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

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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 Anêl

November 29, 2016

Answer to anonymous referee #2

We thank referee #2 for his comments and suggestions. Comments by the referee are highlighted and followed by our answers.

1. ***The global product requires uncertainty estimates/bounds and a description of that derivation. This is essential.***

   We have estimated the uncertainties of the global CO2 distribution and added a subsection which describes the deviation (see Fig.6c ad sect. 5.3).

2. ***The manuscript needs to clarify the product for global CO2, that it is 2-dimensional (varies with latitude and altitude but not longitude). This was unclear earlier in the text, and only clarified in the final conclusion sentence. How this was generated in terms of where the receptors for the***

trajectories were distributed in the stratosphere is unclear. How are the initial air parcels distributed in time and space, and are they run from different longitudes and the results averaged in zonal means?

The reconstructions is indeed produced by averaging over all parcels launched on a latitude circle. Further grouping in latitude bins is made near the pole. This is explained in section 3.1. and 3.2. The reconstructions of balloons and aircraft measurements at given locations is based on multiple launching from the same point and diffusive trajectories as explained in section 3.3.1

3. ***Grammar and spelling should be checked throughout the manuscript. I noted many of the errors below, but it should be thoroughly proofread. Many sentences are awkward. The quality of the writing seems to deteriorate even more in the final few pages, with basic typographical errors and usage of words that are not words.***

   This aspect has been hopefully improved.

Specific comments:

Overall, the journal editor can comment on whether italics are or are not appropriate where used in this manuscript, generally when defining a term or acronym (for example "tropical pipe" is italicized throughout the paper).

The italics term have been removed from the text.

L21: decreases with altitude... is nearly constant with altitude - contradictory. Perhaps the authors intend that CO2 decreases with altitude from the UT to the S but is nearly constant with altitude above 35 km?

Yes that we meant here! The CO2 concentration decrease up to 35km and above it is nearly constant.

Page 4: L 6: where recently assessed from balloon-based
L 28, qualitative? Perhaps better worded would be "shown qualitatively good agreement with in situ observations..." Last line and L1 of Page 5, awkward sentence

We have rephrased this sentence

Page 5:
L25 grammar
L18 this diffusivity effect
L29 models
I don't completely understand why Lagrangian models would not be subject to these problems if the underlying transport model is flawed.

Grammar corrected
CTM are usually run at low resolution due to the numerical cost of chemistry. They are therefore highly diffusive and can generate spurious diffusive transport and mask barrier effects due to the dynamics. Lagrangian models are purely advective and not bounded by diffusion. Added diffusion is however necessary to represent missing turbulence in the advective winds but it is several orders of magnitude smaller than the spurious numerical diffusion of CTM.

Page 6: L10: by scarcity are the authors referring to the scarcity of CO2 observations? confusing sentence, possibly because of wording /grammar errors.
L15: is the ERA-interim analysis used in any of the previously referred-to flawed CTM models that were unable to correctly capture transport?
L15 ERA-Interim definition should be moved up here from Line 20-21.

This paragraph is rearranged and improved.

Page 7: L2: the Lagrangian transport model TRACZILLA (Legras ..), a modified version....
L5: I see now that the Lagrangian model is calculating its own vertical motions, unlike perhaps the CTMs mentioned above, and that is why it performs better?
Line 15: Please clarify the assumption being made here: this corresponds to the assumption that the CO2 in the troposphere (i.e. below this boundary condition) is constant?

The vertical motion is derived from the archived heating rates of the reanalysis. This procedure is also quite common among the stratospheric CTM. The heating rates allow to better separate the horizontal (isentropic) motion and the vertical (cross isentropic) motion. They are also smoother, being accumulated quantities, than the vertical velocities which are instantaneous sampling (undersampling actually) of fast varying fluctuations. As 500 hPa is taken as the lower boundary of the domain, trajectories starting below (as it may happen only in high surface pressure regions) are discarded.

Section 3.1, page 8. The use of clean-air data at the ground, no matter how clean the site, is a bit worrisome. Since Carbon tracker has also been used for the later years, how would it impact your results if you used a lower or higher CarbonTracker level? The 5km level is not only above the PBL, it is significantly higher than the other stations that you use in 1989-1999. It would be worthwhile to investigate the vertical gradient in CarbonTracker to see what kind of error you have just by choosing one level and assuming it is constant. Also it would be useful to look at CarbonTracker residuals against the NOAA North American aircraft network data at altitudes above the PBL, to see if it is a realistic depiction of CO2 mole fractions in the free troposphere and upper troposhere (those profiles go to 8 km).
http://www.esrl.noaa.gov/gmd/ccgg/carbontracker/profiles.php
If residuals are small, then using the CT gradient might be a good way to determine the uncertainty on the assumption and the choice of only one level. CT can

also be used to investigate any longitudinal errors/differences.
It is not clear if the CarbonTracker mole fractions are considered as an average over all longitudes, or for the specific grid cell where the back-trajectory initiates.
L22-23: This is not a great description. CarbonTracker assimilates CO2 observations from atmospheric stations and optimizes underlying fluxes from the listed sources (ocean fluxes, biosphere fluxes, fire and fossil fuel).

The reconstruction procedure is well posed by using a given pressure surface as a lower boundary. As far as vertical transport in the lower troposphere occurs in a matter of days, this is not going to impact our results. Taking the CarbonTracker data at 5km filters out the ground fluctuations and allows to use a fairly limited sample of trajectories to provide a smooth reconstruction.

Page 10: L19: I repeat my question from before, which could be addressed here - it is assigned based on the lat/lon of the boundary crossing after 2000 but prior to 2000 it is only the latitude that matters?

Yes, the attribution of the CO2 concentration to the back-trajectories after 2000 is made depending on the lat/lon position of the air parcel for a given time. We only averaged the 3-hourly CO2 mole fraction from CarbonTracker in daily CO2 concentration that we use to initiate the trajectories.

Page 10-11: Are the particles in each bin spread evenly throughout? I.e. is their initial location in the center of the bin or evenly distributed in time and space? this may have been mentioned earlier but should be clarified here. (Page 11, line 6 discusses how many particles there are per bin, so this would be a good place to discuss how they are distributed in time over the month and in space).

The air parcels are distributed uniformly in longitude 2°/cos(lat) and every 2° for the latitude. At the end of each month, we distribute at each altitude level 10250 particles which are driven with the ERA-Interim winds.

Page 12, L10-15, this discussion of diffusion answers my previous question about the difference in the models - it could be briefly mentioned earlier in the paper to clarify this.

Okay good! In this section, we calculate these reconstructions at a given location and therefore need to proceed differently than the global reconstructions.

Page 13-14. Perhaps you could clarify why this 6-month dispersion of particles is required before the particles are tagged with the value of the global reconstruction. I would think you could just sample your global distribution at the location of the observations?

As the local variability sampled by in situ measurements is mainly due to the chaotic properties of advection, it is necessary to integrate backward in time to reconstruct this variability from the grossly sampled global reconstruction. Launching only one parcel from each measurement point would reconstruct fluctuations growing with the backward integration time. Using a cloud of diffusive trajectories generates a reconstruction that does not depend on this integration time once it is long enough (see Legras et al., 2005).

Page 14, Line 21, reference to figure needed in text here.

Corrected

Page 14, L23: This is the first reference to a CI for the reconstruction. How is it calculated/obtained? The CI or uncertainty is very important to anyone who would be using this product, and the methods for its calculation should be well documented in this paper. Presumably some of this comes from the particle/diffusion release of the 6-month trajectories from the flight, but there should be an uncertainty associated with the global CO2 product as well.

The confidence interval of the reconstructions at a given location is clearly an upper bound of the uncertainty of the global product.

Fig. 2: I would like to see differences in addition to the time series of mole fractions, or correlation plots with $R2$ statistics related to bias and/or RMSE; i.e. a more quantitative comparison. A plot or a table of statistics, or perhaps a single plot with all the flights colored differently, would not add too much length or bulk to the paper.
Fig. 3 shows the differences well enough that it seems fine, and statistics would be difficult to calculate for such a small sample.

The correlation and R^2 statistics have been added in a separate figure.

Page 15, L23: We have no information on the error calculation so this sentence is confusing.

This paragraph has been rewritten.

Page 16, L 13: CONTRAIL should be described somewhere (in the section where SOLVE and the balloon flights are discussed in Section 3.2).

A paragraph describing CONTRAIL data have been added

Page 16, section 5.3, why is CONTRAIL treated differently from Solve and the balloon observations, and no 6-month diffusion of particles is conducted? (this is the same as my earlier question about why that was done for those observations, there should be some explanation for this).

The SOLVE and balloon observations are very localized compare to the CONTRAIL that has a large spatial and vertical coverage over several years as shown in Sawa et al., 2008. SOLVE and balloon observations were used to evaluate the ability to reproduce high resolution in situ measurements while CONTRAIL is a more direct test of the global distribution.

Fig 4, some uncertainty bounds should be calculated and shown for the global initialisation using TRACZILLA. (could be based on differences from Contrail, or based on the spread of values of the trajectories that constructed each bin). Some error is likely also introduced by the choice of the level in CarbonTracker (looking at any vertical gradients in CT product could help quantify this, perhaps it is very small).

We have calculated the standard deviation using the spread of values of the trajectories that constructed each bin.

Page 16: Why was 15 days chosen, was it because that delay fit the data best?

Yes! 15 days was chosen because that delay fit the data best at this height but we recovered the 2 months delay at 19-18km above the tropopause.

Page 16, last line: give mean, standard deviation of differences?
Page 17, Line 6-7: in this period, discussed further in section xX.
Page 17, Line 18: awkward description of the biospheric CO2 seasonal cycle
Page 17, L19: CO2 concentration in the UT increases (subject /verb agreement in this sentence).
Fig 5, caption should indicate that the source of this data is the CO2 global reconstruction

Done

Page 18:
L3: tropics
L13 maximum
L14 observations, also refer to CONTRAIL here to clarify this is the same data set used in the earlier comparisons

L25 isolates
L26 no comma after effect, also this is still awkward phrasing
L27 homogenizes
L28 containment

corrected and sentences rephrased

Page 19:
L9 profiles.... exhibit
L10 I don't think interspelled is a word, please rephrase (perhaps interspersed, or alternating?).
L16, processus (process)
L16, injects

corrected and sentences rephrased

Page 20
L7 that should be which
L11 "This good agreement demonstrates that the Lagrangian model (TRACZILLA) ..."
L23-25: awk sentences (high horizontal mixed and uniformise)
L17 extent
L29 troposphere

corrected and sentences rephrased

Page 21
L1 variability
L1 subject/verb (variability are)
L11 measurements should not be capitalized

corrected and sentences rephrased