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

Anonymous Referee #4

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The authors want to provide a new data set of CO2 for model evaluation and the capabilities of models to simulate transport in the stratosphere. For this purpose they construct a CO2 data set on the basis of a Lagrangian approach (TRACZILLA) by developing a ten year zonal mean climatology of CO2 on the basis of ERA Interim data. They use Carbon Tracker CO2 fields in the free troposphere to build this climatology for the stratosphere. Comparisons to CO2 from the SOLVE campaign in winter 1999/2000 over the northern Arctic are presented. The model data are further compared to measurements of CO2 from the SOLVE and CONTRAIL project covering several years of CO2 measurements from commercial air liners mostly between Europe and Japan over mid-latitude and northern Asia. Four mid-latitude balloon-borne profiles allow for comparison of CO2 deeper in the stratosphere of which one shows considerable deviations to the model data, whereas the other show a remarkable agreement between model

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Discussion paper

and observations. The authors show that TRACZILLA is capable of partly reproducing the CO2 variability also on small scales as provided by these data sets. Based on the few comparisons they claim to provide a new data set for model evaluation from the upper troposphere throughout the stratosphere for the years 2000 to 2010.

In general such a data set is highly desirable. However, in the presented form and with the methods used here, the validity of the CO2 data set for such a task is not provided. The construction method seems to be valid, but the evaluation and interpretation of the results is confusing and based on limited data. Further, no statistical measures or quantitative metrics are used, which would really allow to draw conclusions for the general applicability of the data set as stated in the conclusions. The authors only provide model to observation comparisons for a few limited time series and profiles. Statistical robust evaluation methods are not applied at any point. Also the implications of inconsistencies to previous studies (e.g. the CO2 tropopause condition) are not discussed, nor the reason for inconsistencies. If the data set presented is out of phase at the tropopause by 6 weeks, how is it possible to get the stratospheric CO2-distribution correct?

The more important is a careful analysis of the stratospheric CO2, where observations are sparse. Surprisingly they further don't calculate age of air, which should be easily possible with CO2 at least at altitudes, where the seasonal cycle of CO2 disappeared. I judge this as essential before claiming a reference data set for model evaluation.

I therefore think the paper should be resubmitted to ACPD, after 1) more quantitative statistical evaluation is provided 2) an age of air discussion and comparison (e.g. to MIPAS or balloon-borne data) is included to evaluate the stratospheric data 3) a thorough discussion and definition of processes and regions is given and used (e.g. the term 'tropical pipe')

A data set as intended by the authors would be of high interest for the community, but in the presented form it is just a CO2 data set, which fits some selected and very limited observations.

Major point: 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).

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." - 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.

Specific points: Abstract: Last sentence: Please clearify the sentence and specify: there's a contradicition: decrease or constant? Decrease of CO_2 to 35 km or constant, constant with altitude above?

Introduction: Do you need the first sentence?

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.

p.3, l.6: stratosphere instead of atmosphere? CO_2 is destryed in the upper atmosphere.

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.

p.7, l.2: You forgot 'TRACZILLA'

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 CO_2 as iso surface above to see the gloabl distribution and allow for comparison with e.g. the NOAA CCGG data.

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

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

p.12, l.4: Chapter title: The term 'validation' is used, but one can't validate the results, since you can have agreement for the wrong reasons. Therefore I suggest the term 'evaluation'.

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?

p.13, eqn.4/5: Please clarify the notation of vectors, scalar products and sclara quantities. Why is the 't' in bold font?

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.

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.

p.15, l.27-29: "... The mean in situ CO2 from observations is much more spread in the high latitude proïňĄles (44 âŮę N) above 15 km. There is not a clear explanation about these observed ïňĆuctuations 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?

p.19, l.10: What are gradient layers?

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.

Fig.2: Please include potential temperature along the flighttrack 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?

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

References:

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