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Interactive comment on "Global distribution of CO_2 in the Upper-Troposphere and Stratosphere" *by* Diallo et al.

Diallo, Legras, Ray, Engel and Anẽl

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Answer to anonymous referee #4

We thank referee #4 for his comments and suggestions. Comments by the referee are highlighted and followed by our answers.

Major Comments:

1. ***The evaluation of the data set is very specific and done on the basis of very limited and selected case studies: SOLVE only covers the high Arctic in winter 1999/2000. Similarly the four profiles are arbitrary snap shots, showing disagreement in one case, which is not even tried to be explained sufficiently (p.15).***

   The SOLVE dataset is not limited to the polar regions as we have included test flights and transit flights in the extratropics and the mid-latitude, that is six flights out of 12. Moreover, testing CO2 reconstructions in the polar region during winter is, a priori, the most difficult situation since the polar air is old and should cumulate all the errors in the transport. Regarding balloons, the number of flights with high quality CO2 data is unfortunately very small and only four are available in the 2000-2010 period. We have also extensively used CONTRAIL data which are partly stratospheric at mid and high latitude over a range of 4 years. Therefore we did our best to evaluate our dataset with the available observations. The sparseness of CO2 in the stratosphere is one of the reason of our work. See also our answer to item 3 below regarding the comparison with estimates of the age of air.

2. ***Further the discussion of the data and differences to literature is very superficial: The time delay between the cycles is mentioned but not appropriately discussed, since Boering et al., 1996 use N2O = 310 ppbv as reference values to determine CO2 at the tropopause, whereas in the manuscript a fixed altitude level is used, which is at or even below the tropical tropopause. This is not considered in the manuscript. Instead the authors conclude "...We recover a two-month delay at higher altitude in the layer 18–19km (not shown). The origin of this discrepancy is unclear but is perhaps due to the***

fact that previous studies merge measurements in the deep tropics and the subtropics."

There is actually no contradiction with Boering et al., 1996, as our 15 days delay is valid in a layer under the tropopause where the air is renewed by convection while the estimate of Boering et al. is associated with the lower tropical stratosphere where the age of air with respect to the crossing of the tropopause grows rapidly with altitude. It is about 6 months at 20 km (see Diallo et al., 2012, where the prediction of our trajectories is shown to fit the ER-2 measurements) and it is therefore not surprising that we find a delay of a 2 months in the 18–19 km layer in agreement with Boering et al. We have modified the discussion.

3. ***This is not satisfying for a quantitative reference data set over 10 years. Given the statements in the abstract (l.3-6: "...This product can be used for model and satellite validation in the UT/S, as a prior for inversion modelling and mainly to analyse a plausible feature of the stratospheric-tropospheric exchange as well as the stratospheric circulation and its variability..."), a quantitative assessment and evaluation also of the stratospheric data is required, e.g. by using age of air diagnostics. This would allow for comparison with other data sources or diagnostics (e.g Eyring et al., 2006; Haenel et al., 2015). For this the authors could also include SF6 to their analysis, since the authors conclude that their results hold for any long-lived species (p.20, l.14). Even if SF6 cannot be directly included, the age of air information can be inferred from the data. This would provide a quantitative comparison to evaluate the results on the basis of CO2 and the consistency of the results within the model. It would further help to evaluate their stratospheric data using satellite observations of e.g. MIPAS SF6 in regions where the in-situ data are sparse or absent. Even without SF6 the calculation of age of air allows for comparison with other data sets.***

   The same trajectories have been used in Diallo et al. (2012) to calculate the mean age of stratospheric air which has been compared with age estimates based on CO2 but also SF6 and N2O measurements, and also with the GEOSCCM model. It should be observed that SF6 is photolysed at high altitude and therefore cannot be as easily interpreted as CO2,

especially at high latitude during winter where a large amount of air has descended from high altitude [more detail see Stiller et al., 2008, 2012].

Minor Comments:

1. ***Abstract: Last sentence: Please clarify the sentence and specify: theres a contradiction: decrease or constant? Decrease of CO2 to 35 km or constant, constant with altitude above?***

   Done

2. ***Introduction: Do you need the first sentence?***

   Removed

3. ***p.3, l.2-4: The increase of green house gases does not increase tropical upwelling mass flux, it is the effect on atmospheric temperature structure and wave propagation.***

   Right. Corrected.

4. ***p.3, l.6: stratosphere instead of atmosphere? CO2 is destroyed in the upper atmosphere.***

   Done

5. ***p.4, l.7: Here you need to mention the Engel et al., (2009) study - not on the previous page (l.25), since it is not beased on airborne measurements.***

   Done

6. ***p.7, l.2: You forgot TRACZILLA***

   Done

7. ***p.8, l.20-22 (and l.10 ff): What does this mean: Similarly to 1989-1999.... only at 5 km ? Please specify the altitude criterion of the selected stations for 1989-1999. How many stations contribute? It would be good to show a 3D distribution of the boundary condition: e.g. a zonal mean plot with latitude as y axis, time as x-axis and CO2 as***

iso surface above to see the global distribution and allow for comparison with e.g. the NOAA CCGG data.

Corrected. The 1989-1999 is based on ground stations that are far from sources. The criterion to select the ground stations is that the elevation is high enough to neglect the variability due to localized sources at ground level.The ground boundary condition would project on a 2D map but it contains a large amount of variability which is not relevant here. We choose the 500 hPa surface as a boundary condition after 2000 in order to filter out the surface fluctuation and reduce the number of needed trajectories. The CarbonTracker CO2 which is used to initiate our trajectories is perhaps the best currently available tropospheric CO2 in the range 2000–2014 because it assimilates all available observations, including CCGG data, to produce a 3D distribution of CO2 in the troposphere. Therefore a comparison with CCGG data will be redundant and anyway we do not aim at outperforming CarbonTracker in its domain of validity. Our dataset applies above 500 hPa only. The NOAA CCGG webpage higlights the Carbontracker CO2 http://www.esrl.noaa.gov/gmd/ccgg/.

8. ***p.10, l.11: Wrong sentence? Something is missing...?***

   corrected

9. ***p.11, l.3: Is a zonal mean calculated? What are typical numbers of parcels per box?***

   We launch 10.255 particles per levels and we have 30 levels. The discretization is described in section 2. longitude=2/cos(lat) and latitude=2 degrees. In the tropics we have 180 particles that decrease as 2/cos(lat).

10. ***p.12, l.4: Chapter title: The term validation is used, but one cant validate the results, since you can have agreement for the wrong reasons. Therefore I suggest the term evaluation.***

    Okay... Even if here it's not the case.

11. ***p.12, l.15 ff.: The exponential factor b clearly depends on the driving data set. Does the exponential factor b further depend on the choice of the trajectory model and needs in principle to be determined for each individual trajectory model?***

The exponential factor is a statistical quantity which can only be defined for a large ensemble of trajectories and not for a single one. It is defined for the whole stratosphere at a given date but varies vey little. We made trials of defining by averaging in time for a given set of latitudes and altitudes with negligible effects.

12. ***p.13, eqn.4/5: Please clarify the notation of vectors, scalar products and sclara quantities. Why is the t in bold font?***

    Corrected.

13. ***p.13, l.11: ".. kappa defined by the user..." Is k (kappa) chosen to have the same value in the whole atmosphere? Please add a word, which values have been selected or how a user has to define Kappa.***

    Yes the diffusion is chosen to be the same for the whole atmosphere. We have replaced this discussion by a reference to Legras et al., 2005 where a thorough discussion of the determination of diffusion is provided. It should be recalled that diffusive dispersion of trajectories is only effective during the first 3 or 4 days of the backward integration, hence the chosen diffusivity has to be valid in the lower stratosphere only.

14. ***p.15, l.19-23: Please explain, how a cold front (which is a tropospheric feature) can affect the CO2 a 25 km altitude. The inset in Fig.3. is too small and does not contain a legend. It is further unclear, why the PV gradient should be associated with a CO2 gradient. This paragraph sounds very weird or almost wrong.***

    There is nothing weird here but the mere usage of common concepts in dynamic meteorology. When submited to chaotic stirring, all long-lived tracers that are bound to preserve their tracer-tracer relation tend to align their contours and therefore the CO2 contours are likely to follow the PV contours and to exhibit high gradients at the same location of high PV gradients (as would ozone or N2O do as well). A cold front is the surface signature of a deep structure that penetrates the stratosphere. The map shows the PV distribution at 18.5 km well above the mid-latitude tropopause located at about 11 km. A similar case has been studied in depth by Pisso and Legras, 2008. See also Miyazaki et al., 2009.

15. ***p.15, l.27-29: "... The mean in situ CO2 from observations is much more spread in the high latitude profile (44° N) above 15 km. There is not a clear explanation about these observed fluctuations on the in situ CO2-profile.": What do the authors want to say with such a statement? What does this mean for the comparison? What does it mean for a data set, which is intended to serve as a reference for model evaluation from 2000-2010, if one out of four stratospheric profiles does not fit the observations?***

    It is well known that modelling tracer distribution is highly prone to transport errors in the region of large gradients. It was shown by Pisso and Legras, 2008, that the global pattern is weakly affected but a small displacement can in this case generate important deviations. Therefore it is expected that the test will fail in such a case which cannot be used as a reference. The discussion has been rewritten.

16. ***p.19, l.20 and l.23: What is meant with "the subtropical barrier" in this paper? Do you mean the subtropical jet at the tropopause, which exhibits a seasonality with weaker PV gradients and high permeability in summer? The subtropical barrier normally denotes the boundary of the (leaky) tropical pipe in the overworld (e.g. Palazzi et al., 2009), which does not show the same variability as the STJ and has a different generating mechanism.***

    By subtropical barrier we mean "the subtropical jet at the tropopause, which exhibits a seasonality with weaker PV gradients and high permeability in summer". This is a common terminology in dynamic meteorology where it has been known for a long time that the jet centered at 200 hPa and about 30°N and S inhibits exchanges between the upper tropical troposphere and the lowermost extra-tropical stratosphere.

17. ***Fig.2: Please include potential temperature along the flight track as additional information. Otherwise the information on the plots is without any relevance for a scientific interpretation and an estimate of the quality of the model capabilities. Does the gray area refer to the variability of the data in a bin? If not, how is the error calculated?***

    Potential temperature along the flight-track has been included. The gray area is 95% confidence interval estimated from the model results.

18. ***Fig.5: The continuous color bar is not consistent with the figures, which have discrete colours. Please provide a discrete color bar legend.***

    Done.