Reply to Dr. R. Law (referee #1)

General comments

Saeki et al. describe NIES transport model (TM) simulations of carbon dioxide and methane. The simulations are at higher resolution (0.5 x 0.5 degree) than previous simulations using the NIES TM and use near real-time, GPV, meteorological forcing. A new nudging scheme is introduced in the stratosphere to improve upper level simulations. The authors argue that higher resolution and near real-time forcing is needed to provide better initial conditions for satellite retrievals of column CO2 and column CH4.

The abstract concludes that the new model set-up is 'adequate for use in satellite retrievals'. This is probably the case but I wonder if a stronger conclusion could have been reached if the analysis of the simulations had been more targeted? For example, apart from one illustrative figure, the analysis does not demonstrate whether the higher spatial resolution improves the model performance. Likewise, we do not learn whether the move to near real-time meteorological forcing degrades the model performance.

I have some concerns with the current analysis choices, which I detail below for the areas (Sec 3.1, 3.2, 3.3) that I am most familiar with.

Overall, I do not believe this paper is suitable for publication in its present form. As currently presented, the material seems to be more appropriate to a technical report.

We thank you for your careful review of our manuscript. The reviewer's general concerns (analysis choice, real-time GPV forcing etc.) are repeated in the specific comments, so we answer them in the specific comment section below. Please find below responses to your comments and the changes made to the manuscript. The reviewer's comments are shown in bold, followed by our response.

Specific comments

Section 3.1 and 3.2: In these sections the model output is compared with the GLOB- ALVIEW datasets for CO2 and CH4, firstly for the 2008 annual mean and secondly for the seasonal cycle. I do not believe the analysis adequately addresses the fact that the GLOBALVIEW dataset is a data product; CO2 and CH4 observations are selected and fitted to give pseudo-weekly concentrations with gap-filled values also available. I assume that since the comparison with GLOBALVIEW has been made for 260 and 206 sites respectively for CO2 and CH4, gap-filled data is being used, i.e. the comparison of the 2008 annual mean includes sites that were not actually taking measurements in 2008. I think it would be preferable to do the analysis only for sites that are active in 2008; at least some of the sites that are noted as giving large errors (SCS, ITN, DAA) are those that are reliant on gap-filled data.

Reply: We agree with your suggestion. We reanalyzed using 2008-active sites only for comparisons in sections 3.1, 3.2. The number of sites for CO_2 and CH_4 are 155 and 123, respectively (p. 2225, Line 6). Table 1, Figs. 3, 4, and 5 were replaced to show results of the 2008-active-site comparisons. The numbers of statistics in Section 3.1 and 3.2 were changed to the new statistics.

While there has been some attempt to select the model output by using afternoon data only, this selection will not be comparable to the GLOBALVIEW selection for all sites; mountain sites often use night-time data to avoid upslope flow, while coastal sites are usually selected to sample marine air only. The model error noted for LJO (p2225) is most likely due to a more rigorous marine-only selection of the LJO observations sub- mitted to GLOBALVIEW compared to the model data selection. If this was the explanation intended in the text (p2225, line 19-26), it was not clear to me. With appropriate data selection, I would expect a global scale transport model to be able to simulate LJO GLOBALVIEW data. TAP and CRI

(p2228) are other sites where I suspect that appropriate baseline selection is critical.

Reply: For mountain sites, mountain top topography such as MLO is not precisely reproduced in model resolution of 0.5°. We think the model cannot reproduce diurnal cycles like upslope flow at mountain sites, thus we believe monthly averages are not affected by day-time averages or night-time averages.

For LJO, we did not perform simulated data selection, so we added marine-only selection of the observation as one of possible causes of model-data mismatch (Underline). "climatological CO_2 fluxes" was also added according to suggestion from the reviewer 2.

p.2225: This might be caused by marine-only selection of LJO observations, climatological CO_2 fluxes and the high variability observed near source regions, which is not expected to be captured by global scale transport models.

The annual mean at tower sites are noted (p2226, line 9) for being overestimated by the model. How sensitive is the analysis to the vertical model level at which the simulation is sampled? What is the vertical resolution near the surface? Another problem for these continental sites may be that the flux climatology used for CO2 has no diurnal cycle for the biosphere component. This should be stated more explicitly when discussing these results. I suspect this could account for much of the mismatch between model and observations in summer; afternoon selected data would be for the period of maximum uptake for diurnally varying biospheric fluxes, while the model would have less uptake from using daily mean fluxes.

Reply: We didn't perform sensitivity test for vertical sampling. The model result for a tower site is sampled at the tower elevation above the ground and then interpolated vertically if the tower sampling height above the ground is above the lowest model level (0.996 sigma).

The model sigma levels near the surface is below:

1.000, 0.996, 0.988, 0.978, 0.968, ...

The vertical levels in the model were added as Appendix A.4.

As for the climatological flux, first we apologize our miswriting "monthly terrestrial biosphere flux" (p.2221, line 20). We use "hourly terrestrial biosphere flux" actually. We corrected the sentence to "hourly".

We added a sentence below to make it clear that we used the climatological flux in the model.

p. 2226, line 13: due to their location in areas affected by biogenic CO_2, such as forest or inland plain, where it is difficult to fully represent by the hourly climatological biogenic flux used in the model.

In section 3.2, seasonal cycles from the model for a single year (2008) are compared with mean seasonal cycles from the full GLOBALVIEW period (1979-2008). Wouldn't it be better to make the comparison with the same year?

Reply: We reanalyzed using 2008-active sites only for comparisons in sections 3.1, 3.2.

The number of sites for CO_2 and CH_4 were changed to 155 and 123, respectively. (p. 2225, Line 6)

Table 1, Figs. 3, 4, and 5 were replaced to results of the 2008-active-site comparisons.

The observed seasonality at oceanic sites is described as 'reproduced fairly well' (p2227, line 11). However, I think this is difficult to establish for the southern hemi- sphere, at least based on Figure 4. Seasonal cycles in the southern hemisphere for CO2 are very small, so even a small Model-GV difference could imply a modelled sea- sonal cycle with incorrect amplitude or phase. This certainly seems to be the case for SPO, as shown in Figure 6, where the modelled seasonality for CO2 does not seem to be a good match to the observed seasonality.

Reply: We agree with your comment that CO_2 variation in southern-hemispheric sites are difficult to reproduce in the current model. However, because our goal is to provide a priori concentrations to real-time satellite retrievals and the GOSAT retrieval precision are 3.5 ppm and 15 ppb for XCO_2 and XCH_4 (p. 2218, line 4), the model-data mismatches in southern hemispheric sites are relatively smaller than these GOSAT precision, and also this is the limitation of the current model.

I find the summary statistics provided in Table 1 and Figure 5 and described on p2228 to be hard to assess without any reference to compare them against. This section would be much stronger if equivalent statistics could be provided for the lower resolution version of the model. Then the reader would get a sense of whether the higher resolution model improved the simulation (or whether the simulation is mostly limited by the input flux climatology).

Reply: We added lower-resolution model results to Fig. 5. The model performance is not improved from a lower-resolution model for monthly seasonal variation, thus we think the simulated monthly seasonal variations are mostly limited by the climatological fluxes, as you suggested.

We added a sentence below.

p. 2229, Line 4: The performance of the higher-resolution model is not improved from the lower-resolution model, thus the simulated seasonal variations are mostly limited by the climatological fluxes.

Section 3.3. is titled synoptic variations but the choice of sites, such as SPO and MLO, does not facilitate this comparison. The statistics calculated in Table 2 will be dominated by the trend and seasonal cycle, and consequently the analysis in this section currently adds little to that provided in Sec 3.1 and 3.2. It would seem to me that a better way to focus on synoptic variations is to detrend and deseasonalise the model output and observations before making the comparison and calculating

the statistics. Focussing on HAT alone would be reasonable, or a second site with significant synoptic variations could be chosen. I understand that the early part of the simulation does not use analysed winds that correspond to that time period, but this could be seen as an opportunity. A comparison of statistics for HAT (e.g correlation) between the period with the correct meteorology and the period with incorrect meteorology should show a clear difference in correlation for synoptic timescales. Calculating statistics on a seasonal basis might also be useful in identifying where poorer correlations might be due to the flux climatology or to transport error.

Reply: We added a new plot for CO_2 at Cape Ochi-ishi site (COI) in Figure 6. Also a new plot to show synoptic variations at HAT and COI was added as Figure 7 with a lower-resolution model results, and statistics to show model performances by a higher-resolution model and a lower-resolution model were listed in a new table, Table 3. The higher-resolution model results correlate better with the observations than the lower-resolution model results.

We added sentences below.

p. 2230, line 25: COI is located in the eastern part of Hokkaido, Japan. The monitoring station fronts onto the northwest Pacific Ocean and is influenced generally by northwesterly winds in winter and southwesterly winds in summer (Tohjima et al., 2002). Reflecting seasonal variations of seasonally-varying air mass from Japan and East Asia, CO_2 concentration at COI shows larger seasonal variation than that at HAT. The model captures overall features of CO_2 trend and seasonal variations at COI.

p. 2231, line 4: Figure 7 illustrates synoptic-scale variations of CO_2 and CH_4 at HAT and CO_2 at COI. Synoptic variations were deseasonalized and detrended variations which were extracted from the observed and simulated time series (Fig. 6) by using a digital filter technique (Nakazawa et al., 1997). The synoptic variation in CO_2 at HAT is larger in summer than in winter due to air mass from East Asia. CH_4 at HAT observed numerous peaks throughout the year, which reaches at about 150 ppb. CO_2 at COI has large synoptic variability in summer time. Table 3 lists statistics of the model performances of 0.5° and 2.0° simulations against the observed synoptic variations. The ratio of standard deviations range from 0.73 to 1.10, and the difference between 0.5° and 2.0° simulations are small, while the correlation coefficients for 0.5° simulation show better performance than those for 2.0° simulation. The high-resolution model correlates well with the observations.

This section could also provide an opportunity to compare simulations with GPV vs ECMWF or NCEP meteorological forcing. It would be nice to see that the model performance isn't significantly degraded by moving to near real-time meteorological forcing.

Reply: Our goal is providing a CO_2 and CH_4 simulation in near real time under constraints of operational environment with requirements of guarantied timely delivery of the simulation results. This aim may not be fully explained in the manuscript. Thus we added following corrections to the text to make our goal clearer.

p. 2216, line 6 (abstract): for <u>operational near-real time retrieval</u> of XCO_2 and XCH_4 ....

p. 2218, line 12: <u>For various applications of the retrieved data such as observations of the strong CO_2 emissions by forest fire or volcano eruptions, users desire a near real-time data processing. To serve those needs, an operational retrieval in GOSAT data processing system is conducted at near real time.</u>

Near real-time meteorology data by operational analysis and forecast are generally as accurate as the reanalysis products, but due to data feed limitations the GPV data are provided at lower resolution (0.5° resolution, 21 pressure levels) than the original JMA forecast model (TL959L60, Mizuta et al, 2006). Thus, some performance degradation of vertical transport is expected, as compared to use of higher vertical resolution data from JCDAS or ECMWF reanalysis.

We did not made comparisons with higher resolution data with reanalysis products on the operational base because it was not feasible to use re-analysis in the near real-time simulations. Belikov et al. (2011) made comparisons between NCEP/GFS data and GPV data on NIES TM for 2008 (Section 3.2.4, Figs. 11 and 12 in the paper), and found

GPV-forced NIES TM is compatible with or better than GFS-NIES TM. Thus we think no serious degradation might occur in real-time operation of GPV-NIES TM.

Mizuta, R., K. Oouchi, H. Yoshimura, A. Noda, K. Katayama, S. Yukimoto, M. Hosaka, S. Kusunoki, H. Kawai, and M. Nakagawa (2006), 20-km-Mesh Global Climate Simulations Using JMA-GSM Model —Mean Climate States—, *Journal of the Meteorological Society of Japan*, *84*(1), 165–185, doi:10.2151/jmsj.84.165. [online] Available from: http://joi.jlc.jst.go.jp/JST.JSTAGE/jmsj/84.165?from=CrossRef

Belikov, D., S. Maksyutov, T. Miyasaka, T. Saeki, R. Zhuravlev, and B. Kiryushov (2011), Mass-conserving tracer transport modelling on a reduced latitude-longitude grid with NIES-TM, *Geoscientific Model Development*, *4*(1), 207–222, doi:10.5194/gmd-4-207-2011. [online] Available from: http://www.geosci-model-dev.net/4/207/2011/ (Accessed 14 November 2012)

Technical corrections

p2217, line 10: Suggest re-write as ’In this TransCom continuous experiment, 25 trans- port models participated with two running at 0.5x0.5 degree resolution and the others running at ....’

Reply: We rewrote the sentence as you suggested.

p2217, line 17: Suggest re-write as ’However most model simulations of these green- house gases are still carried out at ...’

Reply: The sentence was rewritten.

P2217, Line 18: Allen et al is 2011 in text but 2012 in references

Reply: “2012” is correct. The main text was changed to “2012”.

p2219, line 29: the GLOBALVIEW datasets should be cited here.

Reply: GLOBALVIEW-CO_2 and CH_4 are cited here.

p2221, line 1: missing open bracket (

Reply: “(“ was added.

p2221, line 2: replace ’by’ with ’at’ 0.5x0.5

Reply: Replaced.

p2221, line 9: delete ‘of’

Reply: Deleted.

p2221, line 9-10: presumably the boundary layer height is not available from GPV – do you have any idea as to the consequence of mixing two data sources for the transport model forcing?

Reply: JMA meteorology data (GPV) did not provide PBL height, so we decided to use and rely upon PBL height from ECMWF dataset, one of the most reliable dataset, rather than deriving diagnostic PBL height from GPV dataset. In the TransCom continuous experiment (Law et al., 2008; Patra et al., 2008), NIES-TM with NCEP metrology and ECVWF PBL height was used at resolution of 1.0°, and the model performance was comparable to the other participants. Thus we think inconsistency might be within an error of PBL height.

p2221, line 14: in different places the 21 GPV pressure levels are mentioned or the model sigma levels. It would be helpful for readers to be clear about when you had to interpolate between different vertical coordinates. (also p2223, line 24)

Reply: We added a sentence below to explain the relationship between GPV-levels and the model sigma levels.

p. 2221, line 16: The 21-level GPV data is interpolated to the model sigma levels just after reading GPV data every time step, and the model results are outputted every 3-hour time after interpolating 21 pressure levels.

p2222, line 23: is 2015 really meant here? if so, explain how you deal with future concentrations/IAV

Reply: Yes, we prepare stratospheric CO_2 climatology up to 2015 when GOSAT official mission will be completed. We added a sentence below to explain how to extent the CO_2 climatology. The detailed method is described in Saito et al. (2011)

p. 2222, line 23: Extension of CO_2 climatology has been done by using average trend plus average seasonal cycle obtained from fitting GV-CO_2 data (Masarie and Tans, 1995) and by using stratospheric age of air (Saito et al., 2011).

Saito, R., S. Houweling, P. K. Patra, D. Belikov, R. Lokupitiya, Y. Niwa, F. Chevallier, T. Saeki, and S. Maksyutov (2011), TransCom satellite intercomparison experiment: Construction of a bias corrected atmospheric CO 2 climatology, *Journal of Geophysical Research*, *116*(D21), 1–13, doi:10.1029/2011JD016033. [online] Available from: http://www.agu.org/pubs/crossref/2011/2011JD016033.shtml (Accessed 17 July 2012)

p2223, line 7-8: I would suggest putting the CO2 correction with the CO2 description before CH4 is discussed.

Reply: the CO_2 sentence was moved before the CH_4 description as you suggested.

p2223, line 13: delete reference to CO2 fossil emissions here since it is the CH4 trend being discussed?

Reply: That was our mistake. We deleted "CO_2 fossil fuel emission".

p2224, line 18: suggest 'close to zero, indicating ..', to replace 'nearly the same zero, indicates'

Reply: We replaced as you recommended.

p2225, line 6: replace 'opened' with 'available'

Reply: We replaced "opened" with "available".

p2225, line 28: Peerez-Landa spelt with only 1 e in 2007a reference – but I'm not sure how relevant these references are

Reply: "Perez" (1 e) is correct. The text was corrected.

Sec 3.1: you might want to add a general comment that you might expect CO2 to fit the GLOBALVIEW dataset better than CH4 since the flux climatology used for CO2 included flux corrections from an inversion (which aimed to fit the data) whereas the CH4 flux climatology is an inventory only

Reply: Thank you for your suggestion. We add a sentence below after p. 2225, lline 17.

"In general, CO_2 might be expected to reproduce the GV data better than CH_4 because the CO_2 climatological flux data set includes flux correction by an inversion while CH_4 does not."

p2227, line 6: it is not actually amplitude being plotted so perhaps should use 'seasonal cycles' instead

Reply: Yes, as you suppose, we meant "peak-to-peak amplitude". To avoid misunderstanding, "seasonal amplitudes" was replaced to "seasonal cycles".

p2228, line 5: replace 'some' with 'about' – maybe note that for oceanic sites biases increase as move north

Reply: "some" was changed "about".

We add a sentence below to mention increasing biases to north.

p. 2228, line 7: The seasonal biases for oceanic sites increase as move north-poleward with a maximum bias about 30 ppb.

p2229, line 2: suggest 'may affect the model's ability to reproduce the CO2 concentra- tion ...'

Reply: We changed the sentence as you suggested.

p2229, line 10: the MLO and SPO WDCGG datasets should be listed in the references

Reply: They were referred as "Dlugokencky 2012a, 2012b; Thoning 2012a, 2012b" and the references were added.

p2231, line 14-18: I don't really follow why observations are averaged while model is a single profile and why two different locations?

Reply: The model stratosphere is nudged to climatological CO_2 as described in Section 2, so we compared the model profile to the averaged CO_2 profile from multiple observations from two sites. There is no other observational site in the stratosphere over Japan except for these two sites, Sanriku and Taiki-cho. In the stratosphere, their differences in locations are negligible.

p2232, line 24: suggest 'all at 13:00'

Reply: "all" was added.

p2233, line 1: Europe and NE America look at least as large as Siberia and Asia

Reply: "Europe and northeastern America" was added to the sentence.

p2234, line 4: the description of fig 10 seems disconnected with the rest of the paper though it is a nice illustration of the impact of higher resolution

Reply: To demonstrate the difference between a higher-resolution model and a lower-resolution model (2°), we added lower-resolution model results in Figs. 5, 6, 7 (newly added), and Table 3 (newly added). So we hope Fig. 10 results is closely related to the rest of the paper.

p2234, line 18: start a new paragraph to discuss CH4

Reply: We started a new paragraph as you recommended.

p2234, line 23: delete 'in equator'

Reply: "in equator" was deleted.

p2235, line 1-2: If the polar vortex is not centred over the pole, would this contribute to what you are seeing?

Reply: Seasonal variation of the vortex affects X CH_4 variation. Generally Antarctic polar vortex is centered over the pole, but if not, the vortex changes the tropopause height and has an influence on XCH_4 values.

p2236, line 1: replace 'correction' with 'correlation'

Reply: Replaced.

p2236, line 9-10: 'resolve' not 'resolves', insert 'better' before 'than the 2.0o model does'. I am not sure that the one example given is enough evidence to make this claim.

Reply: Changed as you suggested.

We added a new plot for CO_2 at Cape Ochi-ishi site (COI) in Figure 6 and a new plot to show synoptic variations at HAT and COI as Figure 7, and Table 3 to show model performances by a higher-resolution model and a coarser-resolution model were listed in a new table, Table 3. We hope these analyses support the advantage of the higher-resolution model.

p2236, line 15: the evidence for the model performance for synoptic variations is weak based on the analysis in this paper

Reply: Please see our reply just above (reply for your comment on p2236, line 9-10).