Response to reviewer 1

Major points

We would like to thank reviewer 1 for a very thoughtful and detailed review of our manuscript. We are also aware that this kind of review requires a lot of time. Even if the review is fairly critical, we feel that the "spirit" of this review is to give us hints and ideas how to improve the paper. So we followed these comments and significantly changed the chain of reasoning although the main results and statements are the same.

Here the list of the most important modifications (the referenced lines in the revised manuscript are marked as L XXX - YYY):

1. *"terminus "in-mixing" is not sufficiently explained..."*
   To avoid any ambiguity we define this terminus in the introduction (as nearly isentropic net transport from the extratropics into the upper part of the TTL and use a new figure (figure 1) in order to clarify the notation used in the paper (L 42-55).

   We also discuss critically (L 557-567) "in-mixing" as a sort of transport that

   - is mainly driven by meridional and isentropic winds, i.e. rather by advective than diffusive part of transport.
   - can be understood as an irreversible process in the sense that air masses which crossed the lateral boundary of the TTL equatorwards do not move back but ascent into the stratosphere by coupling to the Brewer-Dobson circulation.

2. *"...the paper would benefit much from deemphasizing the two parameterizations of the vertical velocity..."*
   Following this recommendation, we changed the strategy of the paper in the following way[(section 2 and appendix) :

   - now only one, the mass-conserving parametrization of the vertical velocities, is used throughout the paper (this simplifies figures 5, 8 and 9 and the corresponding discussion).
   - in section 2 where this parametrization is introduced, all necessary details are explained with more care than in the previous version. Some technical aspects are shifted to the appendix.
   - in section 2 we also critically compare this parametrization with the "old" one (Konopka et al., JGR, 2007) and discuss why the "new" one is more physical. This is necessary in order to keep continuity in the development of CLaMS.

3. *"...figures with mean age do not show something new compared with figures showing passive ozone..."*
   Yes, we agree and follow this recommendation and do not use the mean age in the whole paper at all. Consequently, figures 3 and 4 from the old manuscript are combined to only one figure 3

4. *"...try to diagnose more explicitly the horizontal transport/advection in the model..."*
   Because reviewer 2 gave the same recommendation, we carried out a completely new sensitivity study (L 318-395) and included a new figure 6 discussing the main results of this study. Here, we followed the idea of the reviewer and carried out CLaMS simulations by setting passive ozone to zero if CLaMS air parcels cross the $\pm 15^{\circ}$N latitude equatorwards. The main result of this study is that mainly the seasonality of the horizontal advection, i.e. of the meridional wind drives the seasonality of CLaMS passive ozone in the tropics between 380 and 420 K with the main contribution comming from the northern hemisphere in summer.

5. *"...look at the related longitudinal variation of the observed data and simulations to quantify the impact of the Asian monsoon..."*
   Here, figure 6 was improved (now it is figure 7) in the way that in order to get CLaMS results which are comparable with the MLS observations, averaging procedure imitating the averaging kernel of the MLS instrument was introduced. The resulting CLaMS ozone distributions compare now much better with the MLS data. Furthermore, the ozone seasonality observed by two SHADOZ stations (Kuala Lumpur and Nairobi) which are located downwind of the Asian monsoon anticyclone is also used for interpretation of our results (new figure 8 + L 485-521).

   Generally, in the revised version we compare CLaMS with observations (SHADOZ and MLS) only on two $\theta-$levels 380 and 420 K. (figure 5 that, as recommended, was reduced to these two levels but also the new figure 8). In figure 1 we added the map of locations for all used SHADOZ stations.

Minor points

Almost all of the modifications motivated by the minor comments of the reviewer 1 are now included in the revised version. Here the list of modification (point-by-point):

- *"page 17939, line 12, Randel et al. (2007), include a short description of the procedure"*
  was done (L 60-70)

- *"page 17941, line 17, Mahowald et al. (2002), more explanation for the hybrid coordinate"*
  was done (L 151-156)

- *"page 17941 line 21 to page 17942 line 14, explain better corrected velocity and how the constant correction works"*
  As mentioned in point 2, we completely rewritten this part of our text (L 160-184 + appendix). In addition,

  – colors in figure 2 were improved. Note that in the left panel upwelling with positive velocities is everywhere in the tropics ("corrected" version). Only in our "reference" version (Konopka, 2007) there was downwelling beteeen 350 and 360 K (i.e. between the thick dash-dotted white lines in figure 2).

  – "constant" correction means a constant correction for every latitude at a given $\theta-$level. So this correction is a function of θ. We discuss this point in the appendix.

- *"page 17942, line 20, annual cycle of $\dot{\theta}$. Why does the radiation dominated equatorial stratosphere have an annual cycle? The sun crosses the Equator twice per year, so one might expect a semi-annual cycle. Why is there an annual cycle? Because of ozone, the Brewer Dobson circulation? Please explain."*

  Here the part of our text explaining the annual cycle (L 202-212):

  ... the annual cycle of tropical upwelling is a consequence of the hemispheric asymmetry of the land-sea distribution and of the orography which lead to hemispheric differences in the distribution and intensity of the wave drag driving the Brewer-Dobson circulation. In particular, the lowest tropical temperatures in winter correspond to the strongest wave drag in the northern hemisphere. On the other side, the semi-annual cycle is a consequence of a simple fact that the Intertropical Convergence Zone (ITCZ) roughly follows the sun that crosses the equator twice per year...

- *"page 17943, line 5, explain the abbreviation AP"*
  AP means air parcels. We wrote it out everywhere in the revised version.

- *"page 17943, line 29: ...permanent upwelling ... excludes downward transport in the tropics". This is only true for the model. How can you exclude in the real world, that there are downward transports in some regions, maybe compensating very strong upwards transports in other regions?"*
  Yes I completely agree with your criticism. This only true in some models, at least in CLaMS. We stated it now explicitly in the revised version (L 552-557).

- *"page 17944: please add a map of SHADOZ stations..."*
  was done in figure 2.

- *"Fig. 4, pO3 and age-of-air are really the same thing"*
  Yes we, agree, see answer 3 in the major points.

- *"Fig. 5, HALOE values are lower than SHADOZ values, please comment it"*
  Now, the following block is included in the revised version (L 277-283):

  ...The difference between the HALOE and SHADOZ climatology results probably from the fact that the HALOE observations cover the $\pm 10°$N latitude range almost uniformly whereas the SHADOZ climatology is biased by the geographic positions of the considered seven station of which five are located in the southern hemisphere (see also Konopka at al., 2009, JGR)....

- *"page 17945, lines 3 to 9: Somewhere, you should also say that CLAMS ozone is likely to be off in Southern hemisphere spring to summer, because there is no ozone hole in CLAMS... "*
  was done, here the corresponding text (L 248-252):

...Some differences between the CLaMS results and HALOE are obvious, in particular on the southern hemisphere where the contribution of the ozone hole is not reproduced in CLaMS (no halogen-induced chemistry in this version of CLaMS)...

- *"page 17946, line 1, 3, spell "STJ" out"*
  was done

- *"page 17946, line 27: "MLS are slightly higher" "*
  Yes, we agree, the comparison in the revised version where averaging kernel procedure was applied for CLaMS data, looks now much better. Nevertheless, some differences still exist. The wording "slightly" was removed.

- *"page 17947, lines 14 to 22: Suggest to omit this entire paragraph"*
  We slightly re-formulated this paragraph (see L 522-534). Nevertheless, we also believe that the comparison with the aqua-planet is very instructive as the annual cycles is a consequence of hemispheric asymmetry of the land-sea distribution and of the orography which lead to hemispheric differences both in upwelling and in-mixing.

- *"page 17947: The DJF MLS data show clear "monsoon" signatures over Indonesia, Southern Africa, and South America. Why are these not discussed? "*
  In the revised version, we discuss this point much more extensively (L 460-485). We included also the reference Borchi et al., ACP, 2005, discussing ozone profiles observed with SOAZ in the TTL on the southern hemisphere which show clear signatures of in-mixing.

- *"section 6, separate vertical and horizontal transport more clearly"*
  We hope that the revised version improved this point. See answer 4 in the major points.

- *"page 17948, line 9: "undesirable vertical transport""*
  This wording was removed. In our sensitivity study (end of section 3) we show that for the seasonality at 380 K, the vertical downward transport from above 420 K (i.e. its advective and diffusive parts), is completely negligible in the model (see also answer 4).

- *"Why do you show pO3/(O3 + 40) and not the simpler pO3/O3? "*
  This is because we do not have any tropospheric fluxes of O_3 into the TTL (O_3 is set to zero at the lower boundary). Thus, pO_3/O_3 would overestimate in-mixing, in particular below 380 K where O_3 is small in CLaMS. To avoid it, we use 40 ppbv as the maximal contribution of the troposphere to the seasonality of O_3 in the TTL. With this assumption O_3+40 ppbv roughly reproduces the SHADOZ climatology at $\theta = 360$ K. A more detailed explanation was included (L 567-575).

- *"line 27: 360 K: In Fig. 2 the cross-over is nearer to 380 K. "*
  Yes we agree. The sentence was corrected.

- *"page 17949, lines 1, 2"*
  was corrected.

- *"page 17949, line 7:"*
  The misleading sentence was removed.

- *”Page 17950, line 7: ”be” to ”by” ”*
  was done.