Atmos. Chem. Phys. Discuss., 11, C5546–C5550, 2011
www.atmos-chem-phys-discuss.net/11/C5546/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Three-dimensional variations of atmospheric CO_2: aircraft measurements and multi-transport model simulations" *by* Y. Niwa et al.

Anonymous Referee #1

Received and published: 28 June 2011

The manuscript describes comparisons between transport model simulations with observations of CO2 from commercial airliners made within the CONTRAIL program. Four different transport models are combined with two different flux estimates for CO2. Apart from the model-observation comparison of the CO2 distribution for different areas and seasons, the authors conclude that the contrast between the northern hemispheric sink and the tropical source needs to be larger that found in previous studies using extensive aircraft observations. I recommend publication only after some major revisions on the manuscript, taking into account the comments below.

Main comments: The main finding of a contrast between tropical source and NH sink

that is substantially larger than what was found by Stephens et al. 2007 (in the following referred to as S07) needs to be more substantiated. The authors speculate on the source-sink distribution based on simulations with four different transport models and two different flux distributions and on the comparison with CONTRAIL data. One of the flux estimates used is a bottom-up estimate not generally consistent with the atmospheric constraint, and the other is based on inverse estimates valid for a different time period. One way to improve this would be by using actual inverse estimates based on the different models, following an approach similar to the one used by S07. This would make the supposed differences to the S07 study more clear: are the transport models so different from those used in S07, or are the CONTRAIL data inconsistent with the regular profile data used in S07?

Using fluxes from a different dime period is not state of the art, as interannual variations are likely to happen (see e.g. results from different inversions on the webpage carboscope.eu). Also the flux distribution might change on interannual time scales. Just saying that both periods were similarly affected by ENSO is not sufficient. Further, it remains unclear which flux year was used, was it an average of 1999-2001, or a specific year?

One concern is the use of four transport models, of which three are driven by or nudged to the same JRA-25/JCDAS winds. I would also recommend to add a table describing the different transport models and their characteristics based on the tracer simulations, e.g. with columns ranking interhemispheric exchange rate from SF6, vertical transport rate from radon, and the CO2 gradients during the growing season.

The separation of transport uncertainty and flux uncertainty is a difficult task, but is key to making inferences about the flux distributions. The way this is handled in the manuscript is not very convincing. Using fluxes that are not consistent with surface observations (i.e. not inverted fluxes), the authors make statements about the impact of different fluxes vs. the impact of different models on the modeled CO2 distribution. With two flux distributions, that are sometimes different in certain regions, and sometimes not, this can lead to wrong conclusions. Inferring the impact of transport uncertainty from the differences between the different transport models is also not appropriate, given the small number of models and their lack of independence. Also regarding these issues the paper would benefit from using inverse estimates based on the different transport models for the specific period.

Technical comments:

P12806 L18: replace “free-troposphere” by “free troposphere”

P12806 L25: replace “regional budget” by “regional budgets”

P12807 L20: add “the” before “FT”

P12807 L26: please replace the Xueref-Remy 2010 ACPD reference by the ACP version that appeared recently

P12808 L9: What do the authors mean by “ecophysical”?

P12808 L14: I would suggest to replace “Because the multi-model framework” by “As a multi-model framework” to make the sentence more clear

P12809 L9: “bottom-up” should be introduced (or dropped)

P12809 L24: same for “top-down”

P12812 L27: may be replace “The horizontal travelling length during taking-off and landing ranges about 200–400 km.” by “The horizontal distance travelled during the profiles from ascents and descents is about 200–400 km.”

Fig. 2 (b): Orange line (CDTM) shows strange increase at the beginning of the time series shown. Is this an artifact of the plotting software?

Fig 3+4: Why are CO2 differences shown for JAS, while in Fig. 3 radon is shown for JJA? It would be helpful to have the same periods for Rn and CO2. Also it would be interesting to also have radon at 850 mbar for comparison.

P12815 “averaged correlation coefficients of each vertical profile between the observation and the model mean” this is somewhat unclear. How was this correlation calculated?

Fig 5, caption: “mean standard deviation” should be explained. Is it the standard error of the mean delta-CO2 shown at each vertical bin? If those horizontal lines were taken seriously as error bars, most vertical gradients as well as model-measurement differences would obviously be insignificant.

P12816 L22, also P12817 L2: The fact that changing fluxes have an impact on the gradient (which is not surprising) does not proof flux uncertainty to be a significant cause for too small gradients. It could still be dominated by transport uncertainty. Especially over the IND region, the authors should consider potential transport pathways that could lead to a mismatch. Given that there is significant convective activity, also aircraft profiles will not be unbiased, as they will avoid strong convective cells.

P12817 L8: “12.5gC/mˆ2” is not a unit for fluxes. Also, only knowing the difference but not the magnitude of either flux1 or flux2 makes this hard to interpret for the reader.

P12818 L12: Again, only flux errors are discussed. Can it be excluded that there is a problem with transport in the models?

P12818 L24: The representation of fossil fuel emissions in the vicinity of many of the profiles should be assessed with more care. Has a selection been made on the data for wind direction? Is this only a problem for SSA, not for any of the other regions?

P12821 L25: How was the correlation determined? A correlation coefficient of 0.7 suggests 50% explained variance. The double minimum feature in the FT is not really well captured by the models, and probably does not contribute much to the variance.

P12822 L2: Why is “CO2 variation . . . intruding towards the south . . .” a reason for models to underestimate the seasonal amplitude? Which is the process that is not properly represented in the models?

P12822 L5-8: It should be mentioned that the observed latitudinal gradients are larger than the simulated gradients at both altitudes.

P12822 L15: add “transport” before “model uncertainty”

P12822 L5-16: When assessing impact from flux vs. impact from transport, the difference in fluxes (Flux2-Flux1) is important and needs to be mentioned. For this table 1 should be augmented to include the JFM and JAS flux budgets in addition to the annual totals.

P12822 L23: There seems to be an inconsistency: table 6 shows observed gradients of 4.7ppm in the MBL, not 5.2ppm.

P12822 L26: Only under the assumption of perfect transport.

P12823 L1: To say that there is no strong impact by uncertainty in the modeled vertical transport is I think going too far. The latitudinal gradients are indeed underestimated at both altitudes, but to a quite different degree, especially near 20-40 N.

P12823 L1: The speculation about the location and magnitude of the fluxes should be tested by adjusting the fluxes in the simulation, or better by using inverted fluxes based on the different transport models.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 12805, 2011.