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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A global climatology of stratosphere-troposphere exchange using the ERA-interim dataset from 1979 to 2011" *by* B. Skerlak et al.

B. Skerlak et al.

bojan.skerlak@env.ethz.ch

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We thank Juan Añel for his detailed comments, which were helpful to improve the clarity of our presentation. In addition to several smaller changes, your comments about the PBL height over the Tibetan Plateau led to an important revision of our algorithm.

First of all I would like to say that I have found this a very interesting and nice paper. In my view the authors have done a great work putting numbers on results that to the date just were climatological descriptions. That said I would like to point out a few details and previous and very recent research. Altough it can seem slightly self-serving I think

that it will help the authors to support their results and with them the readers will have a more profound view of the subject.

The authors mention several times in the manuscript the maximum height of the PBL and more explicitely over the Himalayas. They say that it is so high as 3km above ground level. The could not be aware of a two recent studies that show that the PBL over the Tibet can reach in fact 9.4 km above sea level and study the STT and TST exchange during such event: Chen et al. (2013) The Deep Atmospheric Boundary Layer and Its Significance to the Stratosphere and Troposphere Exchange over the Tibetan Plateau. (PLoS ONE 8(2): e56909. doi:$10.1371 journal.pone.0056909$) Chen et al. A record 9.4 km measurement of top of the atmospheric boundary layer over the Tibetan Plateau (submitted to Q. J. R. Meteorol. Soc.) and available from $http://users.ox.ac.uk/\_smit0069/ABL\_Tibet.pdf$ Having into account that they use a Lagrangian transport model and their discussion on deep exchange events, these two references no doubt will improve the discussion of their results.

Thank you very much for this hint. We did not know about these results at the time of submission but learned about them shortly afterwards. We thus decided to drop the 330-hPa threshold and now also take into account those events where the pressure at the PBL top reaches values below 330 hPa. This almost exclusively changes the magnitude of the peak over the Tibetan Plateau and is discussed in the revised Sect. 6.1.

It is good having tested different PV values, but it is an obvious question that it is clarified to the end of the paper. I think that it would be better to clarify it at the beginning of the point 2.2. Maybe it would be good to mention the results by Klaus P. Hoinka in his paper on the tropopause in the Monthly Weather Review (1998). There is there a nice discussion of the problem of matching the PV field with the tropopause.

We now clearly state the sensitivity to the tropopause definition chosen in the introduction and discuss it in more detail in the new Sect. 5.1.

From point 3.1.1 I would like to bring the attention of the authors to the patterns found by Randel et al. (2007) and Añel et al. (2008). The patterns found here by the authors (Figs. 5 and 6) clearly match the previous results mentioned of maximum occurrece of multiple tropopauses. This is at the same time an obvious result and extremely interesting. Multiple tropopauses from reanalisis and radiosondes should agree with the results from Sprenger et al. (2003) but at the same time continues to be a lot of controversy on the origin of the air massess for this multiple tropopause/deep exchange/folding events (if it is predominantly tropical or extratropical and if TST or STT is predominant on a given region). This has been studied and recently published combining langrangian analysis and radiosondes for Boulder (Añel et al. 2012). I think that the authors would improve the manuscript it they discuss their results on the light of these previous findings.

Randel et al. (2007) Observational characteristics of double tropopauses, J. Geophys. Res., 112, D07309, doi:10.1029/2006JD007904. Añel et al. (2008) Climatological features of global multiple tropopause events, J. Geophys. Res., 113, D00B08, doi:10.1029/2007JD009697. Peevey et al. (2012), Investigation of Double Tropopause Spatial and Temporal Global Variability Utilizing HIRDLS Temperature Observations, J. Geophys. Res., 117, D1, doi: 10.1029/2011JD016443. Añel et al. (2012) On the Origin of the Air between Multiple Tropopauses at Midlatitudes, The Scientific World Journal, vol. 2012, Article ID 191028, 5 pages, 2012. doi:10.1100/2012/191028.

Using the dynamical tropopause definition, there is rarely an ambiguity in whether air masses are 'stratospheric' or 'tropospheric' near tropopause folds. Situations where PV values are very high over a vertically large region are more problematic (see Sect. 2.2). Thus 'multiple tropopauses' are a phenomenon intimately linked to the WMO tropopause definition and do not appear using the dynamical tropopause definition. A systematic analysis of the connection between tropopause folds (using the dynamical tropopause definition) and multiple tropopauses (using the lapse-rate definition) would certainly be very interesting but in this study, we want to focus on the spatial and

temporal distribution of the STE events and the exchange between the stratosphere and the PBL. It might be of interest to you that we are currently compiling a 34-year climatology of tropopause folds where we plan to include some discussion on the topics mentioned in your comment.

In order to understand to what extent air masses near the dynamical tropopause still have 'stratospheric' or 'tropospheric' chemical character, it is of course very important to study the (Lagrangian) history of the air and its residence times on either side of the tropopause. An analysis of the chemical composition such as for example done by Hoor et al. (ACP 2004) is of course interesting but, in our opinion, beyond the scope of this paper.

The similarity of the patterns found for (deep) STE mass fluxes and tropopause fold frequencies found in Sprenger et al. (2003) are pointed out in Sect. 3.1.1 and will be subject to further study.

The results found here on time series (point 3.5) probably has something to do with (and are supported by) the detected trends of UTLS baroclinicity and percentages of double tropopause ocurrences. I find that citing the following reference in this manuscript would help to support a result not so deeply discussed: Castaheira et al. (2009) Increase of upper troposphere/lower stratosphere wave baroclinicity during the second half of the 20th century, Atmos. Chem. Phys., 9, 9143-9153 Castanheira et al. (2010) Corregindum to "Increase of upper troposphere/lower stratosphere wave baroclinicity during the second half of the 20th century",Atmos. Chem. Phys., 10, 9057-9058

When analysing our time series of STT and TST mass fluxes for the period 1979-2000, we do not find a significant trend. It is only when analysing the full time series (up to and including 2011) that we find significant trends. This is in contrast to the findings of Castanheira et al. (2009) where the time series of total energy and energy of high frequency waves (m=1,...,4) do not show a transition around the year 2000. Only for the time series of total energy (m=6,...,9), there is a kink visible around the year 1990. Furthermore, we are a bit confused concerning the results of the mentioned study. How is it that although energy in the shallow waves (m=6,...,9) shows a decreasing trend but the available eddy kinetic energy in the middle troposphere is increasing (Figs. 5 and 6). From the vertical structure of these waves (Fig. 1), we would have expected a decrease in the middle troposphere and an increase above 100 hPa (corresponding to m=1,...,4 which show an increase in energy).

Finally one more time my congratulations to the authors for this nice work.

Thank you!

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