead, author can write " VHF radar can be continuously used to detect tropopause height from backscattering signal with an internal of 1 hour"

Response: Thank you very much for pointing out the deficiency. The corresponding sentence has been changed to "Very-High-Frequency (VHF) radars, compared to the tools mentioned above, are capable of continuously monitoring the atmosphere under any weather conditions and detecting tropopause height from backscattered signal with both high temporal and spatial resolution".

(16) L101 : Reference is essential.

Response: Thank you very much for pointing out the deficiency. Two references have been added in the revised manuscript. Please see the sentence "During the past two

decades, VHF radar measurements were commonly used to assist to study the stratospheric intrusions (e.g. Hocking et al., 2007; Das et al., 2016)."

(17) L102 : : : :..in many aspect: : :: : : : Authors must list few example.

Response: To make it more clear, we have changed the corresponding sentence to "However, it still remains uncertain in many aspects when using only the VHF radar to identify intrusion events, especially the criteria for the identification".

(18) L106 : 'radar-derived tropopause: : :: : :..'. Along with the tropopause height, enhancement in the radar backscattering signal is essential to diagnosis the stratospheric intrusion.

Response: The corresponding sentence has been changed to "They found that the rapid ascent in RT altitude (>0.2 km/h) can be a valuable diagnostic for possible stratospheric intrusions and the RT height started to ascent just when the stratospheric air intruded across the tropopause layer".

(19) L129 : '0.5 h time resolution' this is not the time resolution of radar measurements. It is the averaging time (post processing).

Response: In order to achieve simultaneously measurements from troposphere, lower stratosphere, and mesosphere, the radar is designed to operate in three separate modes: low mode (designed range 2.5-~12 km), middle mode (10-~25 km), and high mode (60-~90 km) with height resolutions of 150, 600, and 1200 m, respectively. Under the daily routine normal operation, 15-min break is followed by the 15-min operation cycle (5 min for each mode). Indeed, this 5 min operation for each mode in 5 beams (providing a profile of radar measurements) is the averaging time after post processing. However, the time resolutions of the low, middle, and high mode measurements are all 0.5 h.

(20) L141 : It is not correct to say that "strong potential temperature gradient". "Strong temperature inversion" is the correct word.

Response: Thank you for pointing out the comment. In fact, we meant the strong inversion in potential temperature.

(21) L156 : [2012] & [2014]

Response: Thank you very much for pointing out the deficiencies. The corresponding text have been corrected.

(22) Under AIRS and ERA-I, proper citations are needed.

Response: Proper citation has been added in the corresponding text.

(23) L180 : Replace 'Nov.2000' to 'November 2000', follow it throughout the manuscript. .

Response: Thank you very much for pointing out the deficiencies. We have followed the reviewer's comment and changed the corresponding text throughout the manuscript.

(24) L212-215 : I do not agree with the statement. Generally, during deep convection, humidity increases.

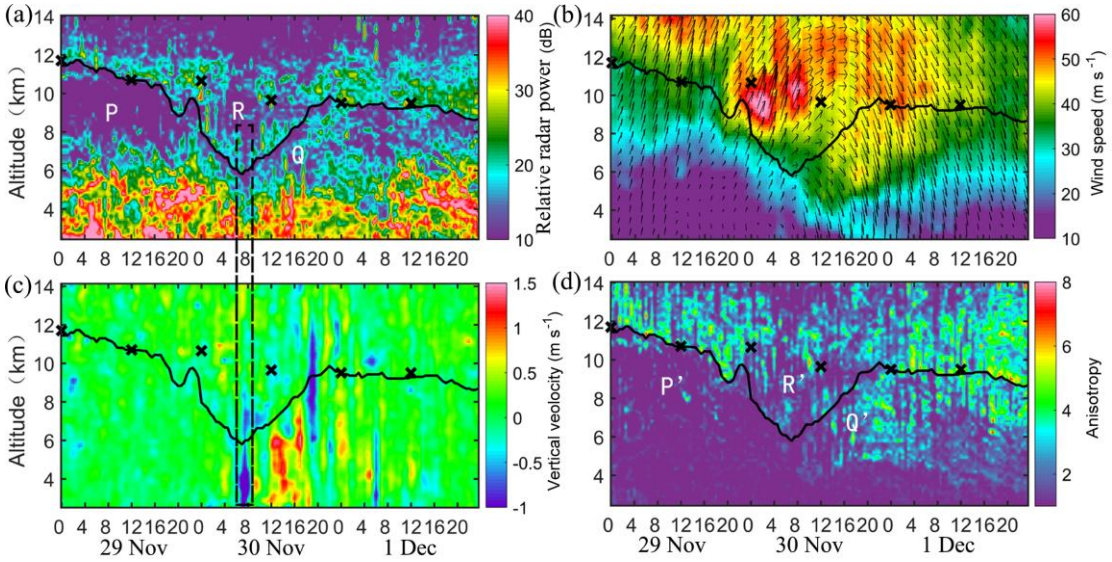
Response: Thank you for pointing out the comment. We actually meant the rapid decrease in humidity under the background condition of cut-off low system, rather than the potential deep convection.

(25) L234 and Fig.5(d) : What does authors mean by 'Aspect sensitivity'. As I understood, it is the difference been zenith and off-zenith but what off-zenith angle and which direction? Is it 10-degree East ?

Response: Yes, you are right. The Aspect sensitivity here means the difference in backscattered echo power between zenith and off-zenith beam. In the manuscript, the radar aspect sensitivity is expressed as the ratio between vertical and oblique (here used the 15-degree north) beam echo power. Please see the sentence "In addition, the radar aspect sensitivity, expressed as the ratio between vertical (p_v) and oblique (p_o, here used the 15-degree north) beam echo power, is mainly caused by the horizontally stratified anisotropic stable air and thus will be used as potential signature of stratospheric intrusions in the troposphere (e.g. Kim et al., 2001)" in the revised manuscript.

(26) Fig.5 : (a), (b), (c), (d) should be level.

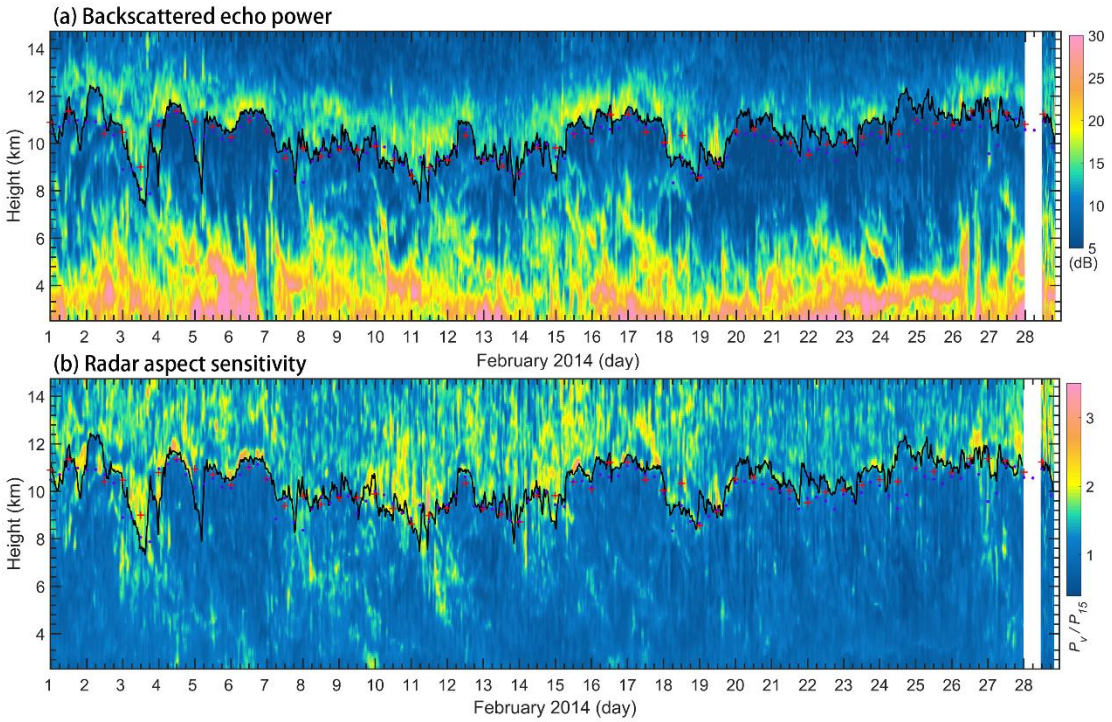
Response: We have followed the reviewer's suggestion and modified the (a), (b), (c), (d) to be level. Please see the picture below.



(27) There is a huge difference between the radar detected and radiosonde detected tropopause. From 5, I could able to see about 2.5 km difference at 12 UTC on 30 November which is absolutely unacceptable (as the authors themselves have mentioned in the manuscript). I would like to suggest the author to relook on the algorithm for the detection of tropopause level by

VHF radar (signal). Authors need to investigate further.

Response: Large difference in height between the radar detected and radiosonde detected tropopause is commonly observed, especially during severe weather conditions. To make the definition of radar tropopause more clear, the sentence about the definition has changed to "Here, the radar-determined tropopause (RT) height is defined as the height (above 500 hPa) where the maximum vertical gradient of echo power located (shown as the orange circle in Figure 1a)" in the revised manuscript. Also shown below is the altitude-time intensity plot of (a) radar backscattered echo power and (b) radar aspect sensitivity for February 2014. The tropopauses determined based on the radar echo definition are shown as a black solid curve. The red crosses and purple dots indicate the location of the Lapse-rate (LRT) and 2PVU tropopause (PVT), respectively. In general, the RT agreed well with both the LRT. However, it is clear to be seen that the differences between the RT and LRT are large (reach to ~1-2 km) on some days especially when the RT experience rapid change (severe weather conditions). The difference of the definitions themselves is to a large degree the main contributing factor. Such as for the cases on 4 and 5 February 2012 when large changes in RT height occurred, a second layer with significantly enhanced echo power is observed above the RT and its altitude of maximum echo power gradient is just well consistent with the LRT (a). According to the definition, the RT well matched the lower part but the LRT often matched the upper part, similar to that observed by Yamamoto et al., (2003) and Fukao et al., (2003).



Yamamoto, M.K., Oyamatsu, M., Horinouchi, T., Hashiguchi, H., Fukao, S., (2003). High time resolution determination of the tropical tropopause by the Equatorial Atmosphere Radar. Geophys. Res. Lett. 30 (21), 2094. http://dx.doi.org/10.1029/2003GL018072.

Fukao, S., H. Hashiguchi, M. Yamamoto, T. Tsuda, T. Nakamura, M. K. Yamamoto, T. Sato, M. Hagio, and Y. Yabugaki, Equatorial Atmosphere Radar (EAR): System description and first results, Radio Sci., 38(3), 1053, doi:10.1029/2002RS002767, 2003.

(28) L241 : Needs reference.

Response: We have followed the reviewer's suggestion and added two references in the corresponding sentence. Please see sentence "It is the difference in definition that contribute most to the large differences, especially under the tropopause fold conditions (e.g. Yamamoto et al., 2003 and Fukao et al., 2003)." in the revised manuscript.

Yamamoto, M., Oyamatsu, M., Horinouchi, T., Hashiguchi, H., & Fukao, S.: High time resolution determination of the tropical tropopause by the Equatorial Atmosphere Radar. Geophysical Research Letters, 30(21), 2003.

Fukao, S., H. Hashiguchi, M. Yamamoto, T. Tsuda, T. Nakamura, M. K. Yamamoto, T. Sato, M. Hagio, and Y. Yabugaki.: Equatorial Atmosphere Radar (EAR): System description and first results, Radio Sci., 38(3), 1053, doi:10.1029/2002RS002767, 2003.

(29) L252-253 : I suggest to estimate CAPE (Convective Available Potential Energy) to confirm the occurrence of convection.

Response: We really would like to thank you for giving us the suggestion. We consider

that the 500 hPa geopotential height, time series of surface hourly meteorological measurements, and maps of Outgoing Longwave Radiation are enough to understand the background condition.

(30) L259-260 : This statement is not fully correct.

Response: Thank you very much for pointing out the deficiency. The corresponding sentence has been changed to "The research by Hocking et al. (2007) has suggested that the rapid ascent in RT height (>0.2 km h-1) can be a valuable predictor for the occurrence of stratospheric intrusions" in the revised manuscript.

(31) L273-277 : larger value of aspect sensitivity cannot be from stratosphere, it is from stratified layers and attributed to the Fresnel reflection/scattering from sharp gradients in the radio refractive index. Thus, it will be mainly from the tropopause. I cannot understand how large value of aspect sensitivity indicates the stratospheric intrusion. If an isotropic turbulence persists, then aspect sensitivity will decrease. Needs further explanation.

Response: Yes, you are right. We mentioned that the abnormal large aspect sensitivity may be a weak signature of the possible intrusions. We have explained that in normal conditions, the aspect sensitivity is usually low in value in the troposphere, due to the presence of isotropic turbulence. The large value in radar aspect sensitivity is mainly caused by reflection from stable atmospheric layer, such as the tropopause or lower-stratosphere. When stable stratospheric air intrudes into the troposphere and without mixing with the surrounding air mass, the intrusions in the free troposphere will be reflected as abnormal large aspect sensitivity. Thus, it is just a potential evidence of possible intrusions. Further strong evidence of the relevant intrusions in dynamical and chemical aspects are presented using satellite AIRS and global reanalysis data.

(32) L281 : replace 'Dec.' with 'December' and throughout the manuscript.

Response: Thank you very much for pointing out the deficiencies. We have followed the reviewer's comment and changed the corresponding text throughout the manuscript.

(33) Fig.7 & L285-286 : I am confused, what actually authors wanted to discuss. Do they want to discuss convective or orographic (as authors mentioned presence of mountain of 1 km north of radar site) generated gravity waves? If so, then it is not sufficient. Either authors need to make a separate section discussing on gravity wave structure (in-depth analysis) or omit this part.

Response: Yes, you are right. We really thank you for the valuable comment and pointing out the deficiencies. Figure 7 and Figure S1 are indeed not essential and need

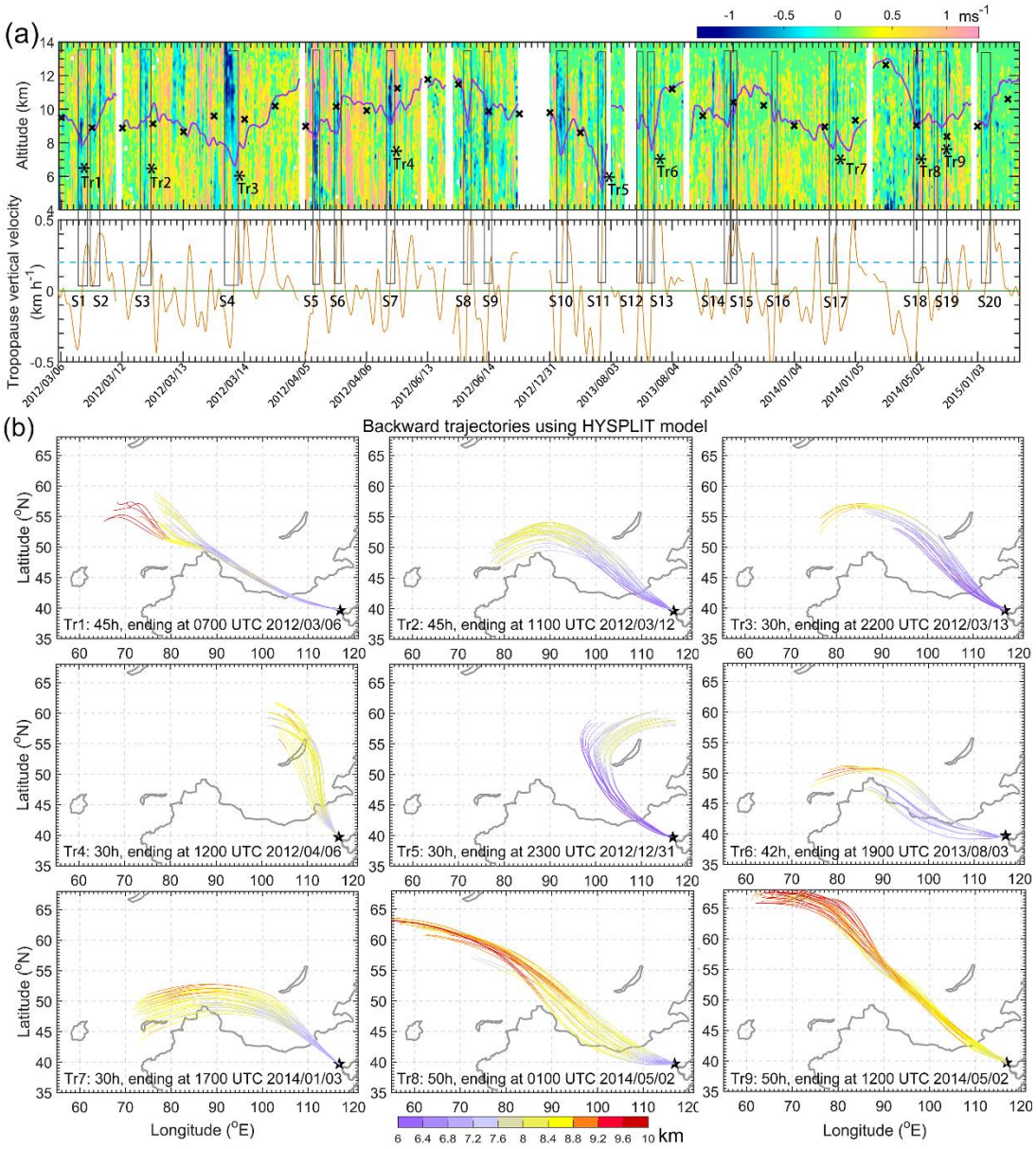
to be deleted. The corresponding text and figures have been modified and deleted, please see the revised manuscript.

(34) L311-314 : From Fig.10, it is seen that the air masses is originated from 7-9 km at 40oN, which is upper troposphere. Thus, the statement of "air of stratospheric origin" is not correct or established here. Authors need to explain this analysis.

Response: Yes, you are right. Thank you very much for pointing out the deficiencies. The statements of stratospheric intrusions are not appropriate for trajectory analysis. The further observations of the AIRS daily 500 hPa ozone distribution is essential to further verify the intrusions are of stratospheric origin. The corresponding statements have been modified and replaced as "downward intrusions". Please see the corresponding text in the revised manuscript.

(35) Fig.12 a: Quality of fig. is poor. Needs better clarity.

Response: Thank you very much for pointing out the suggestion. All the panels in Fig. 12 have modified. Please see Fig. 11b in the revised manuscript. To make the cases identified in Figure 11a more clear, the cases are labeled as S1, S2, S3, …, and S20. The figure is shown below.



(36) One interesting point I could able to find that whenever a synoptic event occurs, the tropopause height decreased to > 9 km (radar tropopause is much more lower to _ 6.5 km), which is a positive point to discuss in the manuscript. I think authors can put more stress in this point while discussing the back-trajectory analysis (see my previous comments). But again the question is that whether the tropopause height can be _6.5 km at 40oN? It is unacceptable fact, which again put a question on the algorithm used for detecting the tropopause height by MST radar. I again suggest authors to relook in this aspect (radar tropopause).

Response: According to our previous response. Large difference in height between the radar detected and radiosonde detected tropopause is commonly observed, especially during severe weather conditions. To make the definition of radar tropopause more clear, the sentence about the definition has changed to "Here, the radar-determined tropopause (RT) height is defined as the height (above 500 hPa) where the maximum

vertical gradient of echo power located (shown as the orange circle in Figure 1a)" in the revised manuscript. Shown below is the altitude-time intensity plot of radar backscattered echo power and radar aspect sensitivity for February 2014. Differences between the RT and LRT are large (reach to ~1-2 km) on some days especially when the RT experience rapid change (severe weather conditions). The difference of the definitions themselves is to a large degree the main contributing factor. Such as for the cases on 4 and 5 February 2012 when large changes in RT height occurred, a second layer with significantly enhanced echo power is observed above the RT and its altitude of maximum echo power gradient is just well consistent with the LRT. According to the definition, the RT well matched the lower part but the LRT often matched the upper part, similar to that observed by Yamamoto et al., (2003) and Fukao et al., (2003). In the manuscript, such difference is also discussed from Figure 6. At 40°N, conditions with tropopause height (both the LRT and the RT) lower than 6.5 km is rare, but it actually exist, no matter whether the difference between the LRT and the RT is large.

(37) L407-410 : Mountain wave is no where discussed in the manuscript. See my previous comment.

Response: Yes, you are right. Thank you very much for pointing out the deficiencies. The discussion about the mountain wave is actually redundant. The corresponding text and figures have been modified and deleted, please see the revised manuscript.

(38) Too many errors in English use, I do not list all that I found, but I hope the authors will carefully improve their writing.

Response: We really would like to thank you for pointing out our deficiencies. We also very sorry for our poor English writing that makes you difficult to read. In the revised manuscript, large revisions have been made, including the issues about the English writing.

References :

Jiang, Y. C., T. L. Zhao, J. Liu, X. D. Xu, C. H. Tan, X. H. Cheng, X. Y. Bi6, J. B. Gan, J. F. You, and S. Z. Zhao (2015), Why does surface ozone peak before a typhoon landing in southeast China? Atmos. Chem. Phys., 15, 13331–13338, doi:10.5194/acp-15- 13331-2015

Grant, Deanne, Jose D. Fuentes, Marcia S. DeLonge, Stephen Chan, Everette Joseph, Paul Kucera, Seydi A. Ndiaye, Amadou T. Gaye (2008), Ozone transport by mesoscale convective storms in western Senegal, Atmos. Envir., 42, 7104–7114,

doi:10.1016/j.atmosenv.2008.05.044

Das, S.S., M. V. Ratnam, K. N. Uma, K. V. Subrahmanyam, I.A.Girach, A. K. Patra,S. Aneesh, K.V. Suneeth, K. K. Kumar, A.P.Kesarkar, S. Sijikumar and G. Ramkumar Influence of Tropical Cyclones on Tropospheric Ozone: Possible Implications (2016), Atmospheric Chemistry and Physics, 16, 4837-4847, doi : 10.5194/acp-16-1-2016