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Interactive comment on "Strong downdrafts preceding rapid tropopause ascent and their potential to identify cross-tropopause stratospheric intrusions" *by* Feilong Chen et al.

Anonymous Referee #2

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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. 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. . . . . (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 $\pm$12 m/s. Anything above $\pm$0.8 m/s is considered to be presence of convective system but certainly not strong updrafts (which could be above $\pm$2 m/s). (3) L22 : 'destroyed' is not a correct word to use, instead 'stability of the tropopasue is weakened as observed by MST radar's SNR' (4) L25-27 : "According to . . . . . intrusions". This sentence is not necessary in the abstract. (5) L27-31 : "Twenty. . . . Discussed". These sentences can be combined and shorten. (6) L31-33: "The observations. . . . . observations". Authors cannot conclude. (7) L45 : How wind speeds plays a important role in STE? Is it shear generated turbulence ? (8) L48-50 : Sentence having repeating words. Few latest references are needed. (9) L64-66 : Sentence is not clear. Needs rephrasing. (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. (11) L84-86: As the sentence is written, 'Ozonesonde' is a tracer to detect the stratospheric intrusion. Sentence should be rewritten for better clarity. (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. (13) L94-96 : Unclear sentence. Needs to be rewritten. (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. (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" (16) L101 : Reference is essential. (17) L102 : ....in many aspect..... : Authors must list few example. (18) L106 : 'radar-derived tropopause.....'. Along with the tropopause height, enhancement in the radar backscattering signal is essential to diagnosis the stratospheric intrusion. (19) L129 : '0.5 h time resolution' this is not the time resolution of radar measurements. It is the averaging time (post processing). (20) L141 : It is not correct to say that "strong potential temperature gradient". "Strong temperature inversion" is the correct word. (21) L156 : [2012] & [2014] (22) Under AIRS and ERA-I, proper citations are needed. (23) L180 : Replace 'Nov.2000' to 'November 2000', follow it throughout the manuscript. . (24) L212-215 : I do not agree with the statement. Generally, during deep convection, humidity increases. (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 ? (26) Fig.5 : (a), (b), (c), (d) should be level. (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. (28) L241 : Needs reference. (29) L252-253 : I suggest to estimate CAPE (Convective Available Potential Energy) to confirm the occurrence of convection. (30) L259-260 : This statement is not fully correct. (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. (32) L281 : replace 'Dec.' with 'December' and 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. (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. (35) Fig.12 a: Quality of fig. is poor. Needs better clarity. (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 $\sim$ 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 $\sim$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). (37) L407-410 : Mountain wave is no where discussed in the manuscript. See my previous comment. (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.

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

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