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

J. A. Añel

juan.anel@smithschool.ox.ac.uk

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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. Althought 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 explicitly 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:


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.

- 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.

- 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 occurrence of multiple tropopauses. This is at the same time an obvious result and extremely interesting. Multiple tropopauses from reanalysis 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 if they discuss their results in the light of these previous findings.


The results found here on time series (point 3.5) probably have something to do with (and are supported by) the detected trends of UTLS baroclinicity and percentages of double tropopause occurrences. I find that citing the following reference in this manuscript would help to support a result not so deeply discussed:

Castanheira 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

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

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