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Interactive comment on "Simulating CO$_2$ profiles using NIES TM and comparison with HIAPER Pole-to-Pole Observations" *by* C. Song et al.

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ACPD 15, C2900-C2903, 2015 I am very grateful to the anonymous referee #2 and give a response as follows.

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.

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Response: The initial condition was produced with short one year spin-up run starting with realistic two-dimensional field given in latitude-pressure coordinates. As model is known to maintain realistic stratospheric air age (Belikov et al, 2013), a longer spin up was considered unnecessary. There was no spin up in model simulation.

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 CO_2 data. It would be helpful to have more information about the Level 4A monthly mean flux estimates that are used here.

Response: Seasonally varying surface CO_2 flux corrections provided by GOSAT Level 4 product were the only readily available flux data optimized for this transport model version and the set of prior fluxes, so we used this dataset. Further details on the fluxes can be found in (Maksyutov et al., 2013). Page 6753, I added more information about GOSAT Level 4A inverse model correction flux in line 11 as follows. "global prior fluxes of biosphere-atmosphere and air-ocean exchange, fossil fuel emissions, biomass burning, and GOSAT Level 4A inverse model correction (Maksyutov et al., 2013), provided by climatological mean of monthly global CO_2 fluxes estimated with GLOBALVIEW and GOSAT SWIR Level 2 XCO2 data. As we use same set of fluxes and same version of transport model as GOSAT Level 4 product, the flux corrections provided by GOSAT Level 4 product provide optimal fit to available observations."

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.

Response: Through the manuscript, I did not use XCO2, the HIPPO observation and simulation are all CO_2.

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 CO_2 vertical gradient across the tropopause is large. The model seems to be generally incapable of reproducing the strong vertical gradients in CO_2 observed by HIPPO, with Fig 4e being the exception. It would be interesting to examine the meteorological conditions for Fig 4e more closely.

Response: For the original Figure 4e, it occurred at about local time 13:00 on 4th November 2009 nearby King Salmon Airport of Alaska, and the meteorological conditions are: continuous rain, nonfreezing, and weaker at the time of observation. In the corresponding period, cloud obscured half or less of the sky. However, I also checked the meteorological conditions of original Figure 4(f), there was no cloud and rain on that day, so there is no inevitable connection between meteorological condition and good modeling results from tropopause to low stratosphere.

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?

Response: Original text cites two references and add sentence in line 18 after "in stratosphere" as follows. (Weaver et al, 1993, Maksyutov et al., 2013). Extending the isentropic coordinates to mid-troposphere levels such as implemented by Chen and Rasch, (2011), Bleck et al (2015) has potential for reducing the transport bias in this region and season. Add references as follows. Page 6759, line 12, "Bleck, R. , Bao, J.-W., G. Benjamin, S., Brown, J. M., Fiorino, M., Henderson, T. B., Lee, J.-L., Mac-Donald, A. E., Madden, P., Middlecoff, J., Rosinski, J., Smirnova, T. G., Sun, S., and N. Wang, A Vertically Flow-Following Icosahedral Grid Model for Medium-Range and Seasonal Prediction. Part I: Model Description. Mon. Wea. Rev., 143, 2386–2403. doi: http://dx.doi.org/10.1175/MWR-D-14-00300.1, 2015." Page 6759, line 27, "Chen, C.-C.

and Rasch, P. J., Climate Simulations with an Isentropic Finite-Volume Dynamical Core. J. Climate, 25, 2843–2861.doi: http://dx.doi.org/10.1175/2011JCLI4184.1, 2012." Page 6761, line 15, "Maksyutov, S., Takagi, H., Valsala, V. K., Saito, M., Oda, T., Saeki, T., Belikov, D. A., Saito, R., Ito, A., Yoshida, Y., Morino, L., Uchino, O., Andres, R. J., and Yokota, T., Regional CO2 flux estimates for 2009-2010 based on GOSAT and ground-based CO2 observations, Atmos. Chem. Phys., 13, 93519373, doi: 10.5194/acp-13-9351-2013, 2013." Page 6763, line 15, "Weaver, C. J., Douglass, A. R., and Rood, R. B., Thermodynamic balance of three-dimensional stratospheric winds derived from a data assimilation procedure, J. Atmos. Sc., 50, 2987-2993, 1993."

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.

Response: The original text has been changed. Page 6757, line 21: "This lower strato-sphere bias is not to deteriorate the transport model performance in the inverse modeling applications (Maksyutov et al., 2013). However, these biased values probably result in greater errors of a flux inversion with signals being transported into lower latitudes in adverse synoptic patterns."

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.

Response: I understand the original text didn't reflect my unvarnished views. The original text has been changed as follows. Page 6758, Line 8: The smaller bias for HIPPO-1 compared with HIPPO-3 arises from seasonal changes in synoptic patterns from January to March and April, as simulated by Patra et al. (2008).

8. Page 6758, line 12-13: What is the evidence that the accuracy in the lower strato-sphere 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 CO_2 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 atmo-spheric mass due to the mismatch in the model time step and the frequency at which the reanalysis data are ingested.

Response: Original text add sentences in line 13 on page 6758 before "Demand for" as follows. This off-line model with horizontal flux-correction attain mass conservation because vertically integrated mass change is in balance with the surface pressure tendency (Belikov et al., 2011). The computation achieve fast convergence with CO_2 distribution tending towards stability in the whole integral height.

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 CO_2 fields is connected to the rest of the manuscript.

Response: I agree the suggestion of anonymous referee and add sentence at the end of line 17 on page 6762 as follows. Employing HIPPO-1, 2, 3, validation of the NIES model provide basis for applying high-precision satellite product, and so we can get more and better carbon sources/sinks information.

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

Response: Page 6749, line 20: I changed "diverging" to "diverse".

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.

Response: I never suspect quasi-geostrophic theory of middle latitude large scale motion so I have accepted the suggestion by anonymous referee. This statement should be changed. Page 6750, line 2: Bregman et al. (2006) pointed that additional difficulties for detecting model biases are caused by the fact that tropical atmosphere is not in geostrophic balance.

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

Response: I have added the reference after line 13 on page 6762 as follows. "Patra, P. K., Houweling, S., Krol, M., Bousquet, P., Belikov, D., Bergmann, D., Bian, H., Cameron-Smith, P., Chipperfield, M. P., Corbin, K., Fortems-Cheiney, A., Fraser, A., Gloor, E., Hess, P., Ito, A., Kawa, S. R., Law, R. M., Loh, Z., Maksyutov, S., Meng, L., Palmer, P. I., Prinn, R. G., Rigby, M., Saito, R., and Wilson, C.: Transcom model simulations of CH4 and related species: linking transport, surface flux and chemical loss with CH4 variability in the troposphere and lower stratosphere, Atmos. Chem. Phys., 11-12813-12837, doi: 10.5194/acp-11-12813-2011, 2011.".

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".

Response: I have changed "stable" and "unstable" in the manuscript. RMSE means the dispersion of the data. Page 6754, line 8, I change "the model values were stable" to "the model values' dispersion was small". Page 6755, line 2-3, I change "not stable" to "disperse". Page 6755, line 9, I change "unstable" to "disperse".

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

Response: Page 6756, line 16: "but" here means "except for" or "apart from", I changed it to "apart from".

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C4275/2015/acpd-15-C4275-2015-supplement.pdf

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