Atmos. Chem. Phys. Discuss., 15, C2900–C2903, 2015
www.atmos-chem-phys-discuss.net/15/C2900/2015/

Interactive comment on "Simulating CO₂ profiles using NIES TM and comparison with HIAPER Pole-to-Pole Observations" *by* C. Song et al.

Anonymous Referee #2

Received and published: 23 May 2015

The authors have conducted an evaluation of the NIES Transport Model (TM) using aircraft data from the HIPPO campaigns. Previous evaluations of the model used column-averaged dry air mole factions (XCO2), however, in this study they used in situ aircraft data, which allows them to evaluate the model in the upper troposphere and lower stratosphere. The new NIES TM uses a hybrid sigma-isentropic vertical coordinate, which, in principle, should provide a better description of the large-scale meridional transport in the upper troposphere and throughout the stratosphere. Unfortunately, there are only a few transport models that employ this approach. It is, therefore, good to have more studies assessing the performance of the model. Fur-

thermore, the HIPPO data are ideal for evaluating the model. The manuscript is well written and, as a purely model evaluation paper, I would recommend publication of the manuscript after minor revisions to address my comments. However, for publication in ACP the manuscript should have greater scientific results. I therefore cannot recommend publication of the manuscript in ACP in its present form. As a model evaluation study, the manuscript is better suited for Geoscientific Model Development than for ACP.

Comments

1. Page 6753, line 9: How were the initial conditions for the CO2 simulation on Jan 1, 2009, obtained? Given the long residence time for air in the stratosphere, was there a long spin up to produce a reliable distribution of CO2 in the stratosphere? The flux inversions will not significantly correct the stratospheric CO2 distribution because of the long timescale for transport of air in the stratosphere.

2. Page 6753, lines 11-13: This study extends the evaluation of Belikov et al. (2013), but the model configuration in this manuscript is different from that used by Belikov et al. (2013). For example, Belikov et al. (2013) used a combination of the EDGAR and CDIAC inventories for anthropogenic emissions and based their non-fossil fuel fluxes on result from a previous inversion analysis that used surface CO2 data. It would be helpful to have more information about the Level 4A monthly mean flux estimates that are used here.

3. Page 6753, last line: More information is needed here on how the HIPPO and model profiles were converted to XCO2, ensuring consistency in the dry air mass between the two datasets.

4. Page 6756, lines 19-20: The large increase in the potential temperature gradient with height reflects the transition to the more stable stratosphere in crossing the tropopause. Based on Figures 3 - 5, it looks as though the bias is largest when the CO2 vertical gradient across the tropopause is large. The model seems to be generally incapable

of reproducing the strong vertical gradients in CO2 observed by HIPPO, with Fig 4e being the exception. It would be interesting to examine the meteorological conditions for Fig 4e more closely.

5. Page 6757, lines 16-18: The authors should provide a reference to support the claim that radiative heating rates are more accurate in the stratosphere. Does this apply to the lower stratosphere (such as between the tropopause and the 350 K level), where the heating rates are small?

6. Page 6757, lines 21-24: Although satellite observations at high latitude in winter are limited, there is no reason to assume that the bias will not be a problem for an inversion analysis. The system is dynamic. One would expect the large-scale motions in the atmosphere to transport this biased signal to lower latitudes, where it could contribute to a mismatch between the model and observations in the context of a flux inversion.

7. Page 6758, lines 8-11: I don't understand the sentence starting with "The smaller bias…" How are the fluxes contributing to the differences in the vertical gradient in January compared to spring? Also, the fluxes are top-down estimates based on XCO2 data. How are these fluxes simplified? More discussion is needed here.

8. Page 6758, line 12-13: What is the evidence that the accuracy in the lower stratosphere should improve with mass-balanced reanalysis data? Previously, on page 6751, line 9, it was mentioned that the model uses a horizontal flux-correction to ensure mass balance, so the model is already conserving mass. How sensitive is the CO2 distribution in the lower stratosphere to this mass correction? Even with the use of mass-balanced reanalysis data, the model will likely require a mass-fixer (similar to that used in most chemical transport models) to adjust the discrepancies in the atmospheric mass due to the mismatch in the model time step and the frequency at which the reanalysis data are ingested.

9. Last sentence of conclusions: I agree with the statement, but it is unclear to me how this last sentence about the need for high-resolution CO2 fields is connected to the rest

of the manuscript.

Technical comments

1. Page 6749, line 20: The wording "diverging distribution" of chemical species is unclear.

2. Page 6750, line 3: But the tropical atmosphere is not in geostrophic balance. This is one of the reasons that meteorological data assimilation is such a challenge in the tropics. The statement here gives the impression that one should expect geostrophic balance in the tropics.

3. Page 6753, line 17: The Patra et al. (2011) reference is missing.

4. Page 6755, lines 3 and 9: The terminology "stable" and "unstable" is odd in this context. The greater RMSE at the higher latitudes does not necessarily mean that the model simulation is "unstable".

5. Page 6756, line 16: "2 ppmv but the LS zone" should be "2 ppmv in the LS zone".

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 6745, 2015.