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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 #2

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The paper presents statistics of multiple tropopause events using radiosonde observations above the Tibetan Plateau in 2008. The frequency of multiple tropopause events is discussed separately for the time before, during and after the Asian monsoon season. Multiple tropopauses are often linked to deformations of the tropopause, so called tropopause folds. Using the ECMWF ERA–40 reanalysis data two case studies of simultaneous events of multiple tropopauses and tropopause folds are presented.
The analysis of the UT/LS region above the Tibetan Plateau using a nice new radiosonde data set is a worth publication in ACP. Nevertheless, the analysis of the radiosonde data in the paper does not reveal substantial new findings on the UT/LS region. The text contains incorrect descriptions of processes in the UT/LS. The authors use terminologies which are not common in atmospheric sciences. There are also problems with the statistical analysis and the text contains misinterpretations. To

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some extent this may be caused by the quality of the English.
I recommend the paper for publication in ACP after some major and substantial revisions.

Major comments:

1. *Lack of new findings:* The paper may represent an analysis of the structure of the UT/LS above the Tibetan Plateau, but it does not report "new findings" on the UT/LS. The tropopause related processes the author described are well known and documented in literature. Further, the difference in frequency of double tropopauses before and during the monsoon season, is not a new finding. It rather reflects the difference in dynamics dependent on season.
   On page 23002 (line 17) the authors write *"To further investigate the observed close relationship between the jets and tropopause folds above, we display the seasonal variation of the jets (Fig. 7) to explain seasonal differences in tropopause structure"*. First, this figure only shows the seasonal variation of the jet stream, it does not give any information on the relationship to tropopause folds! Second, the authors state this relationship as if they would be the first who have investigated tropopause folds and jet structure. There are many earlier publications, which show a coexistence between tropopause folds and jet streams (e.g. Shapiro, 1980), and the connection of double tropopauses with jet streams (e.g. Pan et al., 2009).

2. *Terminologies and explanations:* There are a lot of misleading terminologies and statements probably due to a lack of the understanding of background science. The subtropical jet stream does not cause tropopause folds above the plateau (page 22994, lines 23–24)! Of course, tropopause folds, jet streams and double tropopauses have some relations in the atmosphere, but the definitions are

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different. Double tropopauses are defined by the lapse rate criterion and the tropopause folds are usually dynamically defined by a fixed PV isoline. The authors also state, that they have observed tropopause folds as thermal MT events (e.g. page 22997, lines 4–6). It seems that they believe, "tropopause folds" and "multiple tropopauses" are the same. The authors further use the terminology "simple tropopause" (e.g. page 22996, line 12), which is used in a wrong context. On page 22999 (lines 29–31) the authors suggest the third tropopause (LRT3) as a stratospheric inversion layer and the first tropopause (LRT1) and second tropopause (LRT2) as tropospheric inversion layers. A tropopause is no inversion layer! The south–north movement of the jet streams does not cause the latitudinal variation of tropopause folds (page 23003, lines 1–8). The tropopause folds are not the mean reason for the height variation of LRT1 in winter.

3. *Statistical analysis:* The discussion of the frequency of multiple tropopause events is problematic. The author cannot compare absolute values of frequencies of multiple tropopauses, calculated for the different Tibetan plateau sites, with those of other publications, such as Randel et al. (2007) or Añel et al. (2008). Randel and Añel used different data sets, with a different vertical and horizontal resolution, and their analysis represents a climatology on a larger time period. On page 22999 (lines 4–7) the authors state that the MT occurrences at the three Plateau stations for IOP1 are between 72% to 84%. I guess they calculated the sum of the frequency of the second and third tropopauses at the single stations. This value has no scientific meaning to me. The author cannot compare this value to the values of Randels Figure 9a. This figure represents the frequency of double tropopauses and does not contain any third tropopause. It shows the seasonal frequency for $10^\circ \times 30^\circ$ horizontal bins, so the author cannot directly compare his frequency of second and third tropopauses at three single latitude and longitude positions of the Plateau stations (Gerze, Nagqu, Litang) with Randels mean frequency. The author should reexamine the comparison

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with other publications.

4. *Speculative reasoning:* The discussion is too much speculative. Especially the discussion on the impact of stratospheric intrusions on the high ozone concentration in the upper troposphere (page 23002, line 11) needs some supporting scientific analyses or some convincing references. The impact of intrusions on the ozone concentration in the upper troposphere is too much speculative. The statement of the authors, that intrusions of stratospheric air with a high ozone concentration into the upper troposphere above the Tibetan Plateau would be a new explanation of the higher ozone in the upper troposphere in winter than in summer is not convincing (page 22994, lines 25–27, page 23002, line 11).

5. *Introduction:* The introduction does not provide any description of the characteristics of the two mean features of the paper, the multiple tropopauses and the tropopause folds, in more detail. Concerning the tropopause folds there are some important publications (e.g. Shapiro, 1980, 1987). The author should also cite some analyses on the frequency of both double tropopauses and tropopause folds (summer to winter contrast), which already appeared in literature.
The authors should present a clear picture about their contribution in the context of the present science. They try to give some questions on the objectives of the paper (page 22996, lines 19–22), but these questions should be more precise and they are not properly answered in the paper.

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