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Atmospheric Chemistry and Physics Discussions

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

**Anonymous Referee #1**

Received and published: 15 November 2010

General comments **************** This manuscript has some good points, it goes in the step by step in the formation of multiple tropopauses over the Tibetan Plateau and make good local comparisons. However there are a lot of issues which should be carefully reviewed before having a text acceptable for publication. First the article from the periods of measurement try to differentiate between phases of the monsoon, however the discussion is completely focused on single movements of the jet-stream, without to link any characteristic with the monsoon. More emphasis on the role of such phenomenon would do a nice point.

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 availabe 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).

Also in some parts the manuscript lacks of crucial information and for example the section 2, which is very close to a data a 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.

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

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 previouly 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. 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). So, with a more balanced discussion and presentation of the results this work should be considered for publication.

Specific comments ***************** 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 pathways 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.

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.

Page 22995 ———-

Line 11: I don't think that the reference to Yeng 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.

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

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 adjetives 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.

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 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:

SPARC CCMVal (2010), SPARC Report on the Evaluation of Chemistry-Climate Models, V. Eyring, T. G. Shepherd, D. W. Waugh (Eds.), SPARC Report No. 5, WCRP-132,



WMO/TD-No. 1526, http://www.atmosp.physics.utoronto.ca/SPARC

Gettelman, A., et al. (2010), Multimodel assessment of the upper troposphere and lower stratosphere: Tropics and global trends, J. Geophys. Res., 115, D00M08, doi:10.1029/2009JD013638.

Hegglin, M. I., et al. (2010), Multimodel assessment of the upper troposphere and lower stratosphere: Extratropics, J. Geophys. Res., 115, D00M09, doi:10.1029/2010JD013884.

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.

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.

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

Page 22996 ———-

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

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

Zhang et al. (2010) Cross-tropopause mass exchange associated with a tropopause fold event over the northeastern Tibetan Plateau, Adv. Atmos. Sci., 27(6), 1344-1360, DOI: 10.1007/s00376-010-9129-9

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.

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.

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).

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'.

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.



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.

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.

Añel, J. A., J. C. Antuña, L. de la Torre, R. Nieto, and L. Gimeno (2007), Global statistics of multiple tropopauses from the IGRA database, Geophys. Res. Lett., 34, L06709, doi:10.1029/2006GL029224.

Gettelman, A. and P. M. de Forster, Definition and climatology of the tropical tropopause layer. Journal of the Meteorological Society of Japan, 80:4B, 911-924, 2002

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.

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

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'

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

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.

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)

Sprenger, M., M. Croci Maspoli, and H. Wernli (2003), Tropopause folds and cross-tropopause exchange: A global investigation based upon ECMWF analyses for the time period March 2000 to February 2001, J. Geophys. Res., 108(D12), 8518, doi:10.1029/2002JD002587.

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

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.

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.

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.

Castanheira, J. M., Añel, J. A., Marques, C. A. F., Antuña, J. C., Liberato, M. L. R., de la Torre, L., and Gimeno, L.: Increase of upper troposphere/lower stratosphere wave



baroclinicity during the second half of the 20th century, Atmos. Chem. Phys., 9, 9143-9153, doi:10.5194/acp-9-9143-2009,

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.

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.

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.

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

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/gpl-howto.html http://www.gnu.org/licenses/fdl.html http://www.gnu.org/licenses/fdl-howto-opt.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.

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,

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

- Page 22997 line 8: to remove 'newly developed'. It is unnecessary.

- Page 22999

Line 13: probably it would be more correct 'locates out of the plateau' than 'locates out of the south plateau'

Line 15: 'southern'

Lines 16-17: the statement above the ocurrence 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.



- 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 te 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.

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

Line 21: 'North' or 'northward'

- Page 23002

Line 26: 'Ding and Wang'?

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

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

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

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

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

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