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

M. Diallo et al.

mdiallo@lmd.ens.fr

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[acpd,hvmath]copernicus$_{discussionscolor}$

m.diallo@fz.juelich.de Answer to referee #1 Diallo, Legras, Ray, Engel and Anẽl

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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

November 29, 2016

Answer to anonymous referee #1

We thank referee #1 for his comments and suggestions. Comments by the referee are highlighted and followed by our answers.

Although the paper contains some interesting material which should be published, the manuscript itself has poor quality in many parts. The preparation of the manuscript was not done with the necessary care. Several paragraphs and sections need substantial revisions! The structure of the paper is fair but the grammatically preparation is superficial and the discussion with regard to the scientific content is inadequate. Particularly Chapters 5 and 6 are unclear and both do not contain a detailed discussion of the data product. In the following here are my major points and general concerns:

Major points:

1. ***In Section 3.1, I am missing a critical discussion of the boundary conditions for your exercise, for instance regarding the used data (i.e. the 1989-1999 time period from ground stations): what are the uncertainties of the calculated trajectories (e.g. with respect to the linear interpolation along latitudes (page 8, line 15-16))? Why are the CO2 values from the CarbonTracker considered only at 5 km altitude? Are the results of TRACZILLA affected by the assumed initial conditions? How reliable are the TM5 results in comparison to other CTMs? At least it would be necessary citing relevant literature to motivate your approach – this section does not show any reference.***

   It's not straight forward to estimate of the initial condition in the 1989-1999 as there's no reference global distribution of the CO2 concentration. The linear interpolation is meant to avoid artificial discontinuities in the assignment. The basic uncertainties in the trajectories arise from errors in the advection fields that can be sensitive in some limited circumstances, as illustrated in the comparison to balloons and aircraft measurements. It is proven (see Legras et al., 2005 and Diallo et al., 2011) that the reconstructions are weakly sensitive to the duration of the integration and to the small-scale patterns of the data used in the initialization. Only large-scale bias would be seen in the reconstructed CO2. The CarbonTracker data are used as a boundary conditions at 5km as it is a convenient way to initialize the trajectories as they cross this surface which lays entirely in the free troposphere. The representation of vertical transport by diabatic heating and the mere usage of potential temperature as a vertical coordinate is highly unpractical closer to the surface. This is better handled by a tropospheric transport model like TM5, now properly referenced, but the details of which are highly irrelevant for our purpose. CarbonTracker data is mainly used as a low pass filter which damps the daily and local fluctuations of CO2 at the surface and carries the

   low frequency and large scale ground signal to the 500 hPa surface in a matter of days. A precision, we don't use TM5 data but the data from the assimilation system CarbonTracker which is coupled with TM5 for the transport.

2. ***Regarding Chapter 4, I am missing at least a final statement (paragraph) discussing the uncertainties with respect to the final data product. For example regarding the point mentioned on page 13, lines 18-19: "After the backward integration of few months, then the air particles of the diffusive run are fixed by using the ages calculated from the global initialization." What does this mean? How reliable are these assumptions? What is the range of uncertainties here? Again, no reference is cited in this connection. Similar on page 14, line 7 you stated: ". . . with a diffusivity parameter k equal to 0.1 ...". It would be helpful to provide more information and a critical discussion of the assumptions used here.***

   This paragraph that contained an error has been rewritten. The method used here is based on previous works (Legras et al., 2005; Pisso and Legras, 2008) based on long-lived tracers which provide constrains on the diffusivity. We refer the reviewer to these previous works which contains a thorough discussion of the sensitivity of reconstructions to the diffusivity.

3. ***Chapter 5 is really poor. The description and explanation of results in Section 5.1 is very vague and need a substantial revision. This kind of evaluation (i.e. a comparison of colored lines) presented here is not sufficient. For example (page 14, line 24): "The large peaks seen, e.g. . . . are due to dives and the discontinuities on . . . are also due to fast altitude changes." It is difficult to understand what you are meaning. In particular the mentioned flights are not fitting together, some of them are missed . . . and by the way, Figure 2 is not mentioned at all in Section 5.1. Figure 2 is showing some discrepancies? What are the specific reasons? What is the grey shaded area standing for in Fig. 2? What is needed here is a detailed description***

of the data and a critical discussion of the strength and weaknesses. Your final statement (page 15, line 11) "... explains very well the measurements ..." is not sufficient.

This section has been heavily rewritten

4. ***I have the same concerns with respect to the other sections in Chapter 5. In the beginning of Section 5.2 a detailed comparison is announced, but it is definitely not given in the following. It is still a description of pictures. And it is a mess: Four balloon flights are presented in Fig. 3, three of them are "showing" a good agreement (but only two of them are mentioned in the text; page 15, line 19); one shows significant differences!? The explanation at the end (lines 22-23) is " ... the errors of the reanalysis which are not accounted in our statistical test might be large enough to explain the shift." This is definitely not an adequate explanation!***

   We have improved the discussion. The third case is associated with a meteorological front, a situation which is well known to amplify the advection errors as previously shown by, e.g., Pisso and Legras (2008).

5. ***In Section 5.3 you said that you choose the latitude ranges 10S-20N and 50-60N. What is the reason for chosen these bins? Why are the latitudinal bins different? A more detailed discussion is necessary. Again a statement like "The origin of this discrepancy is unclear, but is perhaps due to the fact that previous studies . . .". By the way: which studies (no references are given)? And at the end of this section the sentence (page 17, lines 5-7) "The large discrepancies ..., as it will be confirmed shortly." is not comprehensible to me.***

   The reason of the choice of the two latitude bands is to select two regions respectively representative of the tropics and of the extra-tropics. Then the altitude ranges are dictated by the location of the tropopause in these two regions, re-

   spectively at 10-11 km and at 17-18 km. We have improved the discussion and added references.

6. ***What I have said in general with regard to Chapter 5 is also valid for Chapter 6. A more detailed discussion of the results and their evaluation are required. Some particular points with respect to Chapter 6: You are discussing results of the year 2010 only in Sections 6.1 and 6.2. What are the other 9 years showing (regarding annual cycle, variability, fluctuations, etc.)? Are they "identical" (similar) or different? Is 2010 exemplary for the other years? Beyond that, no results are showed in Section 6.2 (not either in other sections) regarding the upper stratosphere (all figures show results at altitudes below 35 km). With regard to the Conclusions you stated (page 21, line 3): "In the deep stratosphere, we have found ... ". This remark is not supported by investigations presented before.***

   The results of the year 2010 is considered here as exemplary for the other years. The annual cycle of the zonal mean CO2 for the other 9 years look similar but differ to a growth rate that changes year to year. Due to this changing growth rate, the variability of the stratospheric circulation is better studied using the age of air as in Diallo et al. (2011). The 10-year average CO2 profiles after removing the mean from the CO2 are described in section 5.3. We have extended the top level to 42km in Fig. 6b.

7. ***A separate discussion Chapter at the end is necessary, especially an assessment and classification of the data set regarding the current knowledge. In particular to point out the "new" findings and a rating about the quality of the provided data set would be helpful. As announced in the Introduction (page 6, line 21) "The global (!) distributions of CO2 and its transport in the UTS (!) are investigated . . .". Therefore, it would be nice to see some "global" charts, covering the entire stratosphere in this paper: The final sentence of your manuscript (page 21, lines 12-13) makes me***

curious, but no respective illustrations are shown in this paper. Could you please show (and discuss!) some examples, not only to motivate using this comprehensive data set?

The final section has been rewritten to emphasize the results. We do not fully understand the request of the reviewer as Fig.6 is an example of the global charts which are provided from our latitude x altitude product.

Minor points:

Many acronyms are not explains, in particular in the Introduction.

These acronyms are common. Avoiding them will remove the message that we want underline here and it keep the sentence shorter as well.

Page 3, line 19: . . . increase global change . . .

Done

Page 4, line 14: . . .observations that have high resolution and are very localized, . . .

Done

Page 5, line 29 and in particular on Page 7, line 3: TRACZILLA is not explained (at least references are required).

Done

Page 10, lines 14-17: need a Revision

Done

Page 10, line 22: "It is assumed that the vertical mixing is fast . . ." Why? Reference is needed? How fast? Please explain.

It's assumed the vertical transport is fast mostly driven by convection.

Page 14, line 7: Units should be corrected.

Done

Page 15, lines 15-16: ". . . of four middle latitude stratospheric balloon flights . . .".

Done

Page 20, lines 4-5: "... based on 22 years of data ... over the last 11 years ...". Sentence should be revised.

Done

Page 21, line 3: "In the deep stratosphere we have found ... " no results are shown or discussed in detail.

Done

Figure 4: the chosen latitudinal bins do not exactly correspond to the coordinates of the stations providing the measurements.

Done

Figure 6: the box in the right figure (top right) says "2000-2011"; should be 2000-2010.

Done