Reply to referee #2

General Comments

This paper describes simulation results by NIES-TM high resolution simulation of CO2 and CH4. Saeki et al., insist that the higher resolution of NIES-TM shows good perfor- mance for adopt a priori concentrations for use in satellite data retrievals. The experi- mental setting for meteorology seems to be reasonable (but I consider a combination of JMA meteorology and ECMWF PBL height is one issue). The authors should show more detailed transport processes (mass fixer, cumulus convection and so on). One major issue is that monthly mean CO2 and CH4 flux may be different from actual flux and I have some doubts about the validity of the model validation especially inland sites. The merit of higher resolution NIES-TM is that the model could treat more realistic meteorological data and I think the authors should put more emphasis on this point. I consider this paper should be acceptable after some revisions described below.

We thank you for your careful review of our manuscript. The reviewer's general concerns are repeated in the specific comments, so we answer them in the specific comment section below. The reviewer's comments are shown in bold.

Specific comments

Page 2220, line 7: The authors should show some description for a mass fixer, cumulus convection scheme and turbulent diffusion processes.

Reply: The detail of the model is described in Maksyutov et al. (2008). We added brief descriptions of the mass fixer, the cumulus convection scheme, and the turbulent diffusion process in the model in "Appendix A.1 – A.3".

Page 2220, line 15: The height of the lowest model level may affect the CO2 and CH4 concentrations of this level. The authors should show it (all model levels are preferable).

Reply: The lowest model level is 1.000 ~ 0.996 sigma. We added "Appendix A.4" to show the all model vertical sigma levels.

The model top level is 0.020 sigma, not 0.010 sigma in the original manuscript. We corrected it (p. 2221, line 15).

Page 2221, line 9: As I mentioned in general comments, the combination of JMA analyzed meteorology and ECMWF PBL height is inconsistent. I consider it better to diagnose PBL height from operational meteorology. Could you show that there is no significant effect of this inconsistency?

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.

Page 2221, line 20: One of the most uncertain fluxes is monthly terrestrial biosphere flux. The authors could compare with different climatology flux.

Reply: First we apologize our miswriting p. 2221, line 20 "monthly terrestrial biosphere flux". We use "hourly terrestrial biosphere flux" actually. We corrected the sentence to "hourly".

We agree that the most uncertain flux is a terrestrial biosphere flux. The hourly CASA flux had been used and examined in the TransCom continuous experiment. Recently it was used in Comprehensive Observation Network for Trace gases by Airliner transport model intercomparison (CONTRAIL-TMI) (Niwa et al., 2011) with TransCom-derived monthly inverted flux. Thus we believe the hourly CASA flux is a basic terrestrial biosphere flux suitable for being used as climatology.

Niwa, Y. et al. (2011), Three-dimensional variations of atmospheric CO_2: aircraft measurements and multi-transport model simulations, *Atmospheric Chemistry and Physics*, *11*(24), 13359–13375, doi:10.5194/acp-11-13359-2011. [online] Available from: http://www.atmos-chem-phys.net/11/13359/2011/ (Accessed 13 July 2012)

Page 2221, line 23: The annual global net flux of CO2 determines annual increase of CO2. This may affect annual mean biases. The authors should show each annual net flux of CO2. In CH4 case, such information is available for readers even if there are some chemical losses.

Reply: The annual net flux of CO_2 is 4.32 GtC (fossil + biospheric + ocean + inversion results), and 575 Tg for CH_4. We added sentences below (underlines).

Line 23: <u>The annual total net flux of CO_2 to the atmosphere is 4.32 GtC.</u>

Line 24: For CH_4, we used the monthly varying flux for 2000 <u>(575 Tg/yr)</u>,

Page 2225, line 5: As mentioned by the first reviewer, GV data are not raw observation data. If the authors try to compare better condition, they should use monthly mean of raw observation data.

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.

Page 2225, line 25: The problem may be CO2 flux climatology, not model resolution or meteorological field.

Reply: We added CO_2 flux climatology as one of possible reasons as below (underline).

Line 25: This might be caused by climatological CO_2 fluxes and the high variability observed near source regions,...

Page 2229, line 6: The authors should add some inland sites to show model performance precisely. The results may be worse, but the authors could show some merit of high resolution model (synoptic scale variability).

Reply: We added a new plot for CO_2 at Cape Ochi-ishi site (COI) in Figure 6, and overplotted lower-resolution model results in plots for Hateruma (HAT) and COI sites. A new plate to show synoptic variations at HAT and COI was inserted as Figure 7, and statistics to show model performances by a higher-resolution model and a lower-resolution model were listed in a new table, Table 3. From these statistics, we can know the higher-resolution shows better performance than the lower-resolution model.

Page 2230, line 7: The authors should mention the reason of this slightly large mismatch at SPO?

Reply: We think that the reason of this mismatch at SPO may be a combination of the climatology flux and model transport error. We added a sentence (underline).

Line 7: Differences between the model and the observed small variations in the first half of 2010 are slightly large at 2 ppm, which might be due to climatological CO_2 fluxes and model transport error.

Page 2233, line 12: The authors should mention an effect of PBL height.

Reply: We agree and mentioned about it in the text.

Line 12: due to strong sources/sinks, low tropopause height, and PBL height.

Page 2234, line 4: If the authors maintain lower horizontal resolutions' results, it could be nice to compare these results in section 3.3.

Reply: As we answered your comments for Page 2229, line 6, we added lower-resolution model results for Hateruma site (HAT) and also added a new plot for CO_2 at Cape Ochi-ishi site (COI) in Figure 6. A new plot to show synoptic variations at HAT and COI was added as Figure 7, and statistics to show model performances by a higher-resolution model and a lower-resolution model were listed in a new table, Table 3.

Page 2235, line 4: In conclusions, little mention of the advantages of a higher-resolution model. The authors should mention them.

Reply: We added a new sentence below to mention about a higher-resolution model.

p. 2236, L5: The synoptic variations at HAT and COI were reproduced better by the higher-resolution model (0.5°) than the lower-resolution model (2.0°), that is, the correlation coefficients between the observation and the higher-resolution model were significantly higher than those for the lower-resolution model.

Technical correctionsPage 2234, line 7: Replace 9 to 10.

Reply: We changed "Fig. 9a" and "Fig. 9b" to "10a" and "10b", respectively.