Response to anonymous Referee #3

We thank the reviewer for her/his valuable comments.

This manuscript presents an analysis of tracer observations and trajectories, and provides evidence there has been an increase in advection and mixing in the lower branch of the stratospheric B-D circulation after 2000, but little change in the upper branch. The evidence presented in not unambiguous, as there is uncertainty involved with the interpretation of each diagnostic. However, the fact that several independent diagnostics appear consistent with the hypothesized circulation change provides strong evidence for such a change.

The Reviewer correctly pointed out the ambiguity in the individual interpretation of each diagnostic. Only the combination of the different diagnostics allows for a more substantial hypothesis of a strengthened lower BDC branch. Hence, we added a comment on this subject to the conclusion of our manuscript:

p.28415, l.16: "Even though the individual interpretation of each of the presented diagnostics is not unambiguous the combination of all diagnostics provides strong evidence on our hypothesis of a temporary change in the BDC pattern. This scenario of ..."

MAJOR COMMENTS

1. Why are only Canadian ozone sondes shown? The changes shown could be due, for example, to a change in wave structure of the downwelling and no necessarily an increase in mean extratropical downwelling. To say anything about the change in extratropical downwelling linked to changes in tropical upwelling you need to use ozone sondes from all longitudes.

In principle, this is true - the 3-station average is, of course, only a partial zonal mean. However, we analysed the 3 stations separately and they show much the same strong ozone anomaly in the LMS. This is a hint that this ozone signal is of global and not of local nature. Furthermore, an impact by local downwelling on the observed ozone anomalies in the LMS should be rather small, especially if taken into account the long effective lifetime of ozone in this region and the fact that nearly unhindered zonal tracer transport and mixing driven by the prevailing winds in the stratosphere is on a much shorter timescale than the downwelling. However, the reviewer is right that a complete picture can only be obtained by analysing the whole worldwide ozone sonde network data set, but this is beyond the scope of this paper. In order to clarify the representativeness of the Canadian ozone sonde data set we add the following to the revised manuscript:

p.28405, l.16 "The three station average is, of course, only a partial mean, but the observed strong ozone anomaly in the lower stratosphere after the year 2000 should be representative for the midlatitudes on global scale because of the long effective lifetime of ozone in this region and the fact that zonal tracer transport and mixing driven by the prevailing winds in the stratospheric surf zone (McIntyre and Palmer, 1984) is on a much shorter timescale than the downwelling."

2. Why is there no change in the PDFs for May shown in fig 3? This needs to be discussed.

The reason is most likely that the impact of a change in horizontal transport on the N_2O-PDFs is stronger when the impact of vertical transport is weaker. That means, the N_2O-PDFs in May are less sensitive to changing horizontal transport because the tracer distributions in the LMS are dominated by the strongest downwelling during this season (e.g. Hoor et a., 2005; Hegglin et al. 2006 and Bönisch et al., 2009). In contrast to the N_2O-PDFs, the N_2O/O_3-correlations clearly reveal that there is a change in the portioning of horizontal to vertical transport. This again highlights the fact noted by the reviewer that none of the diagnostics is unambiguous on its own. However, we feel that for the clear arrangement of the manuscript a sophisticated discussion, why the N_2O-PDFs show no changes in May, should be omitted at this point. Therefore, we would suggest the following changes to the manuscript:

p.28407, l.11: "The higher post-2000 N_2O values in winter and summer are ..."

p.28408, l.16: "The differences in R_{VI} ... that the higher N_2O values in winter and summer in the post-2000 years ..."
p.28408, l.19: "The unchanged N_2O-PDFs in May might be caused by the fact that the influence of downwelling on the tracer distribution of the LMS is strongest during this season (Hoor et al., 2005; Hegglin et al., 2006 and Bönisch et al., 2009). Therefore, the N_2O-PDF is much less sensitive to changes in the isentropic transport from the tropics into the extratropics."

Also, what is the interannual variability in the PDFs within the pre-2000 or post-2000 periods, i.e., are the differences in PDFs shown much larger than differences within the pre or post-2000 periods.
Unfortunately, we cannot answer this question because we simply do not have enough data of N_2O in-situ measurements during the same month in the LMS, especially if the PDFs should be analysed separately for the time periods before and after 2000. Of course, there is an interannual variability of the N_2O-PDFs and therefore the interpretation is not unambiguous. Generally, the interpretation of the tracer distribution on geographical or meteorological coordinates (e.g. PV, height, potential temperature or distance to the local tropopause) with a sparse temporal and spatial coverage must be handled with care concerning their global representativeness. Exactly for this reason, we combined different diagnostics to create more evidence for our hypothesis of a strengthened lower BDC branch. Furthermore, we pick up the general remark of the reviewer and add the fact of ambiguity of each diagnostic and of the necessity of their combination for the interpretation to our conclusion (see answer to general remark above).

3. The authors need to be careful with there comments regarding changes in middle atmosphere age. The comments on lines 10-14 on pg 28413 and lines 20-27 on pg 28415 are not consistent. In former it is stated that as residual circulation for upper branch is unchanged then mean age in middle stratosphere should not change, whereas the latter (correctly) acknowledges that an increase in tropical-extratropical mixing in the lower stratosphere will make the middle stratosphere mean age older (if no other changes). The former text needs to be changed so mixing is mentioned. The latter text also needs to be changed as ". . .regardless of change in the strength .." (line 26) is not correct.
The two comments on mean age changes in the stratosphere are not consistent because of imprecise formulations. We changed the text:
p.28413, l.10: "Although the effect of mixing is not included, this analysis is part of the explanation why Engel et al. (2009) found no significant trend of mean age of air calculated from in-situ observations of SF_6 and CO_2 with most measurements taken at altitudes and latitudes that should not be under the influence of the shallow circulation branch (indeed this possibility was noted in the paper)."
p.28415, l.19: "... LMS (45-60°N). The unchanged transit times in the high latitude LMS (60-90°N) is only a partial explanation for the observed unchanged mean age in the middle stratosphere, because an enhanced mixing in the lower stratosphere ..."

MINOR COMMENTS
1. I think the second sentence of the abstract "For this purpose . . ." can be removed. All this information is in the following sentences.
It is correct that all information is also in the following sentences. Nevertheless, the abstract is not too long and furthermore we feel that this very short mentioning of the applied methods and data helps the readability of the abstract. Hence, we would like to keep this sentence.

2. In figures 2 and 6 the labels say "STREAM" and "SPURT" with individual years for each panel, but captions say "pre-2000" and data from several different campaigns. Which is it? Labels and captions should be consistent.
We revised it, so that figure labels and captions are consistent.

3. Figure 5 and 6 need to be swapped.
We revised it.

4. I think it would be very helpful for the reader if a figure was included showing the difference in trajectories for 45-60N and 60-90N trajectories, i.e. showing former are shallower than the latter. Even if in Birner and Bonisch (2000) it is needed here for completeness and to make this paper self-complete.
Fig.1 illustrates schematically the location of the upper and lower branch of the TEM mass stream function and therefore also the shape of the residual trajectories. To our opinion, this is sufficient for the self-completeness of this paper. The more detailed analysis in Birner and Bönisch (2010) needed to diagnose the upper and lower branch, i.e. the transit time distribution, the aspect ratio and the net mass flux, must not be duplicated here.