Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-382-RC3, 2016

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 21 June 2016

The manuscript describes a method and a reconstruction of global CO2 mole fractions through the Upper troposphere and stratosphere. The CO2 reconstruction is based on a back-trajectory model that samples tropospheric mole fractions when the trajectories end at a boundary in the troposphere. The results compare very well with observations. The manuscript requires improvement: the methods are not clear enough (details are in the specific comments below), and, most importantly, there is no uncertainty estimate at all. This is vital if the product is going to be useful to other scientists. The authors should also indicate how this product will be available for public research use. In general, the methods seem sound, and the work should be published after major revisions.

General notes:

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

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?

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.

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).

Abstract:

L13: the potential of [a, or the] Lagrangian model to reconstruct...

L18-19 enter should be enters

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?

Page3: L3: should be "The increase of greenhouse gases, "

L14: seasonal cycle

L19-20 awk sentence (what is "its potential" referring to? - should be "their", in ref. to CO2 abundance and variability?).

L20 should be "abundance".

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

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.

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.

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 con-

stant?

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 significatly 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).

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?

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 distrubited in time over the month and in space).

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.

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?

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

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.

Fig. 2: I would like to see differences in addition to the time series of mole fractions, or correlation plots with Rˆ2, 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.

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

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

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).

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).

Page 16: Why was 15 days chosen, was it because that delay fit the data best? This is significantly shorter than 2 months - perhaps this could be made more clear, that these are the two numbers being compared in the discussion in this paragraph (15 vs 60 days).

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

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

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

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

Page 21

L1 variability

L1 subject/verb (variability are)

L11 measurements should not be capitalized

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-382, 2016.