

Interactive comment on “On the behaviour of the tropopause folding events over the Tibetan Plateau” by X. L. Chen et al.

X. L. Chen et al.

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We would like to sincerely thank Reviewer 1 for her/his considered and largely encouraging comments. We have noted the main queries that she/he raised and will be addressing them point by point in our response. The mentioned literatures are no doubt helpful to this work and have been studied and cited when relevant as well;

Comment: It is a pity that the data are not complete and simultaneous for all the stations. In some parts of the manuscript it would be very useful to have such data available for comparison purposes. However in some cases because of such luck it is simply necessary to trust the authors and other parameters/datasets. If fact the campaigns of measurement give very random coverage with 30 cases with data and 24 cases without data (Table 2).

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Response: From beginning, the experiment was not designed to do research on UTLS. Only parts of radiosonde stations in Fig. 3 of Xu et al. (2009) were supported to join the intensive observation due to economic and labor costs. When this insufficient dataset were used to this study, we also feel a bit pity. Meanwhile for the large area of Tibetan Plateau, this experiment should be the most comprehensive radiosonde network which uses the most advanced sounding system up to date. Rarely papers about UTLS over the Plateau were published. Considering the significance of the Tibetan Plateau to STE of the North hemisphere, this forms the central objective of our paper.

Comment: Also in some parts the manuscript lacks of crucial information and for example the section 2, which is very close to a data and methods section is very self-contained, maybe too much. The authors make some good points about the structure of the multiple tropopauses over the Tibetan Plateau, but in some parts the conclusions are too optimistic and not completely supported by the results.

Response: In order to clarify the relationship between tropopause folds and multiple tropopause, we'll also upload the time series of same picture as Fig. 5 to show the structure variation of UTLS during multi-tropopause and single tropopause period. About optimistic points as both referees suggested, we pondered on it seriously and adjusted related conclusions. Such as Randel and Añel 'lower' estimates multi-tropopause frequency is not exactly right. And without discussing other effects on the ozone distributions, it's not exactly right to say the intrusions can have determinative role on the total ozone. We also revised the discussion part in order to make more scientific conclusions.

Comment: Finally, the inclusion of the code used to compute the tropopause is a very nice and not usual thing which deserves recognition.

Response: Thanks for your recognition. For easy use by other people, the function has been upgraded with some detailed information on key notes and how to use it. We hope other people will use the same code to identify the height of tropopause

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Comment: To summarize, this work is a nice local study from a small set of measurement campaigns which confirms previous results existing in the literature. The authors claim that the percentages of multiple tropopauses found is greater than previously reported but however don't have into account that their results are very local and limited in the time. Therefore, in some cases a good comparison is not possible as previous studies undertook the effort of the characterization from a more global perspective and in some cases with mean annual or monthly values.

Response: We also recognize that both Randel and Añel provide us the multi-tropopause frequency from global view. To make more accurate evaluation, the shortage of our multi-tropopause frequency will be discussed in conclusion and discussion section. And it is not accurate to say their frequency is 'lower estimated'. Both referees remind us the difference in temporal and spatial scale. The contents about multi tropopause comparison have been revised with the conclusion of 'the frequency of MT events are really high in some periods of winter time and has more significant seasonal variation than our expectations.'. The shortage in temporal coverage of our dataset was discussed in conclusion and discussion section, with the sentence 'The temporal coverage of our radiosonde dataset is limited. This can also be one factor that makes our frequency higher than other studies.'

Comment: Also as their results are for 2008 it could be said that a bigger percentage could be partially an additional confirmation of the global trend of MT events recently found out (see references in Specific comments).

Response: Other reasons as you mentioned were also discussed with 'Due to recent rising trend of MT events (Castanheira et al., 2009), this phenomena can also partially explain our percentage of MT events of 2008 higher than the averaged value of past tenth years.' and post future work 'Whether are there any other significant difference in MT frequencies still needs to be testified with more high resolution data of radiosonde.'

Comment: So, with a more balanced discussion and presentation of the results this

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work should be considered for publication.

Response: Thanks for your comments!

Comment: At least in the introduction it would be desirable to include a reference to Park et al. (2009). They have a nice discussion of the carbon exchange over the Tibetan Plateau: Park, M., W. J. Randel, L. K. Emmons, and N. J. Livesey (2009), Transport path-ways of carbon monoxide in the Asian summer monsoon diagnosed from Model of Ozone and Related Tracers (MOZART), *J. Geophys. Res.*, 114, D08303, doi:10.1029/2008JD010621.

Response: The persistent maximum of tropospheric chemical constituents in the upper troposphere–lower stratosphere (UTLS) is coordinated with middle tropospheric ozone minimum (Liu et al., 2009). Both Park and Liu attributed above phenomena to Asian summer monsoon. Besides the Asian monsoon, we also believe the tropopause folds can be related to the variations of vertical distribution of ozone. As a result, in part 4, we add the following content 'Scientists have discovered middle tropospheric ozone minimum in June (Liu et al., 2009), and relatively low ozone mixing ratios extending from the troposphere to the lower stratosphere (Tobo et al., 2008). Liu et al. (2009) related middle tropospheric ozone minimum with Asian summer monsoon. Together with the effect of Asian summer monsoon anticyclone, less intrusions of stratospheric air in summer can also contribute to the above ozone minimum phenomena.'

Comment: According to the Fig. 3 in Xu et al. (2008), NIOST has 16 radiosonde stations. It is not clear after a first glance the cause of having into account only 9 in this study, so, please, clarify it.

Response: Due to economic and labor costs, only parts of radiosonde stations in Fig. 3 of Xu et al. (2009) were employed to join the intensive observation. That's why we have only 9 radiosonde stations in this paper.

Page 22995

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Comment: Line 11: I don't think that the reference to Ye and Gao 1979 is worthy here. My suggestion is to remove it and to put the references included from here to the line 15 in a single bracket before the period.

Response: 'As a huge elevatedpathway for the STE (Fu et al., 2006)' was revised to 'As a huge elevated heating source, with the dynamical pumping and sucking, rigorous deep convection and the subtropical westerly jet, the TP generates an active stratosphere and troposphere exchange (STE) region, forming a short-circuit and pathway for the STE (Duan et al., 2005; Fu et al., 2006; Yang et al., 2004)'.

Comment: Lines 12-13 and 19-20 are very similar and they are close. Please, remove one of them.

Response: The repetitive contents have been deleted.

Comment: Lines 15-16: a characterization of the tropopause in this context can not be done as 'polar or tropical'. The simple fact of giving here a physical/climatological meaning is wrong. In fact the polar tropopause is an extremely weird thing and far of being completely understood. No doubt the authors refer to the altitude of the tropopause so to use adjectives as 'high' and 'low' is much more correct. If it is necessary then it can be linked with true climatological properties of tropical, extratropical or polar tropopauses. This is of application for several parts of the text.

Response: Thanks for your good suggestions, we have changed all 'polar' and 'tropical' to 'low' and 'high' tropopause. And give a description of the two words. Following the line 20 on page 22999 with content of 'The statistics for IOP1 demonstrate an overall bimodal distribution with maxima near 10 km (primarily associated with LRT1) and 17 km (firstly contributed by LRT2). According to the significant difference in the two tropopause height, we mark them as 'low' and 'high' tropopause separately.'

Comment: Lines 16-18: this claim about the lack of high quality observational data is right and it links with the lack of enough vertical resolution for UTLS research. I

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think that this point can be improved and supported by existing literature. The last CCMVal-2 report and two companion papers give a summary of these problems for UTLS research in observations and models:

Response: Three references as you suggested have been listed following the lines.

Comment: Lines 19-20: to specify that it is over the TP. There are several recent works on ozone mini-holes and UTLS for different parts of the Earth.

Response: 'have shown 'Ozone Mini-Hole' events, and ozone valley phenomena' was revised to 'have shown 'Ozone Mini-Hole' events, and ozone valley phenomena over the plateau'.

Comment: Line 24: the statement above the low column ozone is not strictly correct. The low column ozone over the TP according to Tian et al. (2008) is more related to transport than chemical reactions. It would be correct to say that the variability of the total column ozone over the TP is closely related to the uplift and descent of isentropic surfaces. However such statement seems obvious if the one above the transport is had into account before. So, please, use one of the more correct statements.

Response: Revise to 'Tian et al. (2008) found that the variability of the total column ozone over the TP is closely related to uplift and descent of isentropic surfaces.'

Comment: Line 27: this statement is obvious and unnecessary for the work presented. Please, remove it.

Response: Agree, and has been removed.

Page 22996

Comment: Line 1: the meaning of 'upper layer jet' in this context could be obvious but it would be a good idea to explain it better as the typical jet from the stratosphere to the troposphere in conditions with upper layer jet (Reed, 1955; Schmidt et al., 2005; Randel et al., 2007), a typical jet from the stratosphere to the troposphere

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Response: Revised to 'Tropopause folding often happens in conditions with upper layer jet (Reed, 1955; Schmidt, et al., 2005; Randel et al., 2007), which is a typical jet from the stratosphere to the troposphere.'

Comment: Line 3: there are possible citations more recent than Reed, 1955 relevant for this point which should be had into account.

Response: Both Sprenger et al., 2003 and Beekmann et al., 1997 have been listed here.

Comment: Also, perhaps not here but in Page 1 a citation to a recent paper by Zhang et al. (2010) seems very relevant in the context of this manuscript and the authors should be aware of it

Response: An interesting conclusion in Zhang et al. 2010 is that the plateau terrain height has no significant effect on the morphology of folds, and in addition to there aren't any folds in pressure-longitude cross section (see supplementary Fig. 7a). Then it is concluded that the meridional folds related with the westerly jet dominate UTLS above the plateau other than zonal folds. This content has been added in the discussion part. From other aspects, we think it has little usage to our study.

Comment: Lines 17-18: the statement in these lines is too assertive. First, if such statement is true it should be included in the discussion not in the introduction. Second, the authors are assuming that their results are more valid than the results obtained previously by other colleagues. Without to discuss if it is true or not, which would be the basis for such assumption?. Moreover as the authors recognize in the manuscript for example Añel et al. (2008) only uses one station over the TP for his study, so at least in this case to make a generalization for the complete IGRA dataset could be wrong. Therefore this statement must be modified and fitted to the results obtained in the study here presented.

Response: Removed 'This latter discovery tells us both of the above data can pro-

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duce limited information about thermal structures in the UTLS over the Plateau'. We also recognize that both Randel and Añel provide us the multi-tropopause frequency from global distribution. Meanwhile, the frequency difference between our results and theirs over the plateau proves exactly that the frequency of multi-tropopause in some periods can be really high. These also enlighten us the importance of high resolution radiosonde data over the plateau.

Comment: Page 22997, Line 3: the dataset presented here is not the most comprehensive radiosonde dataset for the TP. At least in temporal coverage it is very limited in comparison to others like IGRA. Therefore this statement must be removed or clarified.

Response: Revised to 'this study seeks to clarify characteristics of the MT using the most advanced radiosonde dataset in this area';

Comment: Lines 15-16: the specification of the use of Vaisala RS92 sondes is right as their characteristics are well known. However the specifications of the 'Chinese meteorological radiosonde system' are not enough. The research community probably is not so aware of the sensors and precision of such system. So such characteristics must be included in the text (probably as an appendix) or a reference provided (the source of information must be easily accessible).

Response: Detail information of Chinese GTSI radiosonde can be found from the following website: <http://www.cwqx.com/> According to the manuscript, the accuracy of Temperature $\leq \pm 0.2$ degree, and that of Pressure below 500 hPa $\leq \pm 2$ hPa, above 500 hPa $\leq \pm 1$ hPa. Measurement range: temperature 50 degree~90 degree; Relative humidity 0%~100%; Pressure 1060 hPa~5 hPa.

Comment: Page 22998, Lines 14-15: I don't think that the lack of radiosonde or a different kind of data have been an impediment for the research community to study the MT phenomenon over the TP. I suggest to remove 'due to the lack of radiosonde data'.

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Response: Agree, has been removed.

Comment: Page 22999, Lines 11-12: it would be very interesting if the authors can make explicit here the main differences between the previous soundings for Nagqu included in IGRA and their ones.

Response: We have communicated with Añel, J. A., and he sent us IGRA dataset which they used over and around the TP. The sounding system in our paper should be the same as that of IGRA, because both radiosondes were released by the local meteorological station of Nagqu.

Comment: Lines 12-15: the statement about the frequency of MT based on a single comparison of percentages between the stations should be clarified. A comparison with a sounding at the same date from a station in the plateau would be a nice figure to be included. Actually the interesting result here would be the percentage of cases with a MT over the plateau and without MT for stations out of the plateau. Moreover a comparison with a station slightly southern than Dali would be desirable, for example Mengzi.

Response: The radiosondes on same date are picked out and the frequency is recalculated. See the statistics in supplement Table1. P1 is from 7 Mar to 15 Mar. P2 is from 13 May to 22 May. P3 is from 7 Jul to 16 Jul. From following table, we can see that when moving to southern and low areas, the frequency of MT becomes lower. This part of content has been added to our manuscript. Temperature profiles at Gerze, Nagqu, Litang, and Dali on 26 Feb, 08:00 were plotted in supplement Figure 1. A low tropopause at 250 hPa height can be found at the plateau stations of Gerze, Naqu and Litang. Mengzi only has intensive observations in IOP3, otherwise this station southern than Dali will be a better choice.

Comment: Page 23000, Line 1: this explanation of the behavior of the LRT3 was previously suggested by Añel et al. (2007) and in some way by Gettelman and Forster (2002). It would be a good idea to cite them

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Response: Both referees' comments have been adopted here, and we revised the old sentence to 'LRT3 doesn't show this character suggesting itself as a stratospheric inversion layer (Añel et al., 2007; Gettelman and Forster, 2002). On the contrary, LRT1 and LRT2 should be attributed to tropospheric stable layers.'

Comment: Line 14: 'but can not explain' is not a valid reasoning at this point. At least the authors have not showed any results to evince it. It would be better to say that it does not seem probable.

Response: We agree with you. The sentence was revised to 'but does not seem probable to explain variations of 10-17 km in the LRT1 height during winter time.'

Comment: Lines 18-19: what do you mean with 'simultaneous meteorological situation'? Please, clarify it.

Response: 'simultaneous meteorological situation' is changed to 'simultaneous synoptic situation'

Comment: Line 19: I suggest to cite 'Uppala et al. (2005) The ERA-40 re-analysis, Q.J.R. Meteorol. Soc., 131(612), DOI: 10.1256/qj.04.176'

Response: Revised as you suggested.

Comment: Lines 26-28: what method was used to interpolate?

Response: Spline method has been added to explain interpolation method.

Comment: Page 23001, Line 8: '70 m/s at 200 hPa level'. From the figure it is not so clear that such speeds are reached at the 200 hPa level. In fact the highest value for the isotachs is 55 m/s. I guess that if values higher than 70 m/s were present then at least the 65 m/s isotach should be in the figure. Is it correct? Please, check it.

Response: Thanks for your very careful reading. You are right. We have checked the radiosonde, the sentence is revised to 'The observed maximum wind speed is higher than 60 m/s at 160 hPa level.'

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Comment: Line 10: I would not say that any of the foldings here presented are 'strong'. In fact they are not so apparent as typical examples of folding. So my suggestion is to remove the adjective 'strong' (for all the manuscript and the foldings shown). Moreover I miss along the manuscript a citation to Sprenger et al. (2003)

Response: According to classification of folds in Sprenger et al. 2003, The PV tropopause in Fig. 5 (a) and (b) exhibits a medium and a shallow folded structure. This information has been added in the manuscript. In order to demonstrate the UTLS structure around the Plateau clearly, we reduce the range of latitude in Fig. 5 and Fig. 6 to 10 degree-50 degree N. And the latitudinal terrain profile of the Plateau was also added in the figure.

Comment: Line 17: I would remove this sentence. It is not important here and from the figure such fold does not seem a likely cause of MT in sounding profiles.

Response: Thanks for reminding us the relationship between MT and folds. The sentence was removed. We revised the relationship between folds and MT as the folds were reflected by the MT events. This is also applied to other points.

Comment: Lines 18-25: here it would be extremely useful to include in the manuscript the hodograph of the soundings or a pressure-latitude figure to check the movement of the radiosonde. Probably, most of the readers are not aware of the typical values of the latitudinal or longitudinal displacements for the soundings in this region. Moreover, given the small variation in latitude between the stations for a typical MT-non MT case, it is important to know the difference between the grid point of the reanalysis with a MT detected and the exact point where the radiosonde measured it.

Response: Due to the popular westerly wind, the soundings were blew to east nearly without movement on south-north direction. We also checked that the latitude variation during a sounding time is not more than 0.2 degree, while the longitude variation can be larger than 1 degree, thus we assume the radiosonde is rising vertically in the cross section of pressure-latitude. Thus the multiple tropopauses found in one sounding

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were considered at the same latitude. These descriptions have been also added to the contents to depict the meaning of Fig. 5.

Comment: Page 23003, Lines 1-3: I don't think that the single jet movement is going to explain all the MT events over the TP. Probably it explains a significant percentage of the cases and this is the statement that should be done in the manuscript. Moreover such explanation of all the cases would need of a much more complete and complex climatological characterization for the studied region.

Response: We think the folds can be reflected by MT events to some extent. Thus we revised the previous contents 'The south-northward movements of the jet streamsextending of tropical tropopause over there (Pan et al., 2009).' to 'The variation of UTLS dynamic structures was reflected by the simple or MT observed at the plateau stations. The above observed two distinctive peak values in LRT1 height distribution in winter can be associated with latitudinal oscillative movement of folds and jets. Besides the heating of the plateau and poleward extending of tropical tropopause (Pan et al., 2009; Castanheira et al., 2009), the northward retreat of folds can also contribute to the observed single high tropopause during IOP2 and IOP3.' We also analyzed the seasonal differences of PV in pressure-longitude cross section (see supplementary Fig. 7a). In east-west direction, there aren't any folds above the plateau. Then we conclude the meridional folds related with westerly jet dominate UTLS than zonal folds above the plateau. This makes us believe that the jet movement has important influence on the MT or single tropopause events. Certainly, this conclusion needs more complete observation data to support it. We also mention that 'the heating of the plateau and pole-ward extending of tropical tropopause' can also make the tropopause high in summer time.

Comment: Line 8: this result is also shown and supported by the DT trends found in Castanheira et al. (2009). It could be worth to mention it.

Response: In discussion section, the frequency difference has been discussed with

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other aspects, including the DT trends as you mentioned. Please see the revised manuscript. Thanks for reminding us this paper.

Comment: Lines 10-12: I guess that the authors could mean a different thing that it is written in these lines. The existence of different characteristics along the year does not make more difficult to compute the tropopause, the difficulty is always the same and simply sometimes you can not determine it. It is true that the seasonal differences affect the structure of the UTLS, so the concept of tropopause becomes 'different' in some way

Response: The sentence has been removed.

Comment: Line 20: I don't think that 'inability' is the best word to characterize how ERA-40 deals with the tropopause. I think that 'shortcomings' is a better word.

Response: Agree, 'inability' has been upgraded to 'shortcomings'

Comment: Page 23004, Line 4: it would be better to say that the values found here are higher than previous ones. Line 5-7: I don't think that such statement about radiosonde data can be obtained from the results in the manuscript. Moreover the authors obtain higher percentages of MT's than previous studies but their research don't undertakes any serious work to test if they are more or less valid than previous ones in the literature. Figure 4: I have some doubts about how useful this figure is. Probably it could be removed.

Response: The sentence 'Discovering previous lower estimation ...over the Plateau.' has been revised to 'Discovering MT frequency are really high in periods of winter time, this work could further deepen our understanding in UTLS structure over the Plateau.' The usages of Figure 4 have been described in revised manuscript.

Comment: Figure 5: it is necessary to include in the legend the meaning of the triangles.

Response: The Fig 5 has been updated with the terrain shape of the plateau. The C12650

meaning of triangles has been described in the caption.

Supplement: it should be made explicit that it is Matlab code. It could not be obvious for readers don't using such language and moreover as you mention a previous IDL code by Homeyer it is confusing. Also if it is based in the code used for their recent paper Homeyer et al. (2010), perhaps you should include a reference in the text: Homeyer, Cameron R., Kenneth P. Bowman, and Laura L. Pan (2010), Extratropical tropopause transition layer characteristics from high-resolution sounding data, J Geophys Res, 115, D13108 Moreover you should include in the code a license. Although it can get published with the manuscript, the code should have its own licensing. I suggest that you use the GNU Public License or better the GNU Free Documentation License in this case. They are widely recognized and used. GFDL is compatible with Creative Commons-BY, the same that you have now in the ACPD manuscript. <http://www.gnu.org/licenses/gplhowto.html> <http://www.gnu.org/licenses/fdl.html> <http://www.gnu.org/licenses/fdl-howtopt.html> It would be also great if you can include at the beginning of the function a more step by step explanation of the code for the less skilled readers.

Response: The function has been updated with the following contents, more detailed information can be found in the code.

Technical corrections ***** - It would be desirable an additional checking of the text (use of English language and wording). For example, in some parts the introduction reads like a simple bombardment of ideas instead of a fluent text. – Between numerals and units you have to leave a blank - Page 22996

Response: The introduction section has been revised totally. Please see our answers to referee 2 comments on our introduction. All numerals and units have been checked for inserting blank;

line 13: 'Integrated' instead of 'Integrate' line 26: calls to figures in text should be written using 'Figure' instead of an acronym

Response: 'Integrate' revised to 'Integrated';
Page 22997 line 8: to remove 'newly developed'. It is unnecessary.
Response: The word 'newly developed' has been removed.
Page 22999 Line 13: probably it would be more correct 'locates out of the plateau' than 'locates out of the south plateau'
Response: Has been revised as you suggested.
Line 15: 'southern'
Response: 'souther' revised to 'southern'
Lines 16-17: the statement above the occurrence should be changed. First, the campaign and periods here studied don't give enough data to reach such conclusion. It would be necessary a complete climatological study with several years of measurements. Therefore it is more correct to state that the results obtained from these campaigns support the climatological conclusions reached in previous studies.
Response: The sentence 'It is therefore concluded that MT occurs in winter time with high frequency over the Plateau.' is revised to 'This result supports the conclusion of MT occurs in winter time with high frequency over the Plateau, which is similar to previous climatological studies'.

Page 23001

Line 1: 'definition' is not correct in this context. To make a difference between the official 'definition' of the tropopause given by the WMO in 1957 and the criterion used with PV values (not an official definition), it would be better to replace 'definition' with 'way to identify' or similar. To include here a citation to the report by the WMO (1986) with such criterion, a textbook or for example the article by McIntyre in the Encyclopedia of Atmospheric Sciences (Holton et al. 2002) would be useful.

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Response: 'An alternative definition of tropopause is to use PV.' is revised to 'Another way to identify tropopause is to use PV'.

Line 6: you forgot a comma (Holton, 2004)

Response: A comma has been added.

Line 21: 'North' or 'northward'

Response: 'North' is changed to 'northward'

Page 23002 Line 26: 'Ding and Wang'?

Response: Revised

Table 1: in the legend or the table should be indicated the year.

Response: The legend of the table1 is revised to 'Intensive observation dates of the three periods in 2008'

Figure 3: the title of the figure c) should be '(c) IOP3'.

Response: The title has been updated.

Figure 4: are the colours in the legend of Fig. 4c correct?.

Response: The color of the legend has been corrected.

Figures 5 and 6: as you are using pressure it would be better to use 'pressure-latitude' in the figures and legends.

Response: Revised as you suggested.

Supplement: in the code, lines 12-13: they are identical, one of them should be 'V' and the explanation the corresponding in each case.

Response: The function has been totally reviewed.

We have totally revised our manuscript and uploaded some more figures in order to

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help readers to more easily understand this work.

Xuelong Chen on behalf of co-authors

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/10/C12638/2011/acpd-10-C12638-2011-supplement.zip>

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 10, 22993, 2010.

C12654

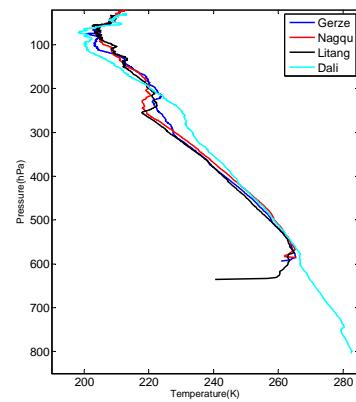


Figure 1 Profiles of temperature at Gerze, Nagqu, Litang, and Dali on 26 Feb, 08:00

Fig. 1.

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Table 1 Frequency of double tropopause (DT) and triple tropopause (TT) during the three observation periods. X means no observation data. P1 is from 7 Mar to 15 Mar, P2 is from 13 May to 22 May, P3 is from 7 Jul to 16 Jul.

	Gierz	Lambu	Nagpo	Litang	Lijiang	Dali	Tengchong	Kunming	Mengzi					
	DT	TT	DT	TT	DT	TT	DT	TT	DT	TT	DT	TT	DT	TT
P1	0%	0%	X	X	0%	0%	0%	0%	X	X	0%	0%	X	X
P2	10%	10%	X	X	10%	10%	10%	10%	X	X	10%	10%	X	X
P3	10%	10%	X	X	10%	10%	10%	10%	X	X	10%	10%	X	X

Fig. 2.

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